1.J.60.

No. ~~1.J.14~~

BOSTON

MEDICAL LIBRARY,

19 BOYLSTON PLACE.

Digitized by the Internet Archive
in 2025

https://archive.org/details/woodsmedicxalsur0005vari

WOOD'S

Medical and Surgical MONOGRAPHS

Consisting of Original Treatises and of Complete Reproductions, in English, of Books and Monographs selected from the latest literature of foreign countries, with all illustrations, etc.

VOLUME V.

NEW YORK
WILLIAM WOOD AND COMPANY
56 AND 58 LAFAYETTE PLACE
1890

1. G. 14

Entered at the Post Office at New York, as second-class matter.

Copyright by Wm. Wood & Co., 1889.

ELECTROTYPED AND PRINTED BY
THE PUBLISHERS' PRINTING COMPANY
30 & 32 WEST 13TH STREET
NEW YORK

CONTENTS OF VOL. V.

NEURALGIA; ITS ETIOLOGY, DIAGNOSIS, AND TREATMENT.

	PAGE
Etiology,	4
Symptoms,	6
Varieties Depending on Situation,	18
Neuralgia of the Fifth Nerve,	18
Ocular Neuralgia,	19
Cervico-Occipital Neuralgia,	21
Cervico-Brachial and Brachial Neuralgia,	22
Trunk Neuralgia,	23
Neuralgia of the Leg,	25
Varieties Depending on Character,	26
Epileptiform Neuralgia,	26
Reflex or Sympathetic Neuralgias,	26
Traumatic Neuralgias,	27
Occupation Neuralgias,	29
Herpetic Neuralgias,	29
Hysterical Neuralgias,	30
Rheumatic Neuralgias,	31
Gouty Neuralgia,	31
Diabetic Neuralgia,	31
Anæmic Neuralgia,	31
Malarial Neuralgias,	32
Syphilitic Neuralgia,	33
Degenerative Neuralgia,	33
Diagnosis,	33
Prognosis,	39
Treatment,	40
Visceral Neuralgias,	51
Special Forms,	53

THE PROGNOSIS OF DISEASES OF THE HEART.

PAGE

Non-organic Lesions, Diseases of the Heart Muscle, and Valvular Lesions with Reference to Prognosis, ... 59

THE SPUTUM: A CONTRIBUTION TO CLINICAL DIAGNOSIS AND PRACTICAL EXAMINATION FOR TUBERCLE BACILLI.

CHAPTER I.

Definition and Diagnostic Significance of the Sputum, . . . 97

CHAPTER II.

General Technique of the Examination of the Sputum, . . . 102

CHAPTER III.

The Constituents of Sputum, ... 104

CHAPTER IV.

Chemistry of the Sputum, ... 129

CHAPTER V.

The Classification of the Sputum, ... 132

CHAPTER VI.

The Sputum in Special Diseases of the Respiratory Apparatus, 137

CHAPTER VII.

The Disinfection of the Sputum, ... 148

CHAPTER VIII.

Practical Methods for the Examination of Sputum for Tubercle Bacilli, ... 151

HYPNOTISM: ITS SIGNIFICANCE AND MANAGEMENT BRIEFLY PRESENTED.

CHAPTER I.

General Remarks, ... 161

CHAPTER II.

Suggestion, ... 172

CHAPTER III.

PAGE
Consciousness and Suggestion, ... 208

CHAPTER IV.

Hints for Hypnotic Practice, ... 213

CHAPTER V.

Illustrations of Cures by Suggestion, ... 220

CHAPTER VI.

Judicial Significance of Suggestion, ... 223

THE FORMS OF NASAL OBSTRUCTION IN RELATION TO THROAT AND EAR DISEASES.

CHAPTER I.

Classification of Diseases; Examination; Symptoms, . . 239

CHAPTER II.

Pathological Anatomy of Bone Inflammation; Hypertrophic Rhinitis; Atrophic Rhinitic; Treatment; Polypi, . . 260

CHAPTER III.

Post-Nasal Growths; Ear Symptoms, etc.; Pharyngitis sicca; Examination; Etiology; Treatment, ... 283

THE FORMATION AND EXCRETION OF URIC ACID, AS ELUCIDATING ITS ACTION IN THE CAUSATION OF DISEASE.

CHAPTER I.

Uric Acid in Relation to Migraine, ... 305

CHAPTER II.

Uric Acid in Relation to Various Morbid Conditions, . . 329

THE INITIAL STAGES OF CONSUMPTION: THE NATURE AND TREATMENT, INCLUDING DIETETIC SUGGESTIONS.

CHAPTER I.

PAGE

The Object of the Treatise, ... 367

CHAPTER II.

The Tubercle Bacillus the most Dangerous of Several Irritants that Cause Tuberculization, ... 378

CHAPTER III.

Domiciliation, Localization, and Dissemination of Bacilli, . 389

CHAPTER IV.

Bacillary Tuberculosis Considered in Detail, ... 402

CHAPTER V.

Answers to Questions Suggested in Chapter I., ... 414

CHAPTER VI.

The Object of Correct Pathology and Diagnosis, ... 427

CHAPTER VII.

Diet and Regimen, ... 441

CHAPTER VIII.

Diet Tables, etc., ... 451

ECTOPIC PREGNANCY AND PELVIC HÆMATOCELE.

The Nature, Causes, and Treatment of these Conditions, . 463

THE ARREST OF GROWTH IN CANCER BY THE INTERRUPTED VOLTAIC CURRENT.

Electro-Atrophy, ... 587

THE DREADFUL REVIVAL OF LEPROSY.

The Extension of Leprosy, and Measures to be taken to Arrest its Spread, ... 603

DISEASES OF OLD AGE.

CHAPTER I.

Senile Changes in Different Organs, ... 631

CHAPTER II.

Diseases Affecting the Aged, ... 648

CHAPTER III.

Hygiene of the Aged, ... 657

URINARY NEUROSES OF CHILDHOOD.

CHAPTER I.

Introduction, ... 669
Classification, ... 671
Physical and Chemical Properties of the Urine, ... 676
Spasm of the Bladder, ... 679

CHAPTER II.

Nocturnal Incontinence of Urine, ... 681

CHAPTER III.

Nervous Polyuria of Childhood, ... 700

CHAPTER IV.

Clinical Cases, ... 722

VARICOSE VEINS OF THE LOWER EXTREMITIES.

CHAPTER I.

Relative Number, Extent of the Disease, etc., ... 735

CHAPTER II.

PAGE

Diagnosis of Varicose Veins in the Incipient Stage, . . 750

CHAPTER III.

Management of Cases, ... 762

CHAPTER IV.

Radical Treatment, ... 780

THE USES OF ELECTROLYSIS IN SURGERY.

CHAPTER I.

Electrolysis, ... 795

CHAPTER II.

Aneurism, ... 823

CHAPTER III.

Nævi, ... 830

CHAPTER IV.

Strictures, ... 842

CHAPTER V.

Diseases of the Urinary Organs, ... 856

CHAPTER VI.

Diseases of Women, ... 862

CHAPTER VII.

Miscellaneous, ... 891

VOLUME V. NUMBER 1.

WOOD'S
Medical and Surgical MONOGRAPHS

Consisting of Original Treatises and Reproductions, in English, of Books and Monographs selected from the latest literature of foreign countries, with all illustrations, etc.

CONTENTS

NEURALGIA; ITS ETIOLOGY, DIAGNOSIS, AND TREATMENT. By W. R. GOWERS, M.D., F.R.C.S.

THE PROGNOSIS OF DISEASES OF THE HEART. By Prof. E. LEYDEN, BERLIN, GER.

THE SPUTUM. A CONTRIBUTION TO CLINICAL DIAGNOSIS AND PRACTICAL EXAMINATION FOR TUBERCLE BACILLI. By PETER KAATZER, M.D.

HYPNOTISM. ITS SIGNIFICANCE AND MANAGEMENT BRIEFLY PRESENTED. By Dr. AUGUST FOREL.

THE FORMS OF NASAL OBSTRUCTION, IN RELATION TO THROAT AND EAR DISEASES. By GREVILLE MACDONALD, M.D.

PUBLISHED MONTHLY

PRICE, $10.00 A YEAR SINGLE COPIES, $1.00

JANUARY, 1890

NEW YORK
WILLIAM WOOD AND COMPANY
56 AND 58 LAFAYETTE PLACE
1890

MEDICAL
OCT 5 1898
LIBRARY.

Entered at the Post Office at New York, as second-class matter.
Copyright by Wm. Wood & Co., 1889.

PRESS OF
THE PUBLISHERS' PRINTING COMPANY,
157–159 WILLIAM STREET,
NEW YORK.

CONTENTS.

NEURALGIA; ITS ETIOLOGY, DIAGNOSIS, AND TREATMENT.

	PAGE
Etiology	4
Symptoms	6
Varieties Depending on Situation	18
Neuralgia of the Fifth Nerve	18
Ocular Neuralgia	19
Cervico-Occipital Neuralgia	21
Cervico-Brachial and Brachial Neuralgia	22
Trunk Neuralgia	23
Neuralgia of the Leg	25
Varieties Depending on Character	26
Epileptiform Neuralgia	26
Reflex or Sympathetic Neuralgias	26
Traumatic Neuralgias	27
Occupation Neuralgias	29
Herpetic Neuralgias	29
Hysterical Neuralgias	30
Rheumatic Neuralgias	31
Gouty Neuralgia	31
Diabetic Neuralgia	31
Anæmic Neuralgia	31
Malarial Neuralgias	32
Syphilitic Neuralgia	33
Degenerative Neuralgia	33
Diagnosis	33
Prognosis	39
Treatment	40
Visceral Neuralgias	51
Special Forms	53

THE PROGNOSIS OF DISEASES OF THE HEART.

PAGE

Non-organic Lesions, Diseases of the Heart Muscles, and Valvular Lesions with Reference to Prognosis. 59

THE SPUTUM: A CONTRIBUTION TO CLINICAL DIAGNOSIS AND PRACTICAL EXAMINATION FOR TUBERCLE BACILLI.

CHAPTER I.

Definition and Diagnostic Significance of the Sputum . . 97

CHAPTER II.

General Technique of the Examination of the Sputum . . 102

CHAPTER III.

The Constituents of Sputum 104

CHAPTER IV.

Chemistry of the Sputum. 129

CHAPTER V.

The Classification of the Sputum 132

CHAPTER VI.

The Sputum in Special Diseases of the Respiratory Apparatus. 137

CHAPTER VII.

The Disinfection of the Sputum 148

CHAPTER VIII.

Practical Methods for the Examination of Sputum for Tubercle Bacilli 151

HYPNOTISM: ITS SIGNIFICANCE AND MANAGEMENT BRIEFLY PRESENTED.

CHAPTER I.

General Remarks 161

CHAPTER II.

Suggestion 172

CHAPTER III.

PAGE
Consciousness and Suggestion 208

CHAPTER IV.

Hints for Hypnotic Practice 213

CHAPTER V.

Illustrations of Cures by Suggestion 220

CHAPTER VI.

Judicial Significance of Suggestion 223

THE FORMS OF NASAL OBSTRUCTION IN RELATION TO THROAT AND EAR DISEASES.

CHAPTER I.

Classification of Diseases ; Examination ; Symptoms . . 239

CHAPTER II.

Pathological Anatomy of Bone Inflammation ; Hypertrophic Rhinitis ; Atrophic Rhinitic ; Treatment ; Polypi . . 260

CHAPTER III.

Post Nasal Growths ; Ear Symptoms, etc. ; Pharyngitis sicca ; Examination ; Etiology ; Treatment 283

NEURALGIA;

ITS

Etiology, Diagnosis, and Treatment.

BY

W. R. GOWERS, M.D., F.R.C.S.,

Assistant Professor of Clinical Medicine in University College, London; Physician to University College Hospital, and to the National Hospital for the Paralyzed and Epileptic.

BOSTON MEDICAL
OCT 5 1899
LIBRARY.

5102

NEURALGIA.

THE word "neuralgia" means simply "nerve pain"; such pain may be due to actual disease of a nerve trunk, by which its fibres are irritated, or it may occur without any organic lesion. It is true that the absence of such morbid change cannot often be proved by actual microscopical examination, but the transient character of the pain, and its migration from one part to another, frequently afford strong confirmation of the opinion that, in a large number of the cases of local nerve pain, the symptoms are not due to actual organic change. Two classes of neuralgias have been distinguished—"symptomatic," in which the pain is a symptom of organic disease of the nerves, and "idiopathic," in which the malady, in the first instance, at least, consists only in functional disturbance. Both of these forms have been called "neuralgia" by some writers. By others, however, the term is restricted to the idiopathic class, and this seems to be the only logical course. It is manifestly unreasonable to describe inflammation of nerves as "neuralgia" when it causes much pain and few other symptoms, and as "neuritis" when other symptoms predominate over pain. But the distinction of the two forms of nerve pain is often very difficult in practice. In some forms described as neuralgia, the pain has certainly been generally the result of neuritis. This is the case, for instance, with sciatica, which is generally an inflammation of the nerve. Yet, not only has the common form of sciatica been described as a neuralgia, but its symptoms have been allowed to influence the description of neuralgia in general. In ascertaining the clinical history of neuralgia, it is therefore of great importance that only cases should be used in which a primary

organic lesion of the nerve trunk or centres can be excluded with reasonable confidence. This limitation does not, however, exclude organic lesions of nerve endings. In many cases some structural irritation of the termination of nerves sets up a neuralgia of wide range, and out of all proportion to its cause. Such cases are classed among neuralgias, and are very different from those in which a primary lesion of a nerve trunk causes local and limited pain.

The subject of neuralgia is a very large one, so numerous are the forms of the disease, and so varied its characters. It will be most instructive to consider the general causes and symptoms of the affection, and also its general pathology, before describing its special varieties.

Etiology.—Neuralgia is essentially a disease of adult life. It is rare before puberty, and is not common in extreme old age, although, when the disease does commence late in life, it is often very severe and intractable. Most cases commence between twenty and sixty years. Children enjoy an almost complete immunity from true neuralgia, although very liable to certain headaches which do not come under this category. Women are more prone to neuralgia than men, but the degree of their liability has often been over-estimated, and the excess of females among the sufferers disappears in the second half of life. Moreover, the relative liability of the sexes is not the same in the several varieties. The tendency to neuralgia is often hereditary, although not so frequently as in the cases of migraine. Anstie found evidence of heredity in only one-quarter of his cases. Sometimes the inherited tendency is not special but general, indicated by the occurrence in ancestors or collaterals of epilepsy, insanity, and other neuroses.

The subjects of neuralgia often present a peculiar temperament. They are what is popularly called "nervous"—excitable, often irritable, anxious, worrying over the trifling ills of life, sleeping badly, and are often extremely liable to headaches not distinctly neuralgic in character. In many patients the neuralgic tendency is deeply rooted; they suffer from neuralgia first in one situation and then in another during the course of years. The disease is more frequent in those of weakly constitution than in the robust, but the latter do not enjoy complete immunity. Among the constitutional relations of neuralgia, those to rheumatism and gout are especially im-

portant. The connection with rheumatism is often conspicuous, and is seen in several aspects. Persons who are liable to rheumatism of the fibrous tissues sometimes suffer from pains which have both rheumatic and neuralgic characters—not specially related to the nerves in situation, and yet paroxysmal and unconnected with movement. Such pains are especially frequent in the limbs and back. Women who suffer from rheumatoid arthritis are often also liable to true neuralgias of great severity. A young lady, for instance, suffered for several months from severe paroxysms of pain in one shoulder and the dorsal spine, apparently neuralgic; these ceased, and she was immediately attacked by subacute rheumatoid arthritis. Lastly, both rheumatic affections and neuralgia are certainly sometimes due to gout, and probably, not infrequently, to inherited gout.

Among the exciting causes of neuralgia, among those that are remote, impairment of general health takes the first place. The affection may be excited by any kind of debilitating influence; overwork of mind or body, over-lactation, prolonged fatigue, and anæmia of every degree and causation, are frequently met with as its immediate antecedents. Certain forms of sensory fatigue, as over-stimulation of the eyes, sometimes seem to have a special influence. Severe emotion, or its physical analogue, mechanical concussion, are also occasional exciting causes; the former is often combined with the latter. As examples of the influence of emotion two cases may be mentioned, one of slight, the other of severe, neuralgia, thus induced. A lady was intensely distressed after parting with her husband, who was going to America. She felt on the point of bursting into tears, and as if the tears would give her relief. Her sister said, "Do not cry; you shall not cry." By an effort she succeeded in restraining her tears, but was immediately conscious of a sense of intense pressure above the eyebrows, and a few days later severe supra-orbital neuralgia came on upon the left side, and lasted for several weeks. A girl of eighteen was much startled and alarmed by the unexpected discharge of a gun close beside her. The same evening facial neuralgic pain came on, and continued for five years in most violent paroxysms, sometimes on one side, sometimes on the other. In this connection it may be noted that the fifth nerve is especially related to emotion, both by influencing the

secretion of tears and also as the sensory nerve of the chief region of emotional display—the face.

No single actual excitant of neuralgia is so frequent as exposure to cold, sometimes general, sometimes local and affecting the part in which the neuralgia is felt. Valleix found a history of exposure to cold in one-third of his cases. Cold may not only produce neuralgia, it may also excite paroxysms of pain when the neuralgia is due to some other cause. Another frequent cause is the irritation of nerves, especially near their peripheral distribution. The pain often extends far beyond the area supplied by the irritated nerve. A common example is the widespread pain that may result from the irritation of a carious tooth; the pain may extend into other divisions of the fifth nerve, and even into the region of the cervical plexus. Moreover, the pain may be felt only or chiefly in some other region than that in which it is produced. Thus, I have known severe neuralgia confined to the second division of the fifth to be due to a carious tooth in the lower jaw, and cease entirely when this was extracted. Traumatic lesions of nerves constitute another cause, relatively infrequent, but very important on account of the extreme obstinacy of the pain they cause.

Toxic influences often give rise to neuralgia. The most frequent are alcoholism, lead poisoning and the presence of an excess of sugar in the blood. The influence of some of these causes in producing neuralgia is, however, probably less than has been assumed, because we now know that the nerve pains that are caused by them are sometimes the expression of actual neuritis. This is especially the case with the pains of alcoholism. Nevertheless, when all cases are excluded in which there is reason to believe that neuritis exists, there remain many others, which, from their character, must be regarded as neuralgic. It is intelligible that a condition which has such an influence as to produce actual inflammation of nerves should often irritate them so as to cause neuralgia. This statement is also applicable to gout, the influence of which has been already mentioned. Both neuritis and neuralgia may unquestionably result from acquired and inherited gout. Malaria is a powerful cause of neuralgia, but its influence is seldom seen in this country.

Symptoms Generally.—The great symptoms of neuralgia is pain, spontaneous, paroxysmal, and felt in certain regions

of nerve distribution. It is usually unilateral; when bilateral it is almost always symmetrical in distribution. Usually the pain is constant in seat for a time, it may be for many years. In other cases it changes, now in one part, now in another. Thus, a girl aged fifteen had suffered for two years from paroxysms of intense pain in various parts—arms, legs, back, different parts of the head—and occasionally universal.

The pain is never constant in degree; there are paroxysms, with intervals of complete freedom, or there is a slight, continuous pain, with intense exacerbations. Continuous pain may be merely a dull ache, but it is generally acute and sharp during the paroxysms, and is described as "darting," "stabbing," "boring," "burning," etc.; often the sufferer can find no words to express its exact character. The sharp pain generally has a darting character. A series of sudden sharp pains occur every few minutes; the series of successive pains constitute a paroxysm, and a series of paroxysms an attack. The intervals between the attacks present extreme variation, and are sometimes remarkably long in proportion to the severity of the pain. Thus, one patient will have attacks daily during several years, while in another (as in an actual instance) intervals of many months separate groups of attacks of most intense pain, each group lasting only a few days. In such paroxysms it is usual for the pains to commence suddenly, but they are rarely as severe at first as they subsequently become. Sometimes a peculiar sensation, such as throbbing, heralds each attack of pain. The attacks gradually increase in intensity, and in each attack the separate paroxysms may present a characteristic augmentation and decrease.

The pain is rarely referred to the skin; usually it is more deeply seated, and often corresponds to the position of a nerve trunk and branches. The throbs of pain sometimes, but seldom, coincide with the arterial pulsations. There is often a darting movement of the pain, usually toward the periphery —centrifugal—less commonly from the periphery—centripetal; still less commonly the pain darts alternately in both directions. Sometimes it seems to be localized in a single point, and then has usually a boring character. When most intense the darting pains seem to radiate to other nerve regions adjacent to that in which it is chiefly felt.

The duration of each attack varies according to the num-

ber of paroxysms and their length. Rarely there is a single momentary pain, and it is over for the time. Commonly an attack lasts several minutes, sometimes for hours. When there is freedom from pain, in the intervals there is sometimes a peculiar sensation in the part, not amounting to actual pain. The intervals vary in duration from a few hours to several months. Often an approximate periodicity exists; exact periodicity is met with in malarial cases, in which it may be singularly precise, the pain commencing at the same hour each day. The intervals in such cases rarely exceed four days. Now and then the periodicity is exact in cases that are not malarial. The pain may be worse at the catamenial periods, and I have once known a fifth-nerve neuralgia to occur only at those times. When there is continuous pain in the intervals between the paroxysms, it is moderate in degree, supportable in itself, but often most trying to the patient, as it prevents rest.

The paroxysms and attacks are often induced by certain influences, external or internal, such as by exposure to cold, sometimes by warmth, by movement, posture, or emotion. In some cases there is remarkable sensitiveness to atmospheric influences. When the paroxysms occur at regular intervals an influence that will induce the pain when it is "due," may be powerless immediately after an attack. During a paroxysm, the influences that will bring it on usually intensify the pain. Movement is especially influential in the neuralgias of the fifth nerve; the slightest motion of the jaws may bring on the pain. A touch on the skin may have the same effect; nevertheless, in many cases, although slight pressure increases the pain, firm pressure gives distinct relief, and even when mere contact with the skin causes an exacerbation, rough rubbing may distinctly relieve the suffering. This difference, however, is not always to be observed. Occasionally alcohol, even in small quantities, invariably intensifies or induces the pain; in other cases it gives relief. (Very curious facts are sometimes met with in regard to the induction of the pain. Thus, in one patient, intense fronto-occipital neuralgic pain was excited by every act of defecation. This action, in some cases, has a very peculiar influence on the nervous system, and so also has micturition, as the familiar shiver shows. I have known micturition to be frequently attended with a moment's loss of consciousness.)

This increased sensitiveness of the skin is a very common accompaniment of the pain. It may involve all forms of sensation, although thermic impressions much less commonly occasion pain than does a touch. Sometimes the tactile impression seems to be felt as pain; more often it excites an increase in the true neuralgic pain. The hyperæsthesia, or hyperalgesia, is usually limited to the region in which the spontaneous pain is felt. When this pain follows the course of a nerve, it is commonly most intense at certain spots, and at these places pressure may cause a special increase in the suffering. In the intervals between the paroxysms, these spots may remain tender, and pressure upon them may induce a paroxysm. They are not usually present until the disease has lasted for some time. In recent cases, and when the attack occurs only at long intervals, although there is no persistent tenderness, the pain may be increased during the paroxysm by pressure on certain places. The tender points were first studied by Valleix, and hence are often called after him. They are present in about half the cases; when absent there is sometimes diffuse, ill-defined tenderness in certain areas. When the tender points are well marked and definite, they are tolerably uniform in their position, and for the most part correspond either to the place at which a nerve trunk emerges from a bony canal, passes over a hard structure, or passes through a fascia to become superficial, or to the point of division of a nerve trunk, or to an anastomosis of two nerve trunks. Their exact situation will be mentioned in the description of the several varieties of neuralgia.

There is occasionally tenderness of the vertebral spine corresponding to the origin of the painful nerve, the *point apophysaire* of Trousseau. It is probable, as Anstie pointed out, that the relation of this to neuralgia has been exaggerated. Tenderness of certain vertebral spines is common apart from neuralgia, and there is not always a close correspondence between the position of the spinal tenderness and the seat of the neuralgia. In trigeminal neuralgia, for instance, there may be tenderness of the cervical spines. It is said (by Brenner) that the spots of spinal tenderness, when undiscoverable by pressure, may sometimes be detected by a weak voltaic current, causing, in those positions, distinct pain.

Other sensory disturbances are occasionally observed in

neuralgia. It is said that the onset of the pain is sometimes preceded by numbness, tingling, etc., in the affected area, but it is doubtful whether this is true of simple neuralgia. Occasionally the attack of pain is followed by transient anæsthesia. Persistent diminution of sensibility is only met with in cases of "symptomatic neuralgia," in which there are structural changes in the nerves. Increased sensitiveness to pain (hyperalgesia) in the whole area of the neuralgia is not uncommon. Vomiting is rarely associated with simple neuralgia, although in migraine, the pain which terminates in vomiting is often of a neuralgic character, either localized in the temporal branch of the fifth, or more extensively distributed over the cranium. Now and then paroxysms of true neuralgia end in vomiting, an interesting link of association with migraine. I have met with this in two cases of neuralgia; one, bilateral, in the anterior branches of the cervical plexus, and the other in the two upper divisions of one fifth nerve. Severe attacks in women often cause hysterical symptoms as the pain is subsiding.

Muscular spasm may be excited by the acute paroxysms of pain, evidently in a reflex manner. It is usually confined to the motor nerve related to that which is the seat of the pain, but sometimes spreads to adjacent areas, very rarely passing into a general convulsion. In a case of cranio-spinal neuralgia, each paroxysm was attended by opisthotonos so severe that the patient rested on the head and the heels. The exacerbation of the pain by movement may lead to temporary diminution of mobility, partly voluntary, partly of inhibitory origin. Herpes is very rarely, if ever, a consequence of true neuralgia, although pain so often accompanies herpes. The hair of the part may undergo changes; it may lose its pigment, fall off, or very rarely over grow. Anstie observed temporary grayness of a lock of hair after each attack, followed at last by permanent loss of pigment. (A very remarkable case has been recorded by Raymond, in which, at the time neuralgic pain in the head was most intense, all the hair of the patient [a woman aged thirty-eight] changed color from black to red, and in a few days to white, and then, in the course of fourteen days, fell off ["Revue de Méd.," September, 1882]).

Vaso-motor disturbance often accompanies a paroxysm. The first effect of the pain is usually to cause a constriction of

the vessels of the part, but this is often followed by their relaxation, flushing of the skin results, and the throbbing of the arteries may considerably intensify the pain. The arterial dilatation may be general, and be demonstrable by sphygmographic tracings (Anstie). In one case of trigeminal neuralgia all the veins of that side of the face became distended during the paroxysm, and as the pain subsided pallor replaced the flushing. The local vascular disturbance may cause local sweating, or local œdema, or even erythema, sometimes mistaken for erysipelas. The œdema thus produced is occasionally considerable; I have known each attack of cranial neuralgia to be accompanied by great swelling of the whole scalp, due to such œdema, which slowly disappeared some hours after the cessation of the pain. In another curious case, attacks of pain in the tongue and face were attended by swelling of each part, which usually came on during the night, and sometimes occurred with very little pain. Repeated attacks of such vaso-motor disturbance may lead to permanent dilatation of the vessels of the surface, and, after a time, to thickening of the cellular tissue, periosteum, and other structures.

Pathology.—Few questions have been the subject of more controversy than the pathology of neuralgia. The difference of opinion is largely due to the different senses in which, as we have seen, the word has been used. The problem of pathology is, What is the nature of nerve pain that has no known organic cause?

In neuralgia we have two symptoms, first, spontaneous pain, and, secondly, "hyperæsthesia" (more properly hyperalgesia), *i.e.*, the transformation into pain of sensations that are not usually painful (or excitation of pain by them); the former includes the latter, and therefore must be first considered. Spontaneous pain means the action of sensory nerve elements apart from local external stimulation. The pain corresponds to certain peripheral nerve areas, and we must therefore look for its cause to the elements constituting a peripheral nerve structure. These are the nerve fibres, their peripheral end organs and the central cells with which the fibres are connected. To which of these can we ascribe a functional activity independent of external stimulation? We know nothing of a capacity for such action on the part of nerve fibres. They possess a limited power of transforming external

energy into nerve force, which constitutes their "excitability," and they "conduct" nerve force, but there is no evidence to show that, apart from such external influences, they are capable of the independent evolution of nerve force. Nor are any facts known which would suggest that the peripheral end organs of the sensory nerves are capable of such independent function. We are thus reduced, by exclusion, to the central terminations of the nerve fibres as the source of the pain in iodiopathic neuralgia. (The central theory has been adopted, among others, by Vulpian, Anstie, Clifford Allbutt and Vanlair, although the theories of these authors differ somewhat in their detail.) The fibres end in nerve cells, and nerve cells, as far as is at present known, are the only elements capable of the independent evolution of nerve force. The conclusion is further corroborated by physiological facts, which show that the sensation of pain depends on a special function of nerve cells. There are separate structures for the sensations of pain and touch; this is proved by the fact that either tactile or painful sensibility may be lost without the other. But nerve fibres are indifferent structures. Even between the fibres which conduct sensation, and those which conduct motion, there is no difference; the one may be experimentally substituted for the other without affecting function (Bidder, Vulpian and others), and it is therefore *a fortiori* probable that there is no difference between the fibres which conduct tactile and painful impressions. The different effect depends on the central connection of the fibres—on their nerve cells. Some of these, when stimulated by the afferent impulse, give rise to tactile sensations, others to pain, and it is probable that the former, when excited in a special manner or degree, may also give rise to pain. We must therefore regard the pain of idiopathic neuralgia as central (the central nature of idiopathic neuralgia was insisted on by the late Dr. Anstie [article, "Neuralgia," in Reynolds' "System of Medicine," and in the separate monograph, "Neuralgia, and its Counterfeits"], with characteristic ability, energy, and fertility of argument. But his position was unnecessarily weakened by the theory that the disease originated in the roots of the nerves as a "primary atrophy." At the same time it is right to say that some passages suggest that he included the nerve cells in which the fibres end in his conception of roots. That the seat

of the disease is frequently the sensory cells of the central nuclei has been urged by Vulpian and also by Vanlair ["Les Nevralgies," 1882]), as due to the spontaneous activity of the cells that constitute the central termination of the nerves When an external cause (*e.g.*, an injury of the nerve) causes pain, the sensation is due to the stimulation of these cells, and the pain which results from their spontaneous "discharge" is necessarily referred to the region to which their fibres are distributed.

But the sensory fibres end in two sets of cells, those of the ganglia on the posterior roots, and those within the spinal cord, chiefly in the posterior cornua. There is no direct evidence to show which of these series of cells is concerned in neuralgia. But we know nothing of any sensory function of the ganglia, and we are therefore justified in looking to the nerve cells within the cerebro-spinal axis as the seat of morbid process.

This conclusion is indirectly corroborated by the symptoms of many cases of neuralgia. Especially significant are: (1) the fact that the pain may occupy adjacent parts of several nerve regions. For instance, in one patient the pain extended on both sides from the seventh cervical vertebra, over the whole occiput and vertex to the coronal suture. (2) The phenomenon of radiation of slighter pain into adjacent nerve regions during severe paroxysms. (3) The phenomena of reflex neuralgia, in which the pain is felt in another region than that of the nerve irritated. All these are explicable only on the theory that the morbid action, felt as pain, is in the central cells, which are no doubt connected according to the relations of the surface regions from which they receive impressions, and to which their disturbance is subjectively referred.

The same conclusion (the central nature of neuralgia) is also indirectly corroborated by facts of pathology of another kind which prove that pain of neuralgic character may be produced by an organic lesion in the gray matters which is here supposed to be deranged in idiopathic neuralgia. A lesion, for instance, involving part of the sensory nucleus of the fifth nerve, caused severe neuralgic pain in the face.

If, therefore, we regard idiopathic neuralgia as the result of the over-action, the "discharge," of the nerve cells constituting the proximate centre of the nerve, the question still

remains, To what is this discharge due? It is often ascribed to hyperæmia of the centre, to dilatation of its vessels. The possibility of this cause cannot be denied, but neither can its efficiency be proved. It is an hypothesis, moreover, which only solves one problem by the introduction of another. Vaso-motor disturbance means the deranged action of the vaso-motor centre for that territory, and it is as difficult to explain the disturbed action of the vaso-motor cells as of the sensory cells. All cells possess the power of evolving force; the discharge of the sensory cells is all we have evidence of, and it seems unjustifiable to assume the intervention of other cells for its production. But it is highly probable that secondary vaso-motor disturbance may result. We know that in all organs vascular dilatation attends functional activity. If the cerebral cortex is stimulated by electricity, local dilatation of vessels quickly follows. It is probable that such secondary central hyperæmia may result from the discharge of other centres, and when established may increase the disturbance.

The over-action of the cells in idiopathic neuralgia has been spoken of as "independent." It is independent so far as our means of observation go. But we cannot tell to what extent the unstable cells may be excited to discharge by stimuli coming from the periphery. We know that some stimuli of this kind (cold or pressure) are effective in exciting attacks. It is highly probable that the afferent impressions of which we are conscious bear but a small proportion to those of which we are unconscious, which, continuous or intermittent, are due to slight cutaneous impressions, to the movement of the blood and to the nutritional processes in the tissues. It is quite possible that such afferent impulses, too trifling to affect our consciousness even when aided by attention, may excite the discharge of the unstable cells. That a strong sensory impression (*e.g.*, painful pressure on the nerve) may sometimes relieve the pain is quite in harmony with familiar facts as to the effect of sensory stimulation; a strong stimulus may inhibit reflex action which may be excited by a slighter stimulus in the same nerve region. These two facts deprive of much of its force an objection which has been often urged against the central theory of neuralgia, that the disease may sometimes be permanently cured by division of the nerve. Division of a nerve does two things, it stops all impressions from the per-

iphery, and it effects a very powerful stimulation of the centre, and to the conjoint effect of these two influences its success (not frequent) in idiopathic neuralgia may reasonably be ascribed. (It is possible, as Jaccoud has assumed, that the pressure acts partly by arresting the peripheral impressions. But that this is its chief action is doubtful, from the fact that pressure on another adjacent nerve is sometimes effectual, and so also is pressure on the peripheral side of the painful point. Cartaz has suggested that in the latter two cases the recurrent nerves may convey the pain and be compressed, but such an explanation does not apply to the effect of cutaneous irritation in arresting the pain, which is a phenomenon of the same class.)

We are accustomed to think of the sensory nerves as distributed chiefly to the skin, but the pain in neuralgia is rarely confined to the skin. It appears to the sufferer to be more deeply seated, and often corresponds in position to the nerve trunk and branches. The fibrous sheaths of both are abundantly supplied with nerves—the nervi nervorum (Sappey ["Journal de l'Anat. et de la Physiologie," vol. i., 1868, p. 47] has demonstrated their existence in the sheath of the optic nerve, and the fact mentioned in the text admits of no other explanation than the existence of similar nerves in all nerve sheaths, which has, indeed, been demonstrated by Horsley ["Proc. Roy. Med. and Chir. Soc."], 1884, and Appendix to Marshall's "Bradshaw Lecture," 1887), which ramify and end in the sheath without penetrating the inter-fascicular septa. If a nerve is compressed (as the ulnar at the elbow) the first sensation experienced is a pain at the spot, from the stimulation of the sheath nerves; if the pressure is continued there is also a sensation (tingling) referred to the peripheral distribution of the nerve in the hand, due to the stimulation of the fibres of the nerve itself. In neuralgia, the central cells of the sheath fibres often seem to be disturbed in function more than those of the fibres of the nerve itself; the pain is referred to the nerve trunk rather than to the skin.

The fact that the pain seems to dart along the nerve must be due to the spread of the discharge in the centre in a certain order from cell to cell. We cannot at present say precisely on what this depends, or why the pain seems sometimes to dart toward, and sometimes from, the periphery. (An in-

genious hypothesis has been formulated by Vanlair, which may be mentioned. According to Pierret there is a relation between the length of a nerve fibre and the size of its cell. The sheath nerves will vary in length according to the distance from the centre at which they terminate. If the cells discharge in the order of their size, beginning with the smallest, the sensation will seem to dart centrifugally. For the apparently centripetal direction, a still more complex hypothesis has been suggested. Most nerves contain recurrent fibres which proceed from adjacent nerves [Magendie, Arloing, and Tripier]. These recurrent fibres ascend the branches and trunk, but all cease before the foramina of exit are reached. If these end in the nerve sheath, the same hypothesis applied to the recurrent fibres will serve to explain the centripetal darting; the longest fibres, which end nearest the centre, will have the largest cells, and discharge last. Unfortunately, we do not know that any of the recurrent fibres end in the nerve sheath, and if they do, it is probable that their central connection is the same as that of the direct fibres. It seems more probable that the arrangement of the cells in the centre depends on the distribution of the fibres in the sheath, and that this arrangement, and not the size of the cells, determines the order of the discharge, which may traverse the centre in opposite directions in different cases, just as in one epileptic an aura may pass down, and in another up, the arm.)

The irradiation of the pain, in severe attacks, to neighboring nerve areas is clearly, as already mentioned, a central phenomenon. An intense discharge always tends to spread to other connected cells, in proportion to its intensity, as the phenomena of epilepsy abundantly illustrate.

The origin of the tender points is obscure, and has been the subject of much speculation. (The hypothesis [of Cartaz and others] which connects these tender points with the distribution of the recurrent nerves, rests on too many unprovable assumptions to deserve detailed description.) Their localization to the places at which nerves emerge from deeper structures, or divide, suggests their dependence on mechanical causes. Accidental pressure, and traction in movement, will have most influence on the nerves at such places, and cause there a greater degree of stimulation of the nervi nervorum (Vanlair). Many phenomena of neuralgia suggest, moreover,

that a neuralgia which is at first purely central may not remain so. We have seen that the pain often causes secondary vascular disturbance in the territory of the nerve. It is most unlikely that such disturbance, conspicuous in the skin, is confined to the surface. It probably involves, also, the deeper structures, and especially the nerve sheaths, in which the pain is especially localized. Such secondary vascular disturbance, and the tissue changes to which it ultimately leads, must constitute a source of irritation of the nervi nervorum, and, in a truly "vicious circle," must intensify the malady, which, at first central, may be thus, at last, peripheral also. It is probable that this mechanism takes an important share in the production of the tender points, and is also the cause of the intractability of some neuralgias.

Not only is it probable that peripheral disturbance takes part in the pathogenesis of central neuralgia, but it is certain that central disturbance is concerned in some neuralgias of peripheral origin. A traumatic cause, an injury to a nerve branch, may induce pain far wider in area than the distribution of the injured branch, or even of the nerve from which it comes. In some cases, again, the pain is felt not in the area of the nerve injured, but in that of some other nerve (reflex neuralgia). An interval usually elapses after the injury before the pain is felt. Lastly, in some traumatic cases, division of the nerve does not cure the neuralgia. These facts can only be explained by assuming that the chief cause of the pain is a morbid state of the central cells, excited by, but to some extent independent of, the peripheral lesion. Doubtless, in all neuralgias of "symptomatic" character, the symptoms depend, in varying degree, on an induced central disturbance.

Various Forms.—Cases of neuralgia differ much according to their situation, character, and cause, and hence it is necessary to describe in some detail the varieties of the disease. According to situation, we have to consider separately those which occupy the head, neck, arm, trunk, and leg. According to character, we have "epileptiform neuralgia," and "reflex" or "sympathetic neuralgia," while of those which depend on special causes the most important are the traumatic, herpetic, anæmic, malarial, syphilitic, and diabetic forms.

It is important to remember that neuralgia is often not confined to a single nerve. Those who are liable to the affec-

tion in a high degree sometimes suffer from neuralgia in many situations, simultaneously or in succession.

Varieties Depending On Situation.

Neuralgia of the Fifth Nerve. Trifacial or Trigeminal Neuralgia; Tic Douloureux; Prosopalgia.— Under these various designations the most common form of neuralgia has been described. Neuralgia of the fifth is probably more frequent than all the other varieties together, and it presents, in most typical forms, the characteristic symptoms of the disease. Nor is this surprising when we consider that the fifth is incomparably the most important nerve of common sensibility in the body.

The causes of this form are all those that have been described in the section on etiology; indeed, the general history of neuralgia is, to a large extent, based on the symptoms of this variety. It is equally common on the two sides. The seat of the pain may be in any of the three divisions of the nerve; and it more commonly occupies one or two of the divisions than all three. The tender points are often well marked, and in them the pain has its chief intensity.

Neuralgia of the first division is felt chiefly in the supra-orbital branch, and hence is often called supra-orbital neuralgia. The frequency with which it was formerly due to malaria has left for it the popular name of "brow ague," although this cause is now rarely operative in this country. The pain radiates from the supra-orbital notch over the anterior half of the head, and is often felt in the eyelids and even in the eye, and in the side of the nose. The most important tender points are the supra-orbital, just above the notch or foramen of that name; a palpebral, in the outer part of the upper eyelid; a nasal, at the emergence of the nasal branch at the lower edge of this bone, and sometimes an ocular, within the eyeball. Pain felt just above the eyebrows is sometimes due to a morbid state of the frontal sinuses, but pain from this cause is generally double, and is often secondary to coryza. The lining membrane of the sinus is supplied by the fifth nerve, and it has been conjectured that the pain occurs when the small opening of the sinus into the nasal cavity becomes closed. Seeligmüller thinks that this is the cause of the pain even in

malarial cases. But the nerves of the sinus (or their centres) seem to be particularly obnoxious to certain influences, as is shown by the peculiar pain, evidently referred to these sinuses, which many persons experience after eating ices. We cannot, therefore, conclude that because the pain occupies this situation, it is necessarily due to a local cause. The supra-orbital region is a not uncommon seat of pain that is apparently neuralgic. This does not follow the course of the nerves, but it may be felt sometimes over one eye, sometimes over both.

Ocular Neuralgia.—The eyeball is an occasional seat of neuralgic pain, often of considerable severity. This is sometimes associated with some error of refraction, especially hypermetropia, but occurs also independently of any abnormality of the eye itself. Either one or both eyes may be affected. The pain may occur spontaneously, or may be brought on by use of the eyes; it is not often accompanied by photophobia. When severe, there is occasionally dimness of sight, apparently of inhibitory origin, and the amblyopia may be accompanied by a peripheral restriction of the field of vision. Ocular neuralgia may exist alone, or be associated with pain in adjacent parts, and sometimes with pain that extends far beyond the limits of the fifth nerve. Bilateral pain sometimes passes from the eyes over or through the head to the occipital region, and even down the neck. Anæmic girls often complain of a peculiar dragging pain at the back of the eyes, increased by their use. Ocular pain is often associated with rheumatism; the subjects of rheumatic iritis are often liable to pain in the eyeballs, which seems to be neuralgic in character.

In neuralgia of the second division, infra-orbital neuralgia, the pain occupies the area between the orbit and the mouth and extends over a great part of the cheek, and to the ala nasi. The chief foci of pain and tender points are an infra-orbital, at the emergence of the nerve beneath the orbit; a nasal, at the side of the nose; a malar over the most prominent part of that bone, and a gingival line below that bone, along the line of the gums of the upper jaw; very rarely there is a point in the palate, or in the upper lip. The most acute pain is often felt only in one portion of the nerve, as, for instance, the side of the nose; but it usually radiates, in a slighter degree, over a wider extent.

When the third division is affected, the pain often extends

over a large area, occupying the parietal eminence and the temple, the ear, the lower jaw, and the tongue. The chief tender points are the inferior dental, at the foramen of that name; the temporal, in the posterior part of the temple on the auriculo-temporal branch; it may be a little lower down, just above the zygoma in front of the ear, and is a very common focus. The parietal, over the parietal eminence, is common to this and to occipital neuralgias. Sometimes there is a focus of pain in the tongue. Separate portions of this branch are sometimes affected alone, especially the inferior dental and the auriculo-temporal. A boring pain limited to the temporal point is especially common. Most intense neuralgia is sometimes confined to the lingual branch. Occasionally, a tender point exists in the cervical spines, at the first two or at the fifth (Armaingaud), its exact cause is obscure.

The pain in trigeminal neuralgia is often peculiarly intense and presents every variety. It may radiate from one part of the fifth nerve to the next, and even to other nerve regions. Thus, in one case of violent neuralgia of the second division of the fifth, the pain often radiated to the occipital region and sometimes to the shoulder on that side. The effects of cold and contact in exciting the pain are well marked, and it is often increased by movement of the face or jaw, so that, in severe cases mastication may be impossible and it may be difficult to give sufficient food. If the ear is the seat of pain, either alone or with other parts, the attack may be accompanied or followed by auditory hyperæsthesia. When the pain is very acute and sudden, reflex muscular spasm may occur in the face (the "tic convulsif" of the French). Paralytic phenomena are rare, but transient paralysis of the third nerve has been observed to follow each paroxysm of pain in the supra-orbital branch. Sometimes paroxysms of severe pain in the fifth nerve are accompanied by subjective flashes of light, especially when the eyeballs are the seat of pain. The vaso-motor disturbance already described is frequently seen, flushing, sweating, permanent dilatation of the vessels (often conspicuous in the eye), salivation, increased secretion of mucus in the nose, lachrymation. Trophic disturbances occasionally occur; acute, as erythema; or chronic, as thickening of the periosteum, loss of hair, or local grayness. Even unilateral atrophy of the face has been met with (Boisson). It is probable that in many of

these cases the pain was the result of actual changes in the nerves. The course of the fifth in the base of the skull through the membranes and bony foramina exposes it to damage from many morbid processes, and causes its fibres to suffer when there is any inflammatory swelling of the sheath.

Occasionally migratory pains are felt in various parts of the scalp, sometimes on one side, sometimes on the other, without any distinct relation to the nerve trunks. There may be tenderness of the skin during and after the paroxysms of pain. This form is sometimes more closely allied to rheumatism than ordinary neuralgia is. Rarely there is neuralgic pain over the whole scalp at the same time, so that, as one patient expressed it, there is "a cap of pain on the head." Pain at the vertex is a common form of headache sometimes closely allied to neuralgia, and it may alternate with characteristic neuralgic pains in other situations.

Cervico-Occipital Neuralgia.—The pain is felt in the region of the neck supplied by the first four cervical nerves and in the posterior portion of the head, chiefly along the course of the great occipital nerve. Thus the pain may extend over the greater part of the neck as well as over the head as far forward as the parietal eminence and the ear. It is occasionally confined to the posterior branches, extending over the back of the neck and occiput. The most important tender points are (1) about midway between the mastoid process and the spine at the point at which the great occipital nerve becomes superficial, (2) over the branches of the cervical plexus between the sterno-mastoid and trapezius, and (3) just above the parietal eminence, the focus common to occipital and trigeminal neuralgia. Fusion of these two forms of neuralgia occurs, so to speak, not only above but below where the distribution of the cervical nerves blends with that of the third division of the fifth over the lower jaw. A primary cervical neuralgia may extend into this region of the fifth; doubtless the centres blend as does the distribution. It is probable that cervico-occipital neuralgia is more often bilateral than any other form, especially when confined to the occipital region. I have known most severe bilateral neuralgia to be limited to the anterior cervical region from the jaw to the upper part of the thorax on each side. The pain in cervico-occipital neuralgia is rarely intermitting; there is more or less dull constant

pain with occasional exacerbations, less violent than in the trigeminal form. The scalp may become extremely tender, so that during the pain the patient cannot bear the hairs to be touched. This form of neuralgia is not common, and Anstie believes that it occurs especially in those who have suffered from other forms.

Cervico-brachial and brachial neuralgia includes those forms in which the pain is referred to the region supplied by the four lower cervical and the first dorsal nerves. The pain may be felt in the lower and posterior part of the neck or any part of the arm and hand, but is commonly most intense in the axilla at the brachial plexus and along the course of the ulnar nerve. The region of the last is a very frequent seat, but sometimes the pain is referred to other nerves. It is commonly increased by movement and may render the arm almost useless. It is often excited by writing, and this may give rise to an erroneous impression that it is connected with the act of writing. Some severe forms of brachial neuralgia indeed have their origin in a sensory occupation neurosis. The most common tender points in brachial neuralgia are the axillary; the circumflex at the posterior border of the deltoid; a superior ulnar behind the elbow, and an inferior ulnar in front of the wrist. The latter is the most frequent of all the brachial foci. Others occasionally met with are the vertebral, by the side of the lower cervical spines; a scapular, at the inferior angle of that bone; an external humeral, on the outer side of the arm three inches above the condyle over the musculo-spiral nerve; and a radial, in the lower and outer part of the forearm. There is usually some constant pain in addition to the acute paroxysms. Occasionally it may radiate to the side of the chest, and then if on the left side may simulate angina pectoris. The pain is almost always intensified by movement. Trophic disturbance in the arm is very rare in cases of true neuralgia, and indeed probably always indicates neuritis. When the pain starts from the fingers it may begin with some sensation other than pain, such as tingling, which changes to pain as it passes up the arm. In some patients with brachial neuralgia the arm is peculiarly liable to be the seat of tingling at night. Brachial neuralgia is not often due to diathetic causes, with the exception of rheumatism, with which it is often associated even when there is no suspicion of

neuritis. On the other hand it is more frequently than any other as the result of injury. Probably many cases of supposed neuralgia are really cases of neuritis of the brachial plexus which may arise by migratory inflammation. But true neuralgia of the arm widely spread may be set up by a slight injury, as a blow which does not apparently cause neuritis. Brachial neuralgia is occasionally associated with neuralgia of the fifth, and this when there is no connecting pain in the neck.

Trunk Neuralgia.—Of the neuralgias of the trunk we have first the dorso-intercostal forms, which occupy the intercostal nerves from the third to the ninth, characterized by pain coursing along the intercostal spaces or parts of them. It is sometimes bilateral and symmetrical. There is usually a constant dull pain with acute stabbing exacerbations, but sometimes the continuous pain exists alone or the sharper pains are excited only by movement, respiratory or other. There are foci of intensity and tender points at the emergence of the three branches of the intercostal nerve—besides the vertebræ near the middle line in front and midway between these two points in the mid-axillary line. Intercostal neuralgia is most frequently due to cold or to injury, such as a contusion. It is sometimes extremely obstinate and of long duration.

The thoracic wall is also the seat of more trifling neuralgic pains; one of these is pleurodynia, which differs from true intercostal neuralgia in being usually localized at one spot not corresponding to the course or exit of the intercostal nerves. It appears to be a true neuralgia, distinguished from myalgia by the fact that it is local, very acute in character, and is excited by expansion of the thorax rather than by lateral movements of the trunk. The theory that it is a neuralgia of the pleural nerves has much probability. Another common neuralgic pain is the infra-mammary pain of anæmic women. Very limited in position it is more or less constant, and is rarely increased by respiration to such an extent as to interfere with the free expansion of the thorax. The relation between intercostal neuralgia and pulmonary trouble is a difficult subject on which satisfactory facts are difficult to obtain and few observers have ventured to corroborate the statement of Woillez that acute intercostal neuralgia is always accompanied by a secondary congestion of the lung. It is more

probable that when this association exists the true relation is the reverse. The intercostal nerves are frequently the seat of herpetic neuralgia.

The neuralgias that occupy the lower half of the trunk have been grouped as lumbo-abdominal. The pain has a course similar to that of the intercostal form. Foci of pain and tender points are found at the back; beside the vertebræ over the posterior branches; at the middle of the iliac crest (iliac point); at the lower part of the rectus (hypogastric point), while sometimes there is in the male a scrotal and in the female a labial point. These pains are often bilateral and may change their position from time to time. They are generally acute pains, but now and then have a constricting character like the girdle pain of organic disease, but distinguished from them by their irritability. Lumbo-abdominal neuralgias seem to be sometimes secondary to disease of the pelvic organs, especially in the female. Neuralgia in the penis sometimes results from masturbation. It may also be due to lithæmia, and from this cause I have known it to be most severe and long continued.

The spinal column is a very common seat of neuralgic pain, especially in weakly women and after concussion of the spine. It constitutes one of the most troublesome of the many pains of hysteria, and one of the most enduring consequences of railway accidents. The pain is in most cases felt through a considerable vertical extent of the spine, and is specially intense in certain spots, commonly in more than one. The dorsal region is the most common seat, next the lower cervical, and least frequently the lumbar region. Sometimes the pain is localized on one side of the spine, close to it. The pain felt in the spine in cases of gastric ulcer seems to be a sort of reflex neuralgia. Often the pain seems to pass up to the back of the head. Spinal neuralgia may be associated with a similar pain in some other part of the trunk, shoulder, arm, or leg. It is usually accompanied by considerable tenderness, and is increased by fatigue, by use of the legs, by long sitting or standing, and also by vibration, such as the movement of a carriage. The latter point is often of considerable diagnostic importance, for it is far more marked in neuralgia than in spinal disease causing pain. On the other hand the pain is not increased by

slight movement, as is the pain of growths and caries. The pain is seldom distinctly paroxysmal, but as already mentioned in one case, paroxysms of pain in the cervical spine and vertex were most intense, and each was accompanied by opisthotonos. It is uncertain in what structure this spinal neuralgia is produced. It is often associated with neuralgia elsewhere, and also with rheumatism of the fibrous tissues, so that some cases seem to be of the nature of rheumatic neuralgia. It has been thought that the membranes are the seat of the pain, but there is no real evidence for or against the theory.

Another very common seat of neuralgia is the sacral region, no doubt in consequence of the plexus of nerves that lies between the bone and the skin. Pains of pelvic origin are often referred to this region, as those of parturition show. Occasionally the pain is felt chiefly about the coccyx—coccydynia it has been termed. It must be remembered that the fibrous tissues over the sacrum are sometimes the seat of acute rheumatism, such as higher up gives rise to lumbago.

Neuralgia of the Leg.—In the lower limb, neuralgia is rare in the region supplied from the lumbar plexus, although a crural form, in which the pain occupies chiefly the front of the thigh, is occasionally met with. In most instances pain in this region is of secondary origin, due to a lesion of the lumbar plexus, as, for instance, from the pressure of an abdominal tumor, or is due to the extension of neuritis from the sciatic up the lumbo-sacral cord.

The majority of neuralgic pains in the leg are in the region of the sciatic nerve, and are grouped under the designation of sciatica. The pain occupies especially the course of the nerve, but tender foci are met with in certain spots; lumbar, near the spine, just above the sacrum; sacro-iliac, at the articulation of the same name; a gluteal opposite the middle of the lower border of the gluteus; a series of spots varying in exact position, along the course of the nerve in the posterior aspect of the thigh; a peroneal behind the head of the fibula; and a malleolar behind the lower extremity of the fibula, and an external plantar at the outer border of the foot. It is, however, certain that sciatica is seldom a true neuralgia. Almost all severe cases are due to inflammation of the nerve trunk.

Lastly, it should be mentioned that in rare cases neuralgic pains are felt almost everywhere, in the limbs, trunk, and head, and apparently constitute a sort of universal neuralgia. The few cases I have seen have been in adult men, and associated with hypochondriasis.

Varieties Depending On Character.

Epileptiform Neuralgia.—The term is applied (according to the example of Trousseau) to an intractable form, in which each attack of pain comes on very suddenly, with intense severity, and lasts usually less than a minute, sometimes only a few seconds, rarely for two or three minutes. The frequency with which the attacks recur varies in different cases; the daily number may be one or hundreds. The pain is most intense, so that the patient stamps about the room in agony, or tries to get relief by violent rubbing or by pressure. Trousseau relates the case of a lady whose malar bone was atrophied in consequence of the pressure, repeated every few minutes for years. The sufferers from this terrible form are usually in the second half of life. The pain is invariably in the region of the fifth nerve, sometimes in the whole, sometimes in part only, but rarely confined to a single branch. Convulsive spasm in the face may accompany the pain (convulsive epileptiform neuralgia).

Reflex or sympathetic neuralgias are those in which the pain is felt in another nerve region than that in which its cause exists. The radiation of neuralgic pain must, as already stated, be ascribed to the extension of the central discharge to adjacent centres, but the term "reflex neuralgia" is not applied to these cases, but to those either in which the pain exists only at a distance from its cause, or in which the distant neuralgia has no sensory continuity with the primary pain. The pain of a carious tooth may be associated with a distant and apparently disconnected pain in some other part of the fifth nerve, or such disease may cause only distant neuralgia, and no local pain. The proneness of the fifth nerve to be affected in neuralgia renders it a common seat of the reflex form. For instance, neuralgia limited to the fifth has been produced by injury to the ulnar nerve, and to the occipital nerve (Anstie). Nevertheless, the converse relation is some-

times observed; carious teeth are said occasionally to cause neuralgia in other parts, as, for instance, in the cervico-brachial region (Salter). When no local pain is felt, we must assume that the centre to which the afferent impressions directly come is not thus excitable to painful activity, or even so as to influence consciousness, but that it is in connection with another centre which, by natural or acquired susceptibility, is disposed to excessive action. Curious cases are on record in which the act of micturition invariably caused a pain in the region of the arm supplied by the ulnar nerve. (Several such cases are given by Vanlair ["Les Nevralgies," p. 330]; one of them comes from a curious source—the autobiography of Clarendon, Chancellor to Charles II. See p. 8.)

The cause and seat of reflex neuralgia may be most varied. When due to causes acting on the cerebro-spinal nerves, the pain is usually felt on the same side, and rarely far distant. A most inportant class of reflex pains are those which are produced by disease of the internal viscera, such as the spinal pain in ulcer of the stomach, the sacral pain in uterine disease, the scapular pain in affections of the liver, the pain in the testicle in renal colic, and pain in the front of the thigh and spine in gonorrhœal orchitis (Mauriac). Many of these are rather reflex pains than neuralgias properly so-called, but they occasionally persist when their cause is removed, or assume disproportionate intensity. They are of very great practical importance on account of the readiness with which their cause may escape attention.

Traumatic neuralgias are those that are excited by injury to nerves—by contusion, and by punctured, lacerated, or incised wounds. In some instances the pain is the result of a neuritis, set up by the injury, which may ascend the nerves and pass, at a junction, to other trunks. But neuralgia may also be set up when there is no more than the local and transient inflammation at the spot injured. Such neuralgia may follow nerve lesions in any part of the body, but is especially frequent in the arm, the nerves of which are much exposed to injury. In the days of venesection, a common form of neuralgia resulted from injury to a cutaneous nerve in the operation. It may follow wounds of both large and small nerves, and by some observers has been thought to be more frequently due to the latter, but it must be remembered that small nerves

are more often wounded than larger trunks. Anstie believes that neuralgia is more common from partial injuries than when the nerves are completely divided. It is a frequent consequence of gunshot injuries of nerves. In some cases the disease is apparently due to the implication of nerves in a cicatrix, or to the morbid enlargement of the nerve ends after amputation, which goes by the name of "bulbous nerves."

The pain may commence a few hours or days after the injury, but more frequently not until after an interval of some weeks or months. It generally begins in the point injured, but is rarely limited to this, or even to the part supplied by the wounded branch. It usually radiates to adjacent regions, and sometimes to parts supplied by other nerve trunks. For instance, a woman received a blow on the thorax, probably contusing an intercostal nerve. The early pain of the injury ceased, but two months later neuralgic pains commenced at the spot and gradually spread over the whole region supplied by the cervical and brachial plexus (Ollivier). The pain may be less intense at the seat of the injury than it is elsewhere, and it may even be absent at the seat of injury, and then is of the purely reflex variety, instances of which have been already mentioned. These facts, and also the circumstance that the pain is not always influenced by excision of the injured nerve, show clearly that it is to a large extent of central origin, the expression of a disturbance which, though excited by the nerve injury, is to a considerable extent independent of it, and doubtless due, not only to the excitant, but also to a predisposition, such as is concerned in the production of other neuralgias. The same fact is seen even more clearly in cases in which some injury leaves a slight local weakness or disability, and years afterward the part may become the seat of neuralgic pain, under the influence of a constitutional tendency.

The pain is usually intermitting in character, but often most intense in degree and causes profound depression of the general system. It may excite muscular spasm, and, especially at the onset soon after the injury, other reflex symptoms, such as vomiting. Vaso-motor disturbance occasionally ensues; trophic changes in the skin and joints have been described, but it is very doubtful whether nutrition suffers except in consequence of considerable secondary neuritis. The course

of traumatic neuralgia is often tedious, sometimes most prolonged, and it has been known to last to the end of life, in spite of every medical and surgical measure that could be devised for its relief. When it ceases, it is prone to recur under the influence of general depressing influences.

Occupation Neuralgias.—These are pains, more or less neuralgic in character, brought on by some habitual act. They are the sensory forms of occupation neuroses.

Herpetic Neuralgia.—Herpes zoster, whatever its seat, is usually accompanied by pain of a neuralgic character. The explanation of this is found in the conclusive evidence that the eruption is the effect of nerve irritation, probably always inflammatory in character. It always corresponds in area to the distribution of certain nerves, and, post-mortem, inflammation has been found in the nerve (Haight) and in the ganglia of the posterior roots (Bärensprung, Charcot, etc.). The neuralgic pain is thus "symptomatic," the result of organic changes in the nerves. Sometimes herpes occurs in the area of a nerve which has long been the seat of neuralgic pain, and hence is regarded as an occasional trophic effect of neuralgia, but it is an extremely rare consequence, and probably only occurs in cases in which the pain is the expression of organic changes in the nerve, and is not a consequence of simple idiopathic neuralgia.

In cases of ordinary zoster, pain may occur before or after the eruption. The initial pain precedes the appearance of the eruption for a few hours or days, is usually moderate in severity, and commonly (but not always) subsides as the cutaneous lesion is developed. Sometimes this pre-herpetic pain is absent, especially in the young, although even then there is usually an initial sensation of tingling in the part. The pain that succeeds herpes is more constant. It comes on usually during the decline of the eruption, and its occurrence bears no relation to the seat of the herpes. The most common situations for neuralgia are those in which herpes is most common: the side of the trunk, the forehead, neck, and leg. The pain has the distribution of the eruption, corresponding to the nerve or nerves affected. It varies in degree, but is usually acute, lancinating, and for a time severe, and is accompanied by great tenderness of the skin. The same tender points are met with as in ordinary neuralgia in the same area. There is an

important relation between the age of the patient and the severity and duration of the pain. In old persons it constitutes one of the most severe and persistent forms of neuralgia, often continuing for months and even years before, at last, it slowly lessens. Occasionally, it persists in unmitigated intensity to the end of life. Sir William Jenner, in his lectures, was accustomed to illustrate the obstinate persistence of this pain in the old, by the instance of a man who, before the days of anæsthetics, endured the excision of the skin to which the pain was referred, in the hope of relief, but found none, and then, unable to bear the continuous agony, he shot himself.

Hysterical Neuralgias.—Neuralgic pains are common in hysteria, but a distinction must be drawn between those which are merely associated with, and those which are due to, the general neurosis. Of associated forms, every variety of true neuralgia may be met with, due to the neuropathic disposition, which is also the cause of the hysteria, and they present the characteristic distribution, tender points, etc., of the ordinary form. Anæmic neuralgias are also common in hysterical patients, in whom the nerve disturbance is often due to poorness of blood. Of the neuralgic pains that are due to hysteria, some are in the cerebro-spinal system, some in that of the sympathetic. Certain local pains in the head are very common, but these differ from ordinary neuralgia in being confined to one small spot, instead of following the course of nerves, and the local pain has a sharp stabbing or boring character, as if a nail were being driven in, and hence has received the name of "clavus hystericus." It is important to remember that a similar pain may be met with apart from hysteria, especially in anæmic persons. Pains in the spine are also extremely common, sometimes very local, and of various characters. Other pains referred to the trunk are usually associated with local tenderness, in the so-called "hysterogenic points." The most important neuralgias of hysteria are those of the viscera, ovarialgia (not necessarily in the ovary itself), and gastralgia, being the most frequent. Pains in the joints and muscles are also common. The visceral neuralgias, and those associated with parietal tenderness, are usually persistent, but the local boring pains, and those in the joints and muscles, are often fugacious and migratory, and this constitutes a diagnostic point of considerable importance.

Rheumatic Neuralgia.—In a loose way, all neuralgias produced by cold are sometimes styled "rheumatic," but the mere causal relation scarcely warrants the epithet. The peculiar affection termed "muscular rheumatism" is also sometimes called a rheumatic neuralgia, but this is to extend the use of the term in a manner that is scarcely justified or needed. Pain that occurs only on movement should never be called neuralgia. Acute articular rheumatism is rarely associated with true neuralgia, but some forms of spontaneous pain are frequently produced by cold in those who present what is termed the "rheumatic diathesis," who perspire easily, are liable to catarrh, and whose urine readily becomes loaded with lithates after a chill. Such pains may correspond to a certain nerve, or may occupy some part of a limb, without any definite relation to nerves, and are often migratory. Their exact pathology is uncertain.

Gouty Neuralgia.—The subjects of gout not unfrequently suffer from nerve pains, apparently idiopathic in character, coming and going, and sometimes very severe. The pain may disappear when an attack of acute gout is developed. The fifth nerve, the intercostals, and the sciatic are those most frequently affected. Severe sciatica sometimes occurs in the gouty, but is certainly due to neuritis. One visceral neuralgia is also sometimes due to gout, gastralgia.

Diabetic Neuralgia.—Patients with diabetes may suffer from neuralgic pains that have no special characters, but Worms has called attention to the symmetry of the pains, which occupy the same nerve on both sides, as a characteristic of diabetic neuralgia. The pain has hitherto been observed chiefly in the third division of the fifth nerves and the sciatics. It must be remembered, however, that ordinary neuralgia is occasionally symmetrical. The nerve pains met with in diabetes are often severe and obstinate until the cause is removed by dietetic treatment. They have been observed to increase and decrease with the amount of sugar in the urine. Ziemssen has suggested that they may be sometimes the result of a peripheral neuritis analogous to that which is met with in alcoholism. While there is some evidence that such neuritis may occur in diabetes, it seems improbable that it is the cause of the pains commonly met with.

Anæmic Neuralgia.—Anæmia is one of the most powerful

causes of neuralgia in all its forms, but certain varieties are more frequent than others in this condition, especially in young women. One of these is situated in the fifth nerve, either in the first division or in the auriculo-temporal branch. It is generally intermittent and is increased by movement, while it is lessened by the recumbent posture. Another, still more frequent and more continuous, is that which is felt in the thorax, in the fifth and sixth interspaces on the left side, the well-known infra-mammary pain. Gastralgia is also common apart from ulcer. Headaches that have no true neuralgic character are also very common.

Neuralgia of the cerebro-spinal nerves sometimes occurs in lead poisoning, but it is uncertain whether it is the result of the toxic influence or of the anæmia which this causes. The knowledge of their cause, however, is very important. According to Briquet, lead colic is in part a neuralgia of the abdominal wall, but the evidence of this is scarcely satisfactory.

Malarial neuralgias are not very common even where ague is frequent. They present nothing characteristic in seat, although the supra-orbital and intercostal forms are the most frequent; nor is there anything special in the character of the pain. Their chief feature is regular periodicity, the intervals between the attacks being from one to four days. The periodicity is less characteristic when the attacks are diurnal than when three or four days intervene. Occasionally the paroxysms are attended with slight symptoms of an ague fit, a trifling cold and hot stage (Anstie). They must not be confounded with the vaso-motor phenomena met with in cases of the ordinary form. It is very doubtful whether the neuralgia is, in most cases, a direct effect of the malarial poison, in the sense in which ague is. It is probably an indirect effect, the result of the anæmic and depressed state of the nervous system induced by malarial influences, even in those who do not suffer from intermittent fever. Neuralgia, apparently due to malarial causes, does not always yield to quinine, even when given in the most approved manner. It is probable that the exact periodicity of many malarial neuralgias is the result of the state of the nervous system produced by the poison, and it does not prove the neuralgia to be truly malarial. In some cases of supra-orbital neuralgia with exact periodicity coming

from malarial districts (recorded by Seeligmüller), quinine failed entirely while other treatment was quickly successful.

Syphilitic Neuralgia.—The pains of syphilis constitute a prominent symptom of that disease, but, for the most part, have no correspondence with nerve distribution, and can therefore be scarcely regarded as neuralgic. Symptomatic neuralgic pains occur in many syphilitic affections of the nervous system which cause irritation of the nerves or their roots, in chronic meningitis, neuritis, and pressure from growths. One of the most severe and obstinate cases of pain in the region of the fifth nerve I have seen, was due to chronic syphilitic meningitis at the origin of the nerve. In such cases the nature of the lesion is usually clear from the evidence of structural damage to the nerve fibres. In the case just mentioned, for instance, there was anæsthesia and paralysis of the masseter. Whether idiopathic neuralgia results from the influence of the syphilitic poison is uncertain. Fournier believes that such neuralgia is common during the secondary stage, but very few conclusive cases have been recorded. It must be remembered that in the early stage of neuritis, etc., pain may be the only symptom, and, on the other hand, the anæmia which results from syphilis may be the real cause of the neuralgia. Anstie believed that syphilis does not cause true neuralgia, although it may recall a neuralgia which had long ceased. A peculiarly distressing post-sternal pain, apparently neuralgic in character, has been occasionally observed in constitutional syphilis (Eccheverria, Buzzard).

Degenerative Neuralgia.—In advanced life, and sometimes before the senile period is reached, neuralgia is occasionally met with, of extreme obstinacy, and associated with other signs of degeneration of the central nervous system, such as failure of memory or persistent mental depression. The neuralgia is apparently one effect of a degenerative tendency. The affection has all the characters of a central neuralgia. The fifth nerve is by far the most common seat, but the pain sometimes occurs in other situations.

Diagnosis.—The diagnosis of neuralgia rests on the unilateral situation of the pain, on its correspondence to the distribution of certain nerves, its intermitting or remitting character (*i.e.*, the occurrence of paroxysmal exacerbations), on the fact that the patient has suffered from similar pains elsewhere,

on the variations in the seat of the pain, and on the absence of any indications of actual damage to the nerve fibres. The variability is a symptom of great importance. If the pain shifts about, now in one spot and now in another, it is not likely to be due to an organic cause. For instance, a man with fronto-occipital neuralgia had foci of pain in the forehead, temple, and occiput, but he never had pain at the same time at more than one of these places. The last of the above indications, however, is the most important element in the distinction of neuralgia from the similar pains which result from organic disease of the nerves due to external pressure or neuritis. It is in the case of the fixed neuralgias (as distinguished from the migratory forms) that the distinction is of chief importance. The diagnosis is more difficult in the case of neuritis than of external pressure (tumor, etc.), because, in the latter case, the cause of the pressure usually produces other symptoms, and the effect of the pressure is progressive, so that gradually increasing signs of a structural lesion are added to the pain. But from neuritis the distinction may be much less easy. The difficulty is the greater, the slighter the degree of neuritis. Severe inflammation causes severe constant pain, at first more intense at its seat than in the distribution of the nerve, because the inflammation is most intense in the nerve sheath, and the sheath nerves suffer first. In severe forms, however, the proper fibres of the nerve are soon implicated to a degree that interferes with their conducting functions, so as to cause at first persistent hyperæsthesia, and then areas of anæsthesia, while if the nerve is "mixed," the muscles supplied become weak, flabby, and rapidly waste, with changes in electrical irritability. In slight cases, on the other hand, the sheath chiefly suffers; there may be no interference with conduction, and the pain resembles neuralgia more closely, although, as a rule, it is more continuous than in true neuralgia. The diagnosis is also difficult when the seat of the neuritis is such as to render the nerve inaccessible to direct examination. If it can be reached, there will be found from the first local tenderness of the nerve. In idiopathic neuralgia tenderness of the trunk in the intervals is only developed after the neuralgia has existed for some weeks. Moreover, in neuritis distinct swelling of the nerve can sometimes be felt. Local tenderness is thus chiefly of significance in the early

stage of the affection, or when it occupies a considerable area of the nerve trunk, and is not confined to certain points.

If, therefore, the pain is migratory, if it is completely intermittent, especially if the intermissions are of long duration, if the attacks are induced by psychical influences, the suspicion of an organic cause will scarcely arise. If, on the other hand, there is continuous pain, rapidly developing to a considerable degree, organic disease should be suspected, and the suspicion will be confirmed if there is persistent alteration of sensibility, muscular wasting or altered irritability, or trophic changes in the skin. Great care is necessary, however, in testing the muscles, because a strong electrical stimulus may greatly increase the pain. The isolated faradic shocks should be employed rather than the current, because they cause much less pain, and because they detect most readily the earliest change produced by neuritis, a slight increase of irritability; an altered reaction to voltaism will also often be found. Early tenderness of the nerve, not merely during but between the paroxysms, extending some distance, and distinct swelling of the nerve, indicate neuritis. The absence of these does not exclude neuritis, because it may occupy an inaccessible portion of the nerve. If the symptoms of structural damage gradually increase and progress, and especially if they involve the whole region of the distribution of the nerve, compression may be suspected, and is confirmed by the discovery of any other symptoms indicating organic disease in the vicinity of the nerve trunk, such as, in the case of the fifth, damage to other nerves which arise or pass near it. It must be remembered that in some cases the differential diagnosis of slight inaccessible neuritis from neuralgia may be impossible, because, on the one hand, the irritation of neuritis may cause neuralgic changes in the nerve centre, and, on the other, a primary functional neuralgia may cause, by reflex vaso-motor disturbance, secondary changes in the sheath, so that in each case a mixed affection, partly functional, partly organic, is the result; or, in current terms, a neuralgia which is at first either symptomatic or idiopathic, may ultimately be both.

In all forms of neuralgia, the circumstance that the patient has previously had attacks of pain in other situation is a very important help in diagnosis. It does not, of course, prove that a given pain is of functional origin, but it is proof of a ten-

dency to such pain, which may justly be allowed considerable weight in the absence of signs of an organic lesion. This character is frequently of very great practical value, especially in cases of neuralgic pain of unusual seat. Another similar indication is the fact that the neuralgia replaces some other functional disease, as, for instance, migraine.

Special Diagnosis.—Their are certain affections with which the several varieties of neuralgia are liable to be confounded. Almost any form of neuralgia may be simulated by the pains of tabes, and the possibility of this cause should always be thought of, especially in the case of migratory pain. In some situations the risk of error is greater than in others, and these will be especially mentioned.

In neuralgias in the branches which are distributed over the skull (fifth and great occipital) it is often doubtful whether the affection should be called headache or neuralgia. The distinction in some cases is one of name rather than of real difference. Either term is employed, according as the pain seems superficial or deeply seated. But the fifth nerve gives fibres to the dura mater, and it is probable that some of the deeply-seated unilateral headaches are really allied to neuralgia, although it is customary, on account of their special association, to describe them separately. The fifth nerve is often damaged by organic intracranial disease, and nerve pain, thus produced, is sometimes mistaken for simple neuralgia. Besides the indications already described, organic disease often causes deeply-seated headache and other symptoms, especially in the functions of other nerves, optic neuritis, and paralysis or convulsion in the limbs. A history of recent syphilis increases the probability of organic disease, but does not render it certain. The occurrence of herpes in the course of a supposed neuralgia is also probable evidence, here as elsewhere, of organic changes in the nerves. It must not be forgotten that neuralgic pains are sometimes the first symptom of morbid growths in the upper jaw and parotid region.

The diagnosis of the cervico-occipital form rarely presents any difficulty. The neuralgic pain usually courses along the nerve trunk, but it must be remembered that occipital neuralgia is occasionally bilateral. In caries of the cervical vertebræ pain may be an early symptom, but it scarcely ever spreads to the occiput, and the early interference with movement is usually characteristic.

The neuralgias of the arm have to be distinguished chiefly from neuritis, by the indications already mentioned. The diagnosis is seldom difficult, because neuritis usually causes trophic changes in the muscles and skin, and the nerves are accessible in a large part of their course.

A more difficult problem is presented by the trunk neuralgias, which have to be distinguished from disease of the internal viscera, and from organic spinal disease. Unilateral pain is a frequent accompaniment of disease of the organs in the thorax and abdomen, and it is hardly necessary to point out that, in every case, a careful examination should be made of the organs adjacent to the seat of pain. The greatest difficulty arises in the case of deeply-seated tumors which cause pain by nerve compression, especially when, as is sometimes the case, the pains are "reflected" and do not precisely correspond in position to their cause. Aneurism of the aorta, for example, often gives rise to such pain, especially when seated in the descending part; pain in the course of the nerves may be the only symptom until sudden death occurs. The pain is usually very severe, and often burning in character, and has not the same foci of intensity and tender points as in ordinary neuralgia. In severe unilateral (and even bilateral) pain, persistent in occurrence, whether uniform in seat or not, this cause should always be suspected. One of the most severe cases of neuralgic pain I have ever seen—darting, stabbing, burning, migratory pains in legs, abdomen, thorax, and left arm—was due to an undiscovered abdominal aneurism. Aneurism of the ascending part of the arch is now and then accompanied by pain passing to the arm, apparently of reflex character. Intercosto-humeral neuralgia may simulate angina pectoris, of which indeed, such neuralgia may be said to form part. The severity of the paroxysms of angina, and the other distressing sensations which accompany the pain, usually render the nature of the attacks sufficiently clear.

The parietal pains which accompany disease of the spinal cord rarely have the acute lancinating character of neuralgia; they are sensations of tightness or constriction, and the obtrusive symptoms in the legs indicate the nature of the disease. To this, however, morbid growths of the cord offer an exception, especially in the early stage. The pains may be very severe and of various character, but they are constant

in seat, are increased by any movement, and, before long, indications of compression of the cord are added to them. The "lightning pains" of tabes are sometimes felt in the trunk, and have often been mistaken for neuralgia. They are distinguished by their changing seat and momentary duration, by the similar pains in the legs, and especially by diminished sensibility over extensive areas. In most cases there are some pains in the legs and the knee jerk is lost, but I have seen one case in which the changes were confined to the dorsal region of the cord and the knee jerk was normal, but in this, as in other cases of the kind, the light reflex of the iris was lost, a symptom of great indirect diagnostic importance. More constant in seat and neuralgic in character are the pains of pachymeningitis, which are caused by the compression of the nerve roots by the thickened membranes. They are distinguished from neuralgia by the wide extent of the pains, their bilateral situation, by the presence of areas of anæsthesia due to still greater damage to the nerves, by muscular wasting in the limbs, and by the symptoms of compression of the cord.

The terrible nerve pain which is sometimes produced by organic disease of the bone of the spinal column closely resembles neuralgia in its fixity of site and unilateral situation, but is distinguished by its peculiar dependence on movement of the trunk. Leg symptoms are usually soon associated with it. The lumbo-abdominal neuralgia may be confounded with renal colic. The distinction rests chiefly on the urinary symptoms that accompany the latter.

The sharp pains of tabes are more frequently felt in the legs than in the trunk, and these also are often mistaken for neuralgia, but the indications already mentioned suffice to distinguish them. The distinction of sciatic neuralgia from neuritis must be made by the indications. Of still greater practical importance is the distinction of crural and sciatic neuralgia from the pains due to pressure on the lumbar and sacral plexus by tumors in the pelvis and abdomen. Such pains are felt along the course of the nerves, and are almost invariably thought at first to be neuralgic. Pains in the front of the thigh are rare except as the result of extension of neuritis from the sciatic nerve to the lumbar plexus, or as the result of pressure. In each case there is generally muscular wasting, which shows organic damage. The diagnosis be-

tween neuralgia in the front of the thigh and pain due to a lesion of the nerves, is also aided by the state of the knee jerk, which is generally early lost in organic disease, but remains intact in neuralgia. In every case of neuralgic pain in this situation, the abdomen should be carefully examined, and whenever pain in the sciatic is of a progressive character, and apparently due to mischief above the accessible part of the nerve, a rectal examination should be made, by which the source of pressure, if there be any, will readily be felt. It must also be remembered that pain felt in the knee may be a reflex effect of the irritation of the branches of the obturator nerve in hip-joint disease, and that obscure pains in the groin and thighs are sometimes the result of disease of the femur. (In one case of intense stabbing pains in the groin, closely resembling neuralgia, the cause was ultimately found to be necrosis of the great trochanter.)

The diagnosis of the spinal forms of neuralgia need not detain us long. In the reflex or sympathetic variety the cause of the pain will usually be discovered, if it is remembered that it may be outside the area in which pain is felt. It is rarely far distant; often in another branch of the same nerve. Pain in any part of the fifth nerve, for instance, may be due to the irritation of a carious tooth.

It is possible to confound epileptiform neuralgia with the form of true epilepsy in which the aura is a sudden pain. I have known, for example, slight attacks of minor epilepsy to be preceded by a most severe momentary pain in one fifth nerve. The occurrence of distinct loss of consciousness, and still more of convulsion, sufficiently indicate the epileptic nature of the case. A painful epileptic aura in a limb, followed by local convulsion, could only be mistaken for neuralgia with reflex spasm, in a patient who had never had a severe fit. But the course of such an aura is usually centripetal and deliberate, and the spasm has also a deliberate march.

Prognosis.—The prognosis in neuralgia is influenced by the age of the patient, by the duration of the affection, its situation, severity, and cause. It is far better when the disease is due to any constitutional condition than when no general cause can be discovered, since, as a rule, the constitutional states that cause neuralgia are amenable to treatment, at any rate in such a degree as to influence the pain. In hysteria,

however, some neuralgias are readily removed; others, especially when there is no anæmia, are most obstinate. The prognosis is better when there is no hereditary tendency than when this is marked. We do not yet know whether the prognosis is influenced by the fact that the heredity is general or special. The more severe the pain, and the longer the disease has lasted, the more difficult is its treatment. It is generally believed that neuralgias of the fifth nerve are more intractable than others. During the decline of life, neuralgias of all kinds, especially those of the degenerative form, are peculiarly obstinate, and in old age they sometimes cannot be relieved by any treatment whatever. However severe the pain may be, neuralgia involves little danger to life; as Buzzard has well said, "the disease does not seem, of itself, to shorten the duration of the life which it fills with suffering." Epileptiform neuralgia is, of all forms, the most obstinate; Trousseau, in his large experience, never knew a case to be cured. When neuralgia has once ceased it is extremely prone to recur, and this fact, which is true of all varieties, must be remembered in giving an opinion regarding the future.

Treatment.—The treatment of neuralgia consists, first, in the relief of the symptom, pain, and, secondly, in the removal of its cause, *i.e.*, in the restoration of normal conditions of function in the sensory apparatus. The means by which these ends are to be secured are threefold—by hygiene, by drugs, given internally and applied externally, and by certain surgical operations on the nerves. It is evident that the treatment to be adopted must be influenced, to some extent, by the nature of the case. When there is distinct evidence of neuritis, the treatment for this, already described, must be adopted. The treatment now to be considered is that of the "idiopathic" form, but the means for the alleviation of all forms is nearly the same.

Although the relief of pain has usually to be the first actual step in treatment, the first in importance is the removal of the causes of the disease. Any discoverable condition on which the neuralgia may depend must be treated. The detailed measures that are necessary need not here be indicated, since they are sufficiently suggested by the enumeration of the causes and causal varieties already given. Especially should any source of nerve irritation be removed, whether in the

region of the painful nerve or outside it. It must be remembered, however, that it is not certain that the neuralgia will disappear on the removal of such a source of irritation. This is especially true of neuralgias of the fifth nerve and decayed teeth. The almost universal association of neuralgia with conditions of debility, so strongly insisted on by the late Dr. Anstie, indicates the importance of hygienic measures calculated to improve the general health; fresh air, adequate rest, and nutritious food in full quantity, and a small quantity of alcohol at meals is usually directly beneficial. The importance of a good supply of animal food should be urged, for all but gouty subjects. I have known severe neuralgia to occur first on the patient commencing a purely vegetable diet, to disappear when meat was taken, and recur with severity at each of four successive attempts to return to vegetarianism. Cod-liver oil, or other easily digested fat, may often be added with advantage. Iron, when there is anæmia, will sometimes alone cure the disease, and even when there is no anæmia it seems occasionally to be beneficial. Of nervine tonics, quinine is of most value in true malarial forms, given in a full dose shortly before a periodical attack is due; but, as we have seen, it is not always successful. In smaller doses it is occasionally useful in other forms, although not so frequently as might be expected from its marked influence on the nervous system. In stupefying doses it may lessen the pain for a time, but does not often produce a lasting effect. It has been thought to be most useful in neuralgias of the first division of the fifth nerve. Nux vomica or strychnine often does more good than quinine. In general, it may be said that whenever the nervous system is feeble, as it so frequently is in neuralgia, nux vomica may with advantage be added to the other remedies that are given. Zinc (including the phosphide) is of little value. Arsenic is occasionally useful, especially in the neuralgias that have been set up by malarial poisoning, and in the degenerative variety. Free phosphorus ($\frac{1}{50}$ or $\frac{1}{30}$ grain) has been strongly recommended, but my experience coincides with that of most recent writers in assigning it a very low position in the list of remedies—low so far as numerical success is concerned, although in rare cases its influence is very striking. For instance, a woman, aged forty-three, with neuralgia of the fifth nerve of thirteen years' duration, at one time associated with brachial,

and afterward with crural neuralgia, lost the pain entirely during three months' treatment with phosphorus, although when the treatment was commenced about twelve severe paroxysms occurred every day. Ammonio-sulphate of copper has been recommended in cases of neuralgia of the fifth nerve. It may be given in doses of $\frac{1}{16}$ or $\frac{1}{12}$ of a grain after food. In syphilitic forms, dependent on actual nerve lesions, iodide of potassium is now and then useful in cases of neuralgia that are not due to syphilis.

Of remedies that have a sedative action on the nervous system, although they are not anodynes, the most important is bromide of potassium. It is occasionally of service in cases of idiopathic neuralgia, paroxysmal in occurrence, although it appears to have less influence on sensory than on motor nerve cells. Its value is greatest in irritable, anxious subjects. Chloride of ammonium has been recommended in intercostal neuralgia. In the rheumatic forms, salicylate of soda has been occasionally found useful, especially in facial neuralgia and sciatica, but in the cases of sciatica which depend on neuritis it has little influence.

Nervine stimulants given at the beginning of an attack occasionally cut it short, especially in neuralgias of the fifth nerve. The most effective are sulphuric ether, valerian, and alcohol. The use of alcohol to relieve pain is, however, fraught with great danger to the habits of the patient, and should be avoided as far as possible. Another nervine stimulant, turpentine, is an old remedy for sciatica, and has been occasionally employed with cases of ordinary neuralgia. Nitroglycerin is sometimes useful for the same purpose, and, as a rule, it succeeds whenever alcohol is effective. It is also extremely valuable in many cases, given regularly three times a day in doses from $\frac{1}{200}$ to $\frac{1}{50}$ of a grain. It is most useful, as in migraine, when the face gets pale at the onset of an attack, but sometimes succeeds when this indication is wanting, although very rarely when flushing is an early symptom.

Anodynes are necessarily very important elements in the treatment of neuralgia. It is the pain for which the patient seeks help; the cause of the pain can only be slowly influenced, and in the mean time the pain itself has to be relieved. In some cases relief is all that can be afforded; treatment fails to

prevent the recurrence of pain, and all that can be done is to lessen its intensity. But in a large number of cases of neuralgia anodynes do more than merely palliate; the repeated removal of the pain tends to prevent its recurrence. This makes it probable that the relief afforded is not merely by an action of the drug on the general sensorium, but that it has an influence on the specific disturbance which gives rise to the sensation of pain. The same conclusion is suggested by the fact that most acute pain may be entirely relieved by an anodyne which has no apparent influence on the general sensory functions of the brain.

No drug gives relief so quickly and so surely as opium or morphia. The hypodermic injection of the latter has to a large extent replaced its administration by the mouth, on account of the rapidity of its action and the slighter degree of gastric and intestinal disturbance which it causes. In epileptiform neuralgia Trousseau found no remedy comparable to opium, which he gave in doses rapidly increasing up to 300 grains of opium or sixty grains of sulphate of morphia a day. Morphia beneath the skin may be given in all forms of neuralgia. It is probable that in most cases it is better to inject it into the seat of pain, because it can then exert some influence on the terminal nerve endings, and, moreover, the injection has a slight counter-irritant influence. But its chief action is on the centre, and this is exerted equally wherever the injection is made; therefore, if, for any reason, local injection is undesirable, a distant convenient region, as the back of the forearm, will answer almost as well.

The dose of morphia should not at first be large, one-tenth, one-eighth, or one-sixth of a grain, according to the severity of the pain. It is unsafe to commence with a large quantity; death has in more than one instance resulted from an injection of one-fourth of a grain, and, still more frequently, alarming symptoms have been produced. It is remarkable, however, in some cases of neuralgia, how little effect beyond the relief of the pain morphia produces. The addition of atropine (in amount about one-twentieth of the morphia used) will often prevent nausea and giddiness and increase the influence of the drug. Caution should be used to prevent the dangerous "morphinism," most cases of which have been set up by the use of this drug for the relief of neuralgia. The hypodermic

syringe should never be placed in the hands of the patient. In cocaine, however, we have an agent that can often replace morphia. A local injection of from half a grain to a grain often gives great relief to the pain, apparently by arresting all impressions from the periphery, which may have a great influence in keeping up the morbid state of the centre, even in cases in which there is no peripheral irritation, and still more when there is. It has little or no central influence, and has, therefore, less permanent effect than morphia. Repeated injection may, however, lead to recovery, if there is peripheral irritation, or a spontaneous tendency to the subsidence of the morbid process in the centre.

Belladonna may be given by the mouth (one-sixth to one-third of a grain of the extract), or atropine by the skin ($\frac{1}{120}$ to $\frac{1}{60}$ of a grain). Occasionally these give marked relief to the pain, and when this is the case they are said to produce a more permanent effect than morphia (Hunter, Anstie, Vanlair). The unpleasant dryness of the throat which is produced by full doses sometimes constitutes a difficulty in the use of these drugs. It is often convenient to alternate injections of atropine with those of morphia in cases in which there is danger of the establishment of the morphia habit.

Aconite and gelsemium are said to have a special action on the fifth nerve, and are frequently useful in trigeminal neuralgias. Aconite is the more powerful of the two, but often causes nausea and unpleasant symptoms. In slight cases, especially those which depend on dental caries, gelsemium is often useful; fifteen minims of the tincture may be frequently repeated. Aconitia may also be given in doses of $\frac{1}{250}$ to $\frac{1}{100}$ of a grain, but it is a somewhat dangerous remedy to employ hypodermically. The alkaloid of gelsemium (gelsemia), will also be found useful for subcutaneous use; the dose is $\frac{1}{60}$ to $\frac{1}{30}$ of a grain. Cimicifuga is often very useful for neuralgia associated with rheumatism, either alone or combined with Indian hemp. In gouty subjects, lithia may also be given with advantage combined with cimicifuga.

Indian hemp is another remedy of great value in certain forms of neuralgia, especially those in which the pains are sudden, brief, and sharp, without the violence which marks true epileptiform neuralgia. It is often very useful continuously administered between the intervals; a quarter to half

or even one grain three times a day. Piscidia erythrina has been recommended when pain is not severe; in moderate doses (ʒ ss. of the liquid extract) it leaves no unpleasant after-effects. Chloral has little influence over pain, but croton-chloral (butyl-chloral) is sometimes useful, especially in neuralgias of the fifth. It is usually given in doses of five grains, but, as Ringer and others have shown, the dose may often be increased (up to twenty grains) with advantage. Combinations of nerve tonics and sedatives are generally necessary, but as the sedative has to be continuously given, its dose must, of course, be moderate. I have found, for instance, in neuralgia of the fifth, the combination of arsenic, quinine, and Indian hemp of great service.

Neuralgic pain is often relieved by local treatment, which is, for the most part, of two kinds, irritant and sedative; and some remedies combine the two. Counter-irritation sometimes gives great relief to neuralgia, but the mode in which it acts is uncertain. Hypothetically, it acts through the vessels, but this influence is more intelligible in neuritis than in neuralgia. It may also exert an inhibitory influence, or may change the mode of action of the nerve centre; just as a blister around the limb will stop the aura of epilepsy. Either blisters, or sinapisms, or the actual cautery may be employed. Flying blisters may be applied over the tender spots, or by the side of the spine. Anstie believed that the latter yield the best results. Chloroform has been injected beneath the skin, fifteen or thirty minims, but, although it often gives some relief, it may cause a troublesome slough. In trigeminal neuralgia the blister may be applied beneath the occiput or behind the ear. The actual cautery is of most service in spinal neuralgias. The gas cautery, or galvanic wire, or thermic hammer is the form generally used. The local injection of carbolic acid or of osmic acid has lately been recommended; they have been employed chiefly in trigeminal neuralgia and in sciatica. (Merces, "Lancet," 1885, No. 2; Jacoby, "Trans. Am. Neurolog. Ass.," 1885, p. 11; Schapiro, "Petersburg Med. Wochenschrift," 1885.) One or two drops of a one-per-cent solution of osmic acid (in water and glycerin) have been injected at a time. It is said to give immediate relief to the pain; some cases are reported as being cured after about a dozen injections. It does not appear to be entirely free from

danger; Jacoby observed palsy of the radial nerve to follow an injection into the arm, as it occasionally follows an injection of ether.

Acupuncture, the introduction of needles into the painful part, is a method introduced from China and Japan, the value of which is not great. If, as in the East, the needles are left in for some hours or a day, they cause much pain. Aquapuncture has also been employed; it consists in the injection of pure water into or beneath the skin. Originally, it was introduced into the skin, or between the true skin and the epidermis, under considerable pressure, so as to force the water to separate the tissues and make spaces for itself. This method gives much pain and does little good. The injection of water beneath the skin is innocuous and occasionally relieves a slight pain.

Of external applications that are at once irritant and sedative chloroform is the most important, pure, or as the linimentum chloroformi (B.P.), or a dilution of one part of chloroform to six of lin. saponis. Next in value are the ointments of veratria and aconitia. They should be rubbed in until tingling is produced, followed by numbness. The milder tincture of aconite may also be painted on the part two or three times a day, avoiding any sores. Camphor-chloral (camphor and chloral rubbed up together in equal parts, so as to liquefy) may also be applied on lint covered with oil silk. Both this and chloral will blister if left on too long. Menthol is also a useful application to the skin in the slighter forms of neuralgia. It causes a singular sensation of tingling and coldness, and for the time lessens the pain. It may be employed pure in the solid form, or rubbed up with chloral, or with spirit or glycerin. Preparations of opium (usually oleate of morphia) are of very little value as external applications. Belladonna is of greater service if employed sufficiently strong; the extract, diluted with one, two, or three parts of glycerin or vaselin, may be smeared over the skin two or three times a day. The oleate of atropia (five-per-cent solution in oil) may be used in the same way. Lanolin has been lately introduced as a better vehicle for external sedatives. These local applications apparently act by lessening the ordinary stimuli which pass along the nerves to the centre, and some of these agents substitute for the ordinary stimulation

a different form, which apparently lessens instead of increasing the morbid action of the centre.

Among external applications that of simple cold or warmth should be mentioned. Each occasionally gives temporary relief if applied continuously to the seat of the pain. In most cases heat is the safer and more effectual. Considerable heat is often little felt at the focus of most intense pain. If there is the least suspicion of active neuritis heat must be applied to the distribution of the nerve with extreme caution, or disastrous consequences may ensue. A warm douche through the nose has been strongly recommended by Seeligmüller in cases of supra-orbital pain, supposed to be produced in the frontal sinuses even when of malarial origin. Ether spray to the spine has also been recommended in various forms of neuralgia. Cold to the painful part is rarely useful, except in the case of neuralgia of the testes, where refrigeration can be more completely effected than elsewhere.

Electricity, properly employed, is an agent of considerable value in the treatment of neuralgia. It may be used in two ways: (1) A strong current, causing, for the time, considerable pain, will occasionally remove the neuralgia at once, no doubt by altering the form of central action, just as does a counter-irritant. The effect is probably only produced in cases of idiopathic neuralgia of slight degree. In some cases of this class, and invariably in neuritis, a strong application renders the pain worse, and the risk of this effect must be borne in mind. The forms in which it is most likely to be successful are recent cases of hysterical neuralgia, especially when the pain is seated in the joints. To produce this energeting effect either faradaism or voltaism may be used. (2) A weak current may be used for its sedative action on the nerves. Either form of electricity may be employed, but the action of the two is essentially different. The voltaic current is the most frequently useful. Authorities are divided on the best method of applying it, but the majority are of opinion (and with this my own experience agrees), that it is best to neglect the direction of the current, and to place the positive pole near the seat of the pain, and the negative in some indifferent situation. If there is reason to believe that the neuralgia is central, the positive pole may be placed, during part of the application, as near as may be to the central termination of the nerve,

and, for another part of the time, on the seat of the pain. The strength should be from two or five milliampères, but if the battery is not provided with a galvanometer, the number of cells must be regulated according to its effect on the sensory nerves, so as to cause a slight tingling or burning sensation, not actual pain. The number of cells to be used will vary in different parts, chiefly in consequence of the varying sensibility and resistance of the skin. The patient's sensations are, indeed, often the best guide. For the face, three, and elsewhere, five cells may be employed to begin with, and the current gradually increased until it can just be felt. All sudden variations in strength should be avoided. A well-wetted sponge should be employed as the electrode, and this should be very gently and gradually applied, and very gently removed when the strength is altered. If the faradaic current is employed as a sedative, it must be extremely weak (so as to be just felt, but to occasion no pain) and rapidly interrupted. The rapidly recurring slight stimulation of the nerves produces after a time a sedative effect, at first slightly increasing, but after a few minutes distinctly relieving, the pain. Its action is analogous to, and quite as effectual as, the mechanical percussors recently introduced.

The surgical treatment of neuralgia comprehends the division, excision, and stretching of nerves, and the ligature of arteries. It is a very large subject, and for detailed particulars the reader is referred to works on surgery. Neurotomy has been frequently adopted in cases of severe, old-standing neuralgia, especially in the branches of the fifth nerve. Sometimes it is successful, more often it fails. Temporary relief may be given, and may continue for some months, but is too often succeeded by a return of the pain in all its old severity. This has been ascribed to the union of the divided ends, and to prevent this the excision of a certain length of the nerve, or the deviation of the extremities has been recommended. In cases of neuralgia of the second division of the fifth, the excision of Meckel's ganglion has sometimes given better results than simple division of the nerve. (See a paper by Dr. Chavasse, of Birmingham [Royal Medical and Chirurgical Society, meeting of Feb. 20th, 1884, and discussion thereon]). It is probable, however, that the transient relief is often due to the influence of the surgical irritation on the centre, which soon

passes off. (Neuralgia will sometimes disappear for a time under strong mental influence. Le Fort, for instance, mentions that a most obstinate lingual neuralgia ceased during the siege of Paris, and returned when the anxious time was over.) In central neuralgia, or organic lesions high up the nerve (as, for instance, in the post-herpetic form), the operation usually fails. It is only to be thought of in cases in which other means have been fairly tried without success, and in which all the branches to which the pain is referred can be divided; it is said that the operation is more likely to be successful if pressure on the nerve relieves the pain. The operation is inadmissible if the nerve is one paralysis of which involves grave consequences, as, for instance, in the case of the sciatic, division of which causes permanent palsy and atrophy of a large part of the leg. These consequences might be deliberately chosen if it were certain that the pain would be exchanged for them, but, unfortunately, relief is too uncertain to make it justifiable to run the risk of adding physical disability to undiminished suffering. The same consideration (and the fact that it can do no more than neurotomy) precludes amputation, except, perhaps, in rare cases in which the pain starts from the extremity of a finger or toe, and local tenderness, etc., make it probable that a cause of nerve irritation exists. (It has been proposed to tear the nerve in two, by making traction on the central portion. The alleged advantage is that the nerve separates nearer the centre than it can be divided. But it always tears within a centimetre of the place where it is seized; the advantage is probably imaginary, and the method is not without danger [see a discussion at the Paris Société de Chirurgie, Dec. 6th, 1882]).

Fortunately, in nerve stretching we have an operation which produces some of the effects of neurotomy, arresting for a time the conducting function, and effecting what may be called an alterative stimulation of the nerve. Its influence is less lasting, but so are also its inconvenient effects, and, as it is sometimes successful, it should generally precede division of the nerve. It has been recommended that strong traction should be made on the central end, but it is doubtful whether this has much influence, and, in the case of the fifth nerve, it is somewhat perilous. (There is evidence that too vigorous stretching of the second division of the fifth nerve has caused

destructive inflammation of the eyeball, probably by the mechanism of inflammation of the Gasserian ganglion [Nicaise and Tillaux, Soc. Clin. de Paris, March 9th, 1882]). Recent literature abounds with records of cases of apparent cure from nerve stretching, even when the neuralgia seemed to be central. For instance, a severe intercostal neuralgia of twenty years' duration is said to have been cured by stretching the terminal branches of two or three of the nerves (Nussbaum, "Aerzt. Int.-Bl.," 1878, No. 53). Intensely severe neuralgia of the third division of the fifth—the pain being in the ear, temple, lower jaw, and tongue—of five years' duration, is said to have been cured by a single stretching of the lingual nerve in the tongue (Le Dentu, "L'Un. Méd.," 1881, vol. II., p. 766). In this and many other cases the pain did not immediately cease, but gradually subsided in the course of one or two weeks. In some rare cases, in which the sheath of an accessible nerve can be felt to be greatly thickened, the nerve has been exposed, the sheath opened carefully, and the nerve for some distance separated from it. This operation has been successful in arresting the pain, especially in traumatic cases. (An instance is recorded by Le Fort, "Soc. de Chir.," July 26th, 1882, in which the median was surrounded by a dense sheath of connective tissue, due to inflammation produced by a gunshot wound.)

S. MED. LIBRARY OCT 5 1898

Ligature of arteries has been confined to that of the carotid, as a last resort in cases of neuralgia of the fifth nerve. All that can be said to justify so dangerous an operation is that it has sometimes, but very rarely, been successful. Compression of the carotid occasionally cuts short an attack of pain, and the repetition of this treatment, continued for a long time, has even produced permanent alleviation.

An attempt to estimate the true position of the surgical treatment of neuralgia is unfortunately beset with almost insurmountable difficulties, due to the fact that after many operations the pain has ceased, or been much lessened, for a time, but has afterward returned, and the cases have been published before sufficient time has elapsed to permit an opinion to be formed of the permanence of the effect. Moreover, the numerous cases in which there has not been even temporary relief are seldom published. In some instances, operation after operation has been submitted to by the patient under the urgent

compulsion of continued suffering, and the records of many cases illustrate very strikingly the need for caution in drawing any inferences from transient relief. (The following cases are illustrations of this. In one instance, a man, at the age of forty-five, had his first attack of neuralgia in the third division of the fifth nerve. Four teeth were extracted from the lower jaw and then one from the upper, and, as the pain ceased for three weeks, the case was published as cured. The pain returning, resection of the alveolar process was performed; the pain ceased for five months, and the case was again published as cured. After a relapse, the inferior alveolar nerve was excised, and freedom for some time was followed by a return, for which the carotid artery was tied, but the effect of even this was not permanent [J. C. Hutchinson, "Am. Med. News," 1885, p. 395]. The same author relates a case of neuralgia of the fifth, in which the following operations were performed without success: an incision through the skin above the ear, division of the supra-orbital and infra-orbital nerves, excision of half an inch of the supra-orbital, ligature of the carotid, destruction of the nerve in the infra-orbital canal. In a case, recorded by Schupper, of neuralgia of the fifth with reflex spasm in the face, the first procedure was scarification of the outer and inner surfaces of the upper jaw; this failing, the surgeon excised the infra-orbital nerve; this likewise failing, he excised the superior maxillary nerve in the spheno-maxillary fossa; there being no relief, he removed the peripheral segment of the same nerve from the base of the orbit. The pain then migrated to the third division of the fifth; the inferior maxillary nerve was therefore excised. An interval of freedom was followed by a return of the pain, and the common carotid was tied. The pain continuing, the facial nerve was divided, and on account of severe hemorrhage, the external carotid and temporal arteries were tied, and the patient is said, at last, to have been "cured.")

Visceral Neuralgias.

The internal viscera of the thorax and abdomen are sometimes the seat of neuralgic pain. Such neuralgias are described in full in the works that deal with the diseases of these organs, and this arrangement is convenient, since the princi-

ples of diagnosis involve a differential discussion of the symptoms of organic diseases of these organs which would be out of place in the present work. A brief outline of the general facts may, however, be given here.

Most of the organs in which these neuralgias are felt receive their chief nervous supply from the sympathetic system. Little sensation attends their normal function. Although it cannot be doubted that afferent impressions are constantly passing to the cerebro-spinal centres, these fail to affect consciousness under normal circumstances. But repeated attention may vastly increase the sensitiveness of the perceptive centres to such impressions, and from such increase arises a large amount of the discomfort of those patients who are termed "hypochondriacs"—correctly, in so far as the organs which lie below the rib-cartilages disturb their conscious life. But the sensation from the viscera may also amount to actual pain, in consequence of the afferent impressions being abnormal, owing to organic disease and varied functional disturbance. Pain may also be felt apart from either of these causes, and such pain is called "visceral neuralgia." Often we cannot tell to what extent it is the result of a local abnormal condition of the nerves of the organs, and how far it is central. It is probably local in causation to a larger extent than in the case of the cerebro-spinal forms of neuralgia, but the same general pathological laws doubtless underlie the two. The difficulty of investigation depends not only on the fact that the organs are concealed from direct examination, but also on the circumstance that they receive their innervation from two sources, from the sympathetic, and from the cerebro-spinal centre by the pneumogastric and spinal branches, and we do not know what share these sets of nerves respectively take in the production of visceral pain. There is, moreover, reason to believe that not only the nerves in the viscera but also the nervous plexuses outside them may be the seat of neuralgia. The difficulties of investigation are greatly increased by the fact that pain may be the only symptom of organic and of functional disease, and it is certain that many examples of such disease have been included among the visceral neuralgias by some writers on the subject.

Most forms of visceral neuralgia are more frequent in females than in males, and may be produced by the same in-

herited tendency that causes the cerebro-spinal forms. Their general causes are also, for the most part, similar. In women, anæmia and hysteria are especially prominent. They may also result from causes that have a local action, and this fact adds not a little to the obscurity of their diagnosis. The essential symptom is pain, which varies greatly in character, sometimes dull and diffuse, sometimes sharp, circumscribed, lancinating or burning. It usually presents paroxysmal exacerbations, and is sometimes actually intermitting. I have known paroxysms of gastric pain to alternate with headache. The exacerbations may be apparently spontaneous, or may be produced by various stimuli, especially by those concerned in the functional activity of the organ. We ought not, however, to include among the neuralgias (as some have done), cases in which pain is confined to periods of functional activity. It is doubtful whether, in such cases, the affection is ever a pure neuralgia.

The diagnosis of visceral neuralgias is as difficult as it is important. Pain is the common, and may be the only, expression of various and very different maladies. The first and chief element in diagnosis is the exclusion of organic disease, by every method of investigation that can be made available. If functional disturbance exists, the affection can only be regarded as neuralgic when the pain is not related to the disturbance of function either in time or in degree.

The treatment of visceral neuralgias must be based on the same general principles as that of the cerebro-spinal forms. It consists in the removal of causes, general tonic treatment, abundant rest, and the use of sedatives. The mode of employment of the latter differs in each case, and in each, also, special measures are necessary, related to the function of the organ concerned.

Special Forms.—Of the intra-thoracic organs, the lungs do not appear to be the seat of neuralgia, although the pain of "pleurodynia," already described, is probably due to an affection of the pleural nerves. The only important cardiac affection of this class is the disease known as "angina pectoris." Severe nerve pain is a prominent symptom of the disorder, but its peculiar and special characters and associations prevent its inclusion among the forms of pure neuralgia.

The most important visceral neuralgias are those of the

abdominal organs. They are usually local and well-defined, but occasionally abdominal neuralgic pain varies in its seat, and is felt now on one side, now on another. That of the stomach (gastralgia, gastrodynia) is one of the best-marked forms. It is frequent in anæmia and hysteria, and a special form constitutes the gastric crises of ataxy. The pain is felt at the epigastric region, and, like most gastric pains, passes through to the back. Pressure does not usually increase it, often, indeed, it relieves the more intense suffering. There is usually a constant dull pain, with more acute exacerbations. It may be most intense when the stomach is empty, and may be relieved by food; it is then possibly due to the morbid action of the nerves or centres from which, in health, the sensation of hunger arises. Or the pain may be increased by food, and in such cases food is often vomited as soon as it is taken. Appetite may be absent, lessened, increased, or perverted. The relation of the condition of appetite to the effect of food in relieving or increasing the pain deserves further study.

The stomach is readily influenced by sedatives, and their administration by the mouth forms an important part of the treatment. Other agents are sometimes useful. In the form that is relieved by food, oxide of silver is often of great service. When there is anæmia, the combination of subcarbonate of bismuth and saccharated carbonate of iron, given before food, is frequently effective. No sedative, however, is on the whole so useful as cocaine, of which a grain may be given when the pain comes on. By repeatedly relieving the pain in this manner, the disease is often cured, apparently by the rest thus given to the nerves of the mucous membrane.

The frequency of intestinal neuralgia (enteralgia) has probably been exaggerated. We are not justified in regarding as enteralgia either vague abdominal pains, which are not increased by peristaltic action, or pain that occurs only when the intestines are in energetic action, or in which there is conspicuous disturbance of the mucous membrane. If these are excluded, the cases of enteralgia become extremely rare, and the history of the affection has yet to be worked out. There is, however, one part of the intestine which is, undoubtedly, the seat of neuralgic pain—the lower part of the rectum. This form occurs in both sexes, as a deeply-seated pain above the

anus and coccyx, more or less constant, but with severe exacerbations, apparently due to spasm. It is usually effectually relieved by suppositories. In some cases, indeed, it is probably a primary spasm, and I have known it produced in a child by the use of senna as an aperient.

Of neuralgia of the liver, hepatalgia, much the same may be said as of enteralgia. Deep-seated pain is occasionally felt in the position of the organ, sometimes diffuse and dull, sometimes sharp and lancinating, but the cases in which we can be sure that it is truly neuralgic are extremely few. In some cases such pain may be due to disturbed function of the organ. Disturbance of function has been regarded by some writers as a consequence of neuralgia. It cannot be denied that such nerve pain may cause such reflex derangement of function, just as it causes vaso-motor disturbance in other situations. But the frequency with which pain is the first evidence of disease outside the nervous system, which afterward causes other symptoms, renders it necessary to exercise great caution in regarding such cases as primarily neuralgic.

The existence of neuralgia of the spleen is not well established, but occasionally, especially in cases of hysteria, there is deeply-seated pain and tenderness in the position of the organ, apparently situated in it or in the nerve plexuses in its vicinity.

The kidney, and apparently the ureter, may be in rare cases the seat of the neuralgic crises of tabes, analogous to the more frequent gastric crises, but the occurrence of primary nephralgia is a still more rare event, and can hardly be regarded as proved. Many cases of supposed renal neuralgia have certainly been due to the passage of a calculus. I have, however, met with one case in which paroxysms of pain in the renal region, had occurred at times during forty years without any indication of a calculus, and it seemed on the whole probable that the pain was of nervous origin.

Neuralgic pain which cannot be referred to any organ is sometimes felt within the abdomen. Such pain is diffuse, varies in intensity, is not increased by pressure, and is not related in time to the functional activity of the organs, or in degree to their disturbance. It is generally central in position, but may be felt above or below the umbilicus, apparently seated in the sympathetic nerves, but has not such a relation to their plexuses as to permit definite localization.

The female generative organs are frequent seats of pain and tenderness. Tenderness in the region of the ovaries is extremely common in hysteria and conditions of nervous weakness; sometimes there is much spontaneous pain in this situation, deeply seated and aching or burning in character. This pain may exist when no evidence of organic change in the ovary can be detected, and appears then to be a pure neuralgia; but in most cases the tenderness is extensive, and exists in the vicinity of the ovary as well as in the organ itself, so that it is probably due, at least in part, to the abundant nerve plexuses with which the ovary is surrounded. The uterus is also the seat of spontaneous pain and of tenderness, apart from organic disease or of displacement. These symptoms may occur in the young and also in later life, even when the involution of the organ is complete. They are often associated with neuralgic pains elsewhere—in the ovaries, and especially in the spine, including the sacrum. By some writers the pains of menstruation, when no organic cause for them can be discovered, are regarded as neuralgic.

Of the male organs of generation, that which is most frequently the seat of neuralgic pain is the testicle, but even here such pain is comparatively rare. It is important to remember, however, that inflammations of the urinary passages are not uncommon causes of neuralgia seated in the adjacent cerebro-spinal nerves.

THE PROGNOSIS

OF

DISEASES OF THE HEART.

BY

PROF. E. LEYDEN, BERLIN, GERMANY.

Professor of Int. Medicine, Special Pathology, and Therapeutics, University of Berlin.

THE PROGNOSIS OF DISEASES OF THE HEART.

GENTLEMEN:—We have heard, in the course of time, quite a large number of papers in this society upon the subject of heart disease. You will remember the addresses of our present chairman, my honored friend, Dr. Fraentzel. I have also made several additions to the subject; likewise Drs. A. Fraenkel, Litten, Lewinsky, and others. However, those questions which are especially important in practice were not discussed hitherto, at least, in an exhaustive manner, and this may offer a proof that it is indeed quite difficult to enter in detail into these questions.

Among those questions in the domain of diseases of the heart which must especially interest the physician in his practice, belongs undoubtedly that of the prognosis which these diseases warrant. If under the term prognosis we understand not alone the question whether a definite lesion of the heart is curable or not, or whether a patient suffering from heart disease who consults us, is to be regarded as a curable patient or not, it appears to be an equally important, difficult, and extensive subject.

The prognosis of a definite disease, or a definite patient, then embraces everything that can be predicted with more or less probability with regard to the further course. Not alone whether a cure can be effected or not, but also whether, and how long, life can be maintained; what measure of well-being or of suffering; what measure of capability and enjoyment of life stands at the disposal of the patient; finally, what conditions and accidents disturb and aggravate the course of the disease; what conditions may make it run a more favorable

course. It is, then, the object of medical science to prevent, as far as possible, the disturbances and dangers, and to utilize the favorable influences (including drugs) at the proper time for the benefit of the patient. In such a sense, the prognosis includes the entire judgment of the physician with regard to the significance of the case. It presupposes the most accurate knowledge, both of the form of disease and of the special conditions of the patient. It is the real quintessence of his medical knowledge. It has often been asked: what is the most marked service of the physician, and by what can we most easily recognize a physician of note? Some have said, he is recognized by the diagnosis; others, and I am inclined to agree in their opinion, by the prognosis. In the end, however, the best physician will be recognized by his results, inasmuch as he who can aid the patient most, and in the most difficult cases, is undoubtedly the best physician. But this success is determined with the greatest difficulty, and can only be measured by an objective measure with equal difficulty. We are, therefore, justified in regarding the prognosis and correct judgment of a case of disease as the best criterion. As a matter of course, this cannot be separated from the other functions of the physician. When it is said in various quarters in literature, that prognosis is a part of the diagnosis, this is entirely correct, as it is a part of the recognition of the case of disease. But it is also a part of treatment, because it shows us where the dangers for the patient lie, and where we must apply the therapeutic lever.

I beg your indulgence in prefixing these general remarks, partly in order to make my own principles clear, but partly, also, to indicate that I regard the present moment as well fitted to bring such an important practical question into this assemblage for discussion. The special reason for me, resides in the fact that here in Berlin, especially since the time of L. Traube, the study of heart diseases has been pursued with special vigor. I may, therefore, hope that this assemblage will furnish rich additions to this subject, which will be fitted to complete my views. The second reason is the fact that at the present time the clinic of heart diseases is found, if not in a new epoch, at least, in a condition of promising progress. The advances in this field have been furnished, on the one hand, from the fact that our clinic has emancipated itself

from the purely physical direction, without underestimating it, and that we have secured a broader standpoint in the diagnosis and judgment of diseases of the heart. While former medicine, prior to the time of physical examination, had a proportionately slight knowledge of diseases of the heart, this knowledge flourished in an extremely luxuriant manner as the result of physical diagnosis. But this striking progress, and this imposing diagnostic knowledge led to one-sidedness in certain respects, in so far as it was over-estimated and regarded as almost the sole object of medical judgment. I may say that our clinic has won a proper standpoint, and that we utilize not alone physical diagnosis, but also the observation of all the local and general symptoms in the diagnosis and judgment of diseases of the heart. In addition, in the course of time, our knowledge has been completed by many forms of disease which had hitherto been little studied. Furthermore, it is a favorable circumstance that we have secured new points of departure in the treatment of diseases of the heart. I refer here less to medicinal treatment which has also been enriched, than to the dietetic and mechanical modes of treatment, which have been the subject of numerous discussions for some years (Oertel, Zander, Th. Schott). Without a doubt, we have thus received new and weighty stimuli in treatment, particularly from Oertel, which I acknowledge with so much more readiness, as they have also opened a further fruitful field to pharmacological medicinal treatment. It is demanded of these methods that they effect more than we have hitherto been able to effect in the treatment of diseases of the heart. It is true, however, that this demand has met with much opposition, and the difficulty of deciding upon therapeutic results has also manifested itself here. If we now attempt, from the present standpoint of experience, to present the prognosis of cardiac affections, we will be able at least to determine approximately to what limits our therapeutic efforts have gone, and we can determine whether, and where, we can exceed these limits by new measures and methods. At the same time, I am well aware of the difficulties of such a discussion, and must at the start crave your indulgence. The difficulty resides in the object as well as in the form of treatment. It is not easy to discuss the prognosis from a general standpoint. It would be easier to

investigate the prognosis of each individual disease, but in this way we would not obtain general results. Great difficulties also reside in the nature of the subject. Even if I would assume to possess more than the usual experience in this field on account of my clinical and consultation practice of many years, it is hardly possible to define with precision the boundaries of our knowledge and powers. These difficulties are increased by the fact that the study of the literature shows very few preliminary works on our special theme. I have found the majority of them in the English literature, and here, also, the practical side of English medicine is manifested. In the medical societies of London lectures and discussions on this subject have been held several times. Among others, I may mention Broadbent ("Prognosis in Heart Diseases," "Brit. Med. Journal," 1884). Excellent remarks are also found in the practical text-books of Williams, Walsh, and Bristow. About two years ago, Sir Andrew Clark read before the British Medical Association a notable paper, "More than 100 Cases of Diseases of the Heart which Lasted demonstrably more than Five Years without producing Grave Symptoms." This lecture has perhaps not been sufficiently noticed in the German reviewing journals. Dr. Litten has furnished a very excellent review in the "Virchow-Hirsch Jahresbericht." In America, Prof. Da Costa is especially to be mentioned. In Germany I do not find these practical questions discussed in the text-books with the precision and exhaustiveness which appear desirable. Among the writers of smaller articles are mentioned G. Mayer, "Ueber heilbare Formen chronischer Herzleiden einschliesslich der Syphilis des Herzens." Aix, 1881. Also Dr. Levinsky who read in this society a paper "Ueber die Prognose der Herzklappenfehler, namentlich die Heilbarkeit der Aorten-Insufficienz." I also mention a discussion between Prof. Riegel and Von Dusch, which touched upon several points in the prognosis of diseases of the heart.

The literature is, therefore, not very rich in this respect, and I hope that perhaps by articles from our society this hiatus in German literature may be filled in a measure.

Until not very long ago, the prognosis of diseases of the heart was regarded as very grave. Every such patient was considered incurable, a candidate for death, who could suddenly fall dead at any moment; *hæret lateri letalis arundo.*

This sentence was placed by Corvisart at the head of his celebrated work on diseases of the heart, and this pessimistic opinion for a long time weighed heavily upon these patients. They were regarded as condemned to death, as a *noli me tangere.* No one dared to touch the fatal arrow. It was taken for granted that we must wait passively until death released the patient. It was only by degrees, and especially in very recent times, that this doleful view gave way to a more hopeful and cheerful one. We have not attempted to cure the valvular disease, but rather to secure for the patient a life free from suffering, or at least endurable, and to prolong existence to a considerable extent. By degrees we have become convinced that many patients with disease of the heart do not experience so much suffering, that they may lead a life full of activity and enjoyment, and that they may enjoy this for many years. Indeed, I find that the laity by no means have such a pessimistic opinion of diseases of the heart as physicians themselves. I have often convinced myself, when I delayed in telling a patient that he suffered from heart disease, that he was not so much terrified as I had apprehended. An important reason which appeared to justify the pessimistic prognosis of cardiac disease lies in the experience that such patients who are still in a tolerable condition not infrequently die suddenly. It has therefore been inferred that they are not secure of life for a single moment, but a more careful examination, such as has been made particularly by English physicians, has shown that in the majority of chronic diseases of the heart sudden death is a rare event. There are only two forms of these diseases in which sudden death occurs more frequently, namely, insufficiency of the aortic valves ("Aortic Regurgitation" as the English prefer to term it; also "Aortic Reflux") and true angina pectoris (Heberdenii). For both diseases we must formulate the prognostic dictum that such patients are not sure of life for a single day, but that life may be preserved for years without any definite limits. With regard to both, I myself have made numerous observations with regard to sudden death, and particularly in the case of aortic insufficiency my experience both in hospital and private practice is unfortunately very large. I have seen the sudden death occur very often in patients who felt quite well, especially those who had passed through a period of severe disturbances

of compensation, and appeared to pass gradually into a promising convalescence. I may add, however, that so far as my memory serves, these cases were always attended with severe sequelæ, especially with pronounced dilatation of the left ventricle, yet the fatal termination occurred unexpectedly in a majority of these cases. In the remaining forms of valvular affections sudden death occurs with comparative infrequency, in mitral lesions in only about two per cent of the cases; that is, so rarely that the physician may neglect this unfortunate termination in the prognosis. We are not able to guarantee life even to perfectly healthy people. Unexpected accidents and sudden deaths occur in health and other cases of disease, and not much more frequently in the majority of cases of heart disease. It would be wrong to call the attention of the patient to this remote possibility, since we cannot prevent it. We should permit the patient the greatest amount of freedom from care and not hang over him the sword of Damocles, which stamps him as a condemned individual deprived of every enjoyment in life.

I may also mention another series of pathological conditions of the heart which sometimes give rise to sudden death. For example, anæmic (degenerative) fatty heart. Also the later stages and beginning of convalescence of acute diseases; sudden death may also be produced by strain or great emotional excitement, but these accidents can be hardly called such as affect especially patients suffering from heart disease. They are rather general conditions, temporary in their nature, which have led to weakness of the heart. Moreover, even these accidents are so rare that they cannot be regarded in the prognosis. In general, it is very probable that heart death is the most frequent cause of sudden death (I do not refer to sudden death in diseases of the heart). It would be very interesting here to obtain from judicial autopsies statistics of sudden deaths. With the exception of certain definite forms, therefore, sudden death is not a frequent occurrence in diseases of the heart. The majority of these patients live for a number of years, and in no small number the duration of life is not shortened in a material manner.

I will now arrange the further remarks on the prognosis of diseases of the heart according to three standpoints: 1st, with regard to the general disposition and conditions of life;

2d, with regard to the special groups of diseases of the heart; 3d, with regard to individual physical or physiologico-pathological symptoms.

I. Under the first head I first mention the age of the patient. Apart from the definite form of the heart disease this has a prognostic significance, in so far as young children tolerate heart diseases very poorly, and succumb to them very readily. In later childhood, youth, and early manhood, on the other hand, the prognosis of heart disease is in general most favorable. This is owing chiefly to the fact that at an early age those forms of heart disease which furnish a relatively favorable prognosis predominate. At an advanced age, on the other hand, those forms are most frequent which entail a grave prognosis. The most frequent causes of heart disease of advanced age is arterio-sclerosis, which bears within it the decided tendency to a more or less progressive course. This progressive course may be very slow. Indeed, it may come to a standstill for a long time, but the fear of its rapid spread is present at all times. The arterio-sclerosis occasionally develops very early in the heart and large vessels, even in the thirties—cases which for this very reason are explained with difficulty. On the other hand, I desire to emphasize the fact that the compensation of valvular lesions occurs as readily at an advanced age as in youth. With advancing age the heart increases in size and functionating power. In fact, the heart is the only organ whose relative mass increases with age. In young people, the organ is perhaps more easily exhausted than in older individuals. The sudden death in insufficiency of the aortic valves occurs at least as frequently in young people as it does in the old.

The second general etiological factor to which I desire to call your attention is sex, which must be considered with regard to the prognosis of heart disease, in so far as the female sex in general admits of a decidedly better prognosis than the male. I find this relation so sharply defined both in hospital and in private practice, that I desire to express this statement with decision. In hospital practice, it is true, the difference in sex does not appear so sharply, because women in the lower classes live in almost the same manner as the men, and are subjected to the same hard labor, emotional excitement, and in part, excesses. The reason for this difference in the prog-

nosis between male and female patients with diseases of the heart resides in the fact that the female sex is less exposed to the causes which produce severe cardiac affections. Among these, I include first, arterio-sclerosis, which in the female sex, even in the working classes, is much rarer than in males, and in families in which it is hereditary, attacks the female members to a slighter extent. According to my experience also, true angina pectoris permits in general a much better prognosis in the female than in the male. Furthermore, hard work, excessive bodily strain, and alcoholic excesses are much less frequent in females. Moreover the feminine character is more suited to bear the discomforts of diseases of the heart and thus to ameliorate its course. Finally, experience teaches that women are more attacked by those lesions of the heart which permit a more favorable prognosis. Mitral lesions occur mainly in females; aortic insufficiency in males.

The third point in the general etiological conditions refers to the mode and conditions of life. As a general thing, patients of the wealthier classes tolerate heart disease better than those of the working classes, because the most dangerous foe of existing lesions of the heart is bodily work and strain. It leads to heart failure and disturbances of compensation. Very much depends, therefore, upon the condition and occupation. Existing heart disease is often aggravated by emotional excitement. The patient requires an existence which is free from care, good nourishment, and general care of the body. The character must also be considered, inasmuch as violent passions, excesses, and carelessness often injure the patient, and a careful, quiet, and temperate individual will tolerate such a disease much more easily and will succumb at a much later period.

Finally, the prognosis is also influenced by the manner in which patients react to drugs. Our remedies, even the principal one, digitalis, do not always possess a certain and constant effect. Their action is easily exhausted, and they finally fail to act. A patient in whom these remedies act promptly is undoubtedly in better case, particularly if the effect obtained lasts for a longer time. The more rapidly the action of digitalis diminishes and disappears, and the more often we are forced to resort to this remedy, the more the fear increases that its activity will diminish and cease. It is therefore one

of the most important and difficult tasks to administer digitalis economically, and it is in this very regard that the physician who is experienced in diseases of the heart will appear at his best. If at all possible, we should not give too large doses of the drug, and should not allow them to be continued too long (at most three bottles of 1.00); then the remedy should be discontinued from two to three weeks. In this way we will preserve most easily the efficiency of the digitalis and thus have at hand a remedy which will restore the disturbed compensation. If the digitalis fails to act, we are not entirely powerless, but the prognosis becomes so much more unfavorable, because the effect of the other analogous drugs and of the other methods is much more uncertain.

Before I proceed in my remarks I desire to demonstrate a heart. The case is apropos of my lecture in so far as we have to deal with a sudden death from insufficiency of the aortic valves. The patient was a coachman, æt. thirty-five years, admitted to hospital March 11th, 1889, and died suddenly on April 13th. He came to the First Medical Clinic in a miserable condition, with very notable disturbances on the part of the heart, severe dyspnœa, great weakness, restless nights, œdema of the lower limbs. Physical examination showed the signs of insufficiency of the aortic valves. With regard to the previous history, it was found that the patient had never suffered from articular rheumatism. As he belonged to the working classes I inquired with regard to overwork. The patient stated that he was not concerned directly with especially hard work, but that as a furniture-truck driver he was compelled repeatedly to help in carrying furniture. He himself did not attribute his disease to overwork, but said that it began with cough in January; a few days later he suffered from dyspnœal attacks which developed into signs of a severe lesion of the heart. It may be assumed that the patient, who evidently did not observe himself very closely, was not kept in bed by a simple cough, but that the cough was the expression of a cardiac lesion, and that the dyspnœal symptoms developed more strongly in the course of a few days. The patient does not mention any distinct cause, but it is noticeable that he dates his disease from a certain day. As this occurred after Christmas and New Year, it is, at least, not to be excluded, although not demonstrated with certainty, that

he experienced a sudden excessive strain, and the specimen rouses the suspicion that we have to deal with a rupture of the aortic valves as the result of strain. The patient entered with very severe symptoms. The condition was gradually improved. The pulse became more vigorous, the nights were quiet, and the patient began to recover. He asked to get up, but this was not permitted. He appeared to be doing well. He had been up five or six days, but had not yet left the room, when, one day after he had been out of bed for a few hours, he suddenly felt ill. He was rapidly put into bed, but falls forward, and is dead. Here, therefore, we have sudden death in insufficiency of the aortic valves, as I have unfortunately observed in many cases. I emphasize the fact that we have to deal here with a severe case. The specimen is interesting, from the fact that the aortic valves exhibit openings. In one place a paper roll as thick as a lead pencil has been carried through the base of the valve and this opening has not been enlarged post mortem to any noticeable extent. The heart has been kept in alcohol and therefore would be more apt to have shrunk. The third valve is also torn and covered with vegetations, which do not contain micrococci but are formed of deposited fibrin from the blood.

II. In continuing my review, I proceed to the second part of my prognostic considerations, and will discuss the heart diseases according to their diagnostic form. In this respect, I distinguish three groups: 1, the non-organic heart lesions or heart diseases; 2, the diseases of the heart muscles; and, 3, the valvular lesions.

1. When we examine a patient who complains of heart symptoms, or states that he has disease of the heart, it is our first duty to decide whether we have to deal with an organic affection or not. I use the term "organic" in the sense that a true anatomical lesion is present, while often valvular lesions alone are understood under this term. In my sense, however, the distinction is evidently more precise. We are to determine whether we have to deal with anatomical, that is, organic lesions of the heart, or with non-organic diseases, that is, with heart symptoms without true anatomical lesions. These non-organic diseases are also called nervous or neurasthenic or functional affections of the heart. Their separation from the organic is not entirely distinct and cannot be determined

definitely in every case, but in principle the differentiation is extremely important. Its significance resides in the fact that we can at once give a much better prognosis in a non-organic disease of the heart, while organic affections entail a serious, if not grave prognosis. This distinction is so much more important because the nervous affections of the heart occasionally appear with very violent and threatening symptoms which terrify the inexpert, but we can at once give a good prognosis as soon as we are certain of the diagnosis of a nervous disease. I must mention, however, that this rule has a few rare exceptions, that nervous affections of the heart, for example, from fright or grief, especially in older people, may be attended with serious sequelæ, and also that sudden deaths have been observed occasionally as the result of fear and terror. The certain diagnostic differentiation between an organic and a non-organic affection of the heart is by no means always easy. As a rule, we assume a non-organic disease when the most careful physical examination reveals no abnormality either on percussion or auscultation. This diagnosis is confirmed when we have to deal with young neurasthenic and especially female individuals. But even this conclusion, which is generally justified, must be restricted in two directions: in the first place, murmurs (anæmic, accidental) may occur in non-organic affections of the heart. They are almost always systolic and loudest at the apex. Even dilatation of the heart may occur, as is shown in the most striking way by Basedow's disease. On the other hand, profound organic diseases of the heart, particularly of the heart muscle, may exist, although no abnormality is demonstrable by physical examination. The diagnostic and prognostic judgment in this respect may be very difficult. It was formerly believed that a heart in which a murmur was heard is diseased, that one in which no abnormal physical signs are present, is healthy, but the continued investigation of the facts has shown that neither one nor the other conclusion holds in all cases. Angina pectoris is very important in this regard. I distinguish three principal forms: true angina pectoris (Heberdenii) which depends upon disease of the coronary arteries; next symptomatic angina pectoris, which appears occasionally in almost all forms of heart disease; and nervous or neurasthenic angina pectoris. The symptoms of the first and third forms may be

very similar. In both physical examination affords no positive findings, but their prognostic significance is extremely different. In such cases the chief factor in differentiation lies in the consideration of the general conditions. When elderly people, especially men, are attacked by angina pectoris, we have to deal probably with the organic form. In young, anæmic female individuals we assume, other things being equal, the nervous form. The diagnosis may be very difficult when, as I have seen on several occasions, nervous angina pectoris develops in old men, and it can then be obtained only after an accurate knowledge of the individual and of the course of the disease. I must also mention Basedow's disease, which illustrates the fact that nervous affections may also be attended with dilatation of the heart. In proportion to the violent symptoms this disease furnishes a relatively favorable prognosis, inasmuch as it may be not alone easily borne, but may also retrogress, and it is only in the highest grades in older people and in other complications, that life is directly threatened. Brief mention may be made of the nervous affections of the heart in diseases of the spinal cord, concerning which we have as yet but slight experience. I have described one form of angina pectoris in tabes dorsalis, which was attended with very severe symptoms; in one case it appeared to be connected with the fatal termination. Conditions of heart weakness and paralysis with a fatal termination have also been observed in progressive muscular atrophy and bulbar paralysis.

2. The second group are the diseases of the heart muscle. These form an extremely important part of the general diseases of the heart. Indeed, if we include conditions of the heart muscle with the valvular affections, we have now to discuss perhaps the most important part in the general doctrine of cardiac affections. As a matter of course, diseases of the heart muscle are diagnosed with much more difficulty because they are accessible only in small part to physical diagnosis. For a long time they were very much neglected or included in the general notion of myocarditis. At the present time, we know at least that very different affections are presented. A general prognosis of diseases of the heart muscle can therefore be made with extreme difficulty. The forms which can be diagnosed physically may be classified in three groups: 1st,

hypertrophy; 2d, dilatation, and, 3d, heart failure or insufficiency of the heart. These three forms are distinguished quite sharply physically, but they may be associated and pass into one another. I make the distinction only to add some prognostic remarks, first, in the direction of the danger which these changes in the heart muscle bring, and, secondly, in how far they are capable of improvement or recovery, especially when we take into consideration our new therapeutic methods. From this standpoint it may be said at the start that hypertrophy is no disease *per se*, and therefore cannot be the subject of treatment. The English writers, particularly Corrigan, have justly emphasized the fact that hypertrophy is not a disease, but on the contrary, the compensation of a disease; that it is not to be combated but to be assisted. But hypertrophy is the expression of severe disease which is situated outside of the heart, whether in the arterial system or in the kidneys. The more marked the hypertrophy the more marked is the causal affection, and the prognosis of hypertrophy of the heart will depend upon the degree and the importance of the disease which we presuppose as its cause. On the other hand, despite the existence of hypertrophy, a condition of exhaustion, heart weakness, or paralysis may set in attended with evident and rapid danger to life.

Dilatation of the heart is the most important with regard to prognosis. That it is to be distinguished from hypertrophy I may assume to be sufficiently well grounded, although the combination of hypertrophy and dilatation is a very usual one, and I do not wish to maintain that the distinction between the two is always easy. Yet this question is not very important diagnostically, because in that form of dilatation which is independent of hypertrophy, as a rule, severe heart symptoms, such as dyspnœal conditions or even circulatory disturbances, are present. In general, dilatation undoubtedly furnishes a more unfavorable prognosis than hypertrophy, because it is the expression of a condition of distention of the heart muscle, and such conditions result in weakness of the organ, which always entails danger.

The question now arises concerning prognosis: 1, whether every dilatation is co-ordinate with regard to prognosis, or whether differences exist; and, 2, whether we possses means, either with the aid of nature, or by our methods of treatment,

which will improve or cure the dilatation. The first question is to be answered by the statement that the causes and significance of dilatation of the heart present manifold differences. We must especially distinguish those dilatations which result from valvular lesions, from those which apparently appear independently. As is known to you, our esteemed Chairman, Dr. O. Fraentzel, has called the latter idiopathic dilatations, and has made them the subject of special study. Without doubt, these excellent investigations which have recently been presented to the medical public in a special article are sufficiently well known to you.

As regards the prognosis of dilatation of the heart in general, it must first be remarked that dilatation of the right half of the heart is of much less significance than that of the left ventricle. Dilatations of the heart which are manifested by enlargement of the area of cardiac dulness upon the sternum, and beyond it toward the right, and which are often associated with enlargement, distention, and dilatation of the cervical veins, may develop rapidly and may disappear with equal rapidity. They are apt to develop in all stases in the lesser circulation, in pneumonia, emphysema, pleural effusions, etc. In valvular lesions, especially mitral disease during an exacerbation, we also find them increase notably, and then retrogress. The right heart, especially the auricle, is as easily distended as a venous vessel and readily accommodates itself to a greater or less stasis of blood in the lungs. Without doubt, such a dilatation may disappear quickly, and is not very important in the prognosis of the cardiac affection. I will not fail to mention, however, that hypertrophy of the right ventricle, after it has lasted for some time, is not so apt to retrogress. This is manifested, not so much by increased dulness on the right side, as by the intensified apex beat (at the left border of the sternum).

In the left ventricle matters are entirely different. Its dilatations are of the greatest importance as regards the function of the heart. They threaten heart failure, and if they continue, endanger life. Nevertheless, it is undeniable that these dilatations do not always furnish a grave prognosis and that some cases may undergo retrogression and complete cure.

In my opinion this is true undoubtedly of the rapidly developing dilatations, especially after acute diseases. Traube

observed dilatation of the heart without endocarditis in the course of acute articular rheumatism, and I have repeated this observation several times. Analogous dilatations also occur after other infectious diseases, typhoid fever, measles, scarlatina, erysipelas, diphtheria. The mildest forms of this variety are recognized only by the audible galop-rhythm. Other cases reveal distinctly a displacement of the apex-beat to the left. These dilatations are capable of complete recovery even when they are attended with grave symptoms of heart weakness and tachycardia. I do not mean to imply that all such cases are curable, or free from danger. On the contrary, it is well known that, for example, the heart affection after diphtheria endangers life in the highest degree, and also the dilatation after scarlet fever, when it is the result of an intense nephritis.

In like manner the dilatations of the left ventricle which result from strain—with or without coincident valvular lesion—may also undergo retrogression. I have been able to convince myself repeatedly of this when patients with fresh strain of the heart were brought to the clinic. The apex-beat, which was pushed beyond the left mammary line, returned to the normal position as soon as the patient was improved by rest and suitable treatment; the difference occasionally amounted to 1 to 2 cm. I observed this affection particularly in two cases of heart strain which returned in the course of their disease (of several years' standing) repeatedly to the clinic with exacerbations of the disease. The first examination then showed displacement of the apex beat to the left beyond the formerly observed limits, and after a few days a return to its normal position.

Finally, the dilatation of Basedow's disease may also disappear.

The matter is somewhat more difficult in dilatations which have developed slowly and continued for a longer time, whether in the dilatation called idiopathic by Fraentzel, or in the dilatation of aortic insufficiency or chronic nephritis. In all these cases the prognosis is serious and depends in the individual case upon the degree of dilatation, the process which had led to the dilatation, and the intensity or stage of the sequelæ, which are very similar to the disturbances of compensation in valvular lesions. The most unfavorable cases are those in

which the dilatation depends upon a progressive process such as arterio-sclerosis. The chronic forms of myocarditis furnish very slight hope of successful treatment. But even when retrogression of the dilatation can no longer be effected we may hope to improve the function of the heart muscle and restore compensation. In the majority of cases of considerable dilatation, however, even this succeeds only temporarily. In the future we may hope to attain better results. I regard the treatment of dilatation of the left ventricle as a problem of our modern therapeutics. We succeed occasionally in producing temporary diminution in size by digitalis. The new methods of cardiac therapeutics, the dietetic, mechanical plan of Oertel, the gymnastic of Zander, and the treatment with baths, have opened the prospect of securing the cure of dilatation of the heart and converting it into a desired hypertrophy, even in those cases which cannot be cured permanently by our previous methods. But it is desirable to obtain unanswerable proofs for this assertion, and at the same time, to remember the possible sources of error which may occur in the examination and comparison of the size of the heart.

The third form, weak heart, insufficiency of the heart, may also depend upon very different causes. Its symptoms are so evident and easily recognized that the diagnosis, as a rule, is patent. The weakness of the heart's action is shown by the smallness of the pulse, which is sometimes very frequent, sometimes slow, and easily compressed; by general weakness, striking pallor and coldness, syncope, dizziness, and oppression and feelings of anxiety. It is evident that a condition in which the heart muscle threatens to be paralyzed is very distressing and dangerous. The question whether, and under what circumstances this heart weakness can be overcome is a very pressing one. In the first place comes the diagnostic question, whether the heart is organically diseased or not. This distinction is very important in prognosis, although even the weakness of a non-organically diseased heart may become very dangerous at any moment. In general, however, this functional weakness is of slighter significance. In young, neurasthenic, especially hysterical individuals, the prognosis is always more favorable, especially if we know that the patient has suffered from similar attacks before. Youthful age, female sex, offer a more favorable prognosis of heart weak-

ness, although the symptoms are almost always threatening and disquieting.

The most dangerous is the acute (suddenly occurring) heart weakness in febrile diseases, both at the height of the fever, and particularly at the fall of temperature and beginning of convalescence. The justly dreaded collapse of febrile diseases and the sudden cases of death at the beginning of convalescence depend essentially upon rapidly growing heart failure. The rapid heart failure of angina pectoris and anæmic fatty heart is almost equally dangerous, although these conditions may be recovered from. At all events, these accidents are so important that the physician must be able to recognize the first signs in order to prevent the catastrophe.

The chronic cases of heart weakness (called weakened heart by Stokes) occur in all chronic affections of the heart with or without valvular lesion.

They develop when the heart muscle becomes too weak to fulfil the work required. This results in those conditions of stases, insufficiency of the heart, which correspond to disturbances of compensation in valvular lesions, and are best known as such. The work of the heart is either increased abnormally, as in valvular lesions, arterio-sclerosis, cerebral diseases, or if normal, the vigor of the heart is diminished by diseases of the myocardium (myocarditis) or nutritive disturbances (anæmia) or poisoning (alcohol, etc.) or dilatation (idiopathic dilatation).

The prognosis of this chronic insufficiency of the heart will depend upon its degree, but chiefly upon its cause. Progressive diseases, such as arterio-sclerosis, and cerebral affections, which have led to diseases of the heart, offer but slight prospects of complete restoration, while conditions of poisoning or imperfect nutrition, or strain are capable of restitution. The conditions here are very analogous to the disturbances of compensation in valvular lesions and the prognosis depends in great part upon the condition of the heart muscle and the intensity of the sequelæ. Further on we will return to these conditions. Here we will merely remark that in addition to the medicinal treatment the new methods of cardiac therapeutics, especially the mechanical treatment, promise much better results than those hitherto obtained. They are said to be able not alone to overcome the weakness of the heart but

even to include a definite increase in the heart muscle. The indisputable proof for this assertion is not offered easily, and I believe we must await further tested observations before a final conclusion is possible. But it appears to me to be of less importance to decide whether the favorable effect is attributable directly to the cardiac gymnastics or rather to the general improvement in nutrition and function of the entire organism, in which the heart takes part in an eminent degree. A stimulating and tonic action upon the heart muscle is also claimed from the use of warm baths, and this is even compared with the action of digitalis.

We will also devote a few lines to fatty heart, which deserves special mention among diseases of the heart muscle. As I have formerly pointed out, we must distinguish two forms of fatty heart: *a.* The heart in obese individuals; *b,* fatty degeneration of the heart muscle.

The heart in obesity presents various forms and degrees of change which must be judged in different ways. So long as there is merely an accumulation of fat upon the heart, while the heart muscle itself is healthy, so long the prognosis is relatively favorable. Even when dropsical symptoms are already present the prognosis is not bad, and we not infrequently see such cases cured even in the hospitals. Suitable dietetic treatment, accompanied by methodical, muscular action, practised according to the principles of the Oertel method, produce recovery in such cases. But if further changes of the heart are present, especially dilatation of the left ventricle, with pronounced weakness of the heart, the prognosis becomes very doubtful and may become very dangerous at any moment. This is still more true when in elderly obese individuals arterio-sclerosis is added to simple fatty heart, progresses slowly and produces incurable conditions. The dangerous dilatation of fatty heart occurs chiefly in the left ventricle. Enlargement of the heart to the right, beyond the sternum, has no material prognostic importance, especially as this dulness is frequent in obese people and depends upon the abundant development of fat on the pericardium.

The second form, which is also often called fatty heart, is fatty degeneration of the heart muscle. Years ago, I showed that this latter form, as such, is hardly capable of anatomical diagnosis, that it can only be inferred with probability from

the symptom of distinct heart weakness in poorly nourished and anæmic individuals. If we have to deal with simple anæmia or weakness, the prognosis is not unfavorable, but in all severe anæmia and cachexia the accompanying heart affections also furnish an unfavorable prognosis, and in general these forms depend in great part upon the significance of the general condition. We now come to

3. The discussion of the progoosis of valvular lesions. Here the important question first arises whether valvular lesions can ever be completely cured. This question has been discussed a number of times in German literature. One of the earliest and most important articles was furnished by Jaksch, Sr. (Prague, 1860), who first showed with certainty that valvular lesions may recover completely. In his text-book on Auscultation and Percussion, Gerhardt (1868) reports several cured cases (two mitral lesions, one aortic lesion). Then Benecke ("Zur Therapie des acuten Gelenkrheumatismus und der mit ihm verbundenen Herz-Krankheiten"), in calling attention to the curative power of the Nauheim thermal waters, reports fifty-five cases of valvular lesion which were improved or cured by the use of these baths. G. Mayer, in Aix, in his article "Ueber heilbare Formen chronischer Herzleiden" (1881), mentions several cases of cured valvular lesions in his own practice (two mitral insufficiencies). Finally, in our own society, Levinsky held an interesting address on April 17th, 1882, on the prognosis of valvular lesions, which was followed by a vigorous discussion. Levinsky showed that the majority of the known cases of cured valvular lesions were mitral insufficiencies, but that aortic insufficiencies may also heal. He reported one case in which the diagnosis of aortic insufficiency had been made by Traube himself and in which Levinsky noted the recovery. During the discussion, Dr. Badt reported a case of cure of aortic insufficiency. Fraenzel reported two cases of mitral insufficiencies, one of stenosis; and I reported two cases of cure of aortic insufficiency. It is hardly necessary to add anything further to this exhaustive review of the general literature. In the discussion which followed Dr. Broadbent's lecture, Dr. Campbell Black mentions the following: In a youth of fifteen to sixteen years two aortic murmurs were discovered accidentally, although the patient suffered from no symptoms. At the end of five years he married against the advice of the

physicians, had children, etc. Five years later, another examination of the heart showed that the murmurs had disappeared entirely. The patient appears and remains healthy.

From all these observations it is undoubted that valvular lesions may recover, but that the large majority of the recoveries occur in mitral insufficiency. It is indeed proven that aortic insufficiency may also recover, but with comparative rarity. By recovery we mean, as a matter of course, not alone the disappearance of the murmur, but also the permanent disappearance of all heart symptoms. But the cardinal point lies in the permanent disappearance of the murmur. This proof must arouse some suspicion with regard to aortic insufficiency, because it has been demonstrated that the diastolic regurgitant murmur at the aortic valves may disappear, although the insufficiency and its mechanical results have not been compensated. In the experimental examinations on artificial valvular lesions made by E. Klebs. O. Rosenbach, Jager, François Frank, it was found that in rabbits a diastolic murmur was rarely heard, despite the destruction of the aortic valves. Furthermore, it has been observed by myself and others, that in cases in which the aortic insufficiency was combined with aortic stenosis, the diastolic murmur disappears as soon as the stenosis has reached a high grade. Finally, Staff Surgeon Renvers, in an article entitled: "Casuistische Beitraege zur Lehre von der relativen Insufficienz der Aortenklappen," "Charité-Annalen," 1888, p. 223, has shown that in aortic insufficiency resulting from arterio-sclerotic dilatation of the ascending aorta, the murmur increases as soon as distention of the aortic ring is produced by the increased blood pressure, but that this murmur disappears in rest when the aortic pressure is lessened. To this must be attributed a portion of the cases of aortic insufficiency reported as cured.

Hence it follows that valvular lesions which have undoubtedly been observed may be cured in the course of time, and this is true not only of mitral lesions but also of aortic lesions. In the latter, however, such a real and permanent cure is so rare that the physician can hardly take it into consideration with regard to prognosis. Mitral insufficiency, on the other hand, warrants a certain hope of recovery, especially in those cases in which it is recent and has developed from an acute disease. Complete recovery can be expected so much the

more if no sequelæ dependent upon the valvular insufficiency have yet developed. But while the recovery of a valvular lesion must be regarded as exceptional, the second question arises, what is the prognosis of a valvular lesion which does not recover? Experience has taught unquestionably that valvular lesions, recognized by undoubted physical signs, may exist, although the patient experiences no discomfort whatever. The patient is often examined for some other reason, and the valvular lesion is then discovered accidentally. This fact, that valvular lesions are discovered accidentally, and that the patient feels perfectly well, has been known for a long time. We describe this condition as the stage of perfect compensation. The disturbances which might arise in the mechanism of the circulation, as the result of valvular disease, have been prevented by the increased action of that part of the heart situated behind the mechanical disturbances. In practice, as a matter of course, the prognosis which is to be given in those cases in which the patients feel perfectly well is a very important matter. Until recent times such a patient, although he felt perfectly well, was nevertheless regarded as a candidate for death who might die at any moment. "*Hœret lateri letalis arundo.*" The patient himself considered himself as lost; the parents of the unwitting child looked with deep sorrow upon their condemned darling. Compared with these gloomy, pessimistic views, it is to be regarded as an advance whose importance cannot be underestimated, that we have learned more and more from medical experience that such complete compensation may last for many years and that the patient during this time may enjoy life undisturbedly. At the present time, such experiences are no longer rare, and every experienced physician will remember one or more illustrations of this kind in his practice. It was, therefore, a meritorious service, when Sir Andrew Clark, of London, in his lecture to which I have already called attention, made mention of 684 cases in his own experience in which a chronic valvular lesion of the heart had lasted at least five years, although its presence was not indicated by symptoms which had materially disturbed health. The patient consulted the physician for other symptoms (dyspepsia) and the heart lesion was discovered accidentally. In many cases the stage of undisturbed compensation lasts much longer and much more than five

years, even twenty years. It is undoubtedly worthy of note that the absence of heart symptoms for years was determined in such a considerable number of cases. This fact is of the greatest practical importance. It follows, in the first place, that we may not regard every case of heart disease as a candidate for death, and may not treat him in a corresponding manner, but that we must permit him to retain his unconstraint as much as possible. A. Clark mentions cases in which the patient, treated, since the diagnosis of valvular lesion, like one grievously sick, maintained a gloomy existence, secluded from the world; but from the fact that he was permitted to resume his occupation and enjoy life, he was so encouraged and stimulated as to consider himself almost cured. Correct as this medical treatment is, it seems to me to be going too far when Bristowe says: "So long as any one suffering with an affection of the heart suffers no symptoms, he should not be informed concerning it, and should continue in his customary mode of life." I am surely not one of those who regard it as the duty of the physician always to communicate to the patient or his relatives the whole truth concerning his condition. On the contrary, the fortunate unconstraint of the patient would often be lost as soon as he obtained knowledge of his incurable disease. This knowledge is often much more serious than the disease itself. But a patient with a valvular lesion may not live as freely as a healthy individual, and must know sufficient concerning his disease so that he will not expose himself to influences which may aggravate his condition. Oertel says with justice: "It is a dangerous mistake that patients with well compensated valvular lesion require no special advice." On the contary, he can retain permanently his relative condition of health only by avoiding certain noxious influences, among which we may mention particularly bodily strain, while other influences, such as cold, alcoholic drinks, tobacco, coffee, etc., are also important.

Another practical question is, What occupation in life shall a child suffering from valvular lesion select at a later period, especially, whether military service, for example, is possible? The latter question is to be answered in the negative almost in every case, but it does not appear to me to be absolutely imperative to advise a patient who acquires a valvular lesion while in the service to abandon his calling at once. At all

events, mitral insufficiency does not require this decision in every case. Life insurance must also be considered. In England, it appears that a well-compensated valvular lesion does not prevent acceptance in life insurance. In our country such patients may not be accepted. Finally comes the question of marriage. Marriage is not entirely devoid of danger in patients suffering from heart disease. Males may suffer injury from sexual excess. In women pregnancy produces material disturbances. Women suffering from heart disease are very apt to experience disturbances of compensation with dropsy during pregnancy, although this is by no means always the case. I have seen a number in my clinic; the majority recovered after a confinement. The delivery itself passed off well, compensation was restored, but some suffered irretrievable disturbances and did not recover.

Since experience teaches that valvular lesions in many cases may remain for a long time in a state of complete compensation, without notable impairment of health, but that, on the other hand, very many lesions are rapidly aggravated and lead to death, we must attempt in the individual case to make the prognosis more precise, that is, to say whether the case in question is one which permits us to hope for a long stage of compensation, or one which will probably take an unfavorable course. The following data may be considered in determining this part of the prognosis:

a. The first point is, what valve is diseased, and in what manner (whether insufficiency or stenosis). Among all valvular lesions, mitral insufficiency furnishes the best prognosis, that is, it gives the best guarantee both for recovery and for the prolonged maintenance of compensation. The worst prognosis is furnished on the average by insufficiency of the aortic valves, in which the chance of sudden death must also be taken into consideration. Next to this we may mention mitral stenosis. In order, however, to avoid misunderstanding of my statements I will expressly mention that mitral insufficiency and aortic stenosis also occasionally run a very unfavorable course, and that cases of aortic insufficiency are known in which this lesion has lasted for ten, fifteen, or twenty years without noticeable symptoms. I myself have known for fifteen years of a gentleman suffering with this valvular

lesion which, being completely compensated, has produced no symptoms up to the present time.

The prognosis of aortic insufficiency exhibits great differences. As is well known it occurs in two forms: the endocarditic form, other things being equal, gives a better prognosis than the second, the arterio-sclerotic form, which appears at an advanced age (occasionally at an unusually early period), and is thus due to a progressive process. It is also to be noted that the presence of the diastolic sound in the aorta (in addition to the diastolic murmur) improves the prognosis, inasmuch as it allows us to infer a relatively slight insufficiency.

b. The sequelæ in the heart must also be taken into consideration. The less marked the hypertrophy (dilatation) of that part of the heart situated behind the lesion, the more favorable is the prognosis. Considerable dilatations are more unfavorable. The least favorable is marked dilatation of the left ventricle, with insufficiency of the aortic valves.

c. The intensity of the murmurs.

It might be supposed that the valvular lesion is so much more severe the louder the murmur dependent upon it. But this is not true, as has already been stated, particularly by the English writers. On the contrary, loud, distinct murmurs are more favorable than very feeble ones. It is well known that in mitral stenoses when they become very intense the presystolic murmur disappears more or less completely and an analogous condition holds good in aortic lesions. I can confirm this experience entirely from my own observation. Broadbent says: "Physical signs are often deceptive. There is no greater mistake than to measure the danger of a valvular lesion by the intensity of the murmur," and in another place: "As a rule, a loud murmur is less serious than a feeble gentle one. The latter indicates weakness of the heart, which entails the greatest of all dangers. Gairdner also says: "Inexpert observers tend to overestimate the significance of murmurs and to pay too much attention to the mere character of the sounds, while underestimating the other conditions under which the murmur develops.

d. The nature of the endocarditic process, especially with regard to the question whether it is a stationary or a progressive one. The importance of this question is evident. There are a number of valvular diseases which may be called sta-

tionary. In another series of cases the process has a decided tendency to progressive advance. As a rule, congenital heart lesions are entirely stationary. The process has run its course, does not relapse, and the well-established compensation is not disturbed apart from special accidents. I know a young lady who has been a patient of mine for twelve years and who then consulted me concerning a heart lesion which was regarded by some physician as an aneurism. I made the diagnosis of congenital insufficiency of the pulmonary valve. The patient had no further disturbances, except that she occasionally felt the murmur and occasionally only thought that she felt them.

But up to the present time, the condition has remained the same. The patient has become accustomed to her heart trouble and has learned not to notice it further. Moreover, this patient is a twin and has developed as rapidly and vigorously as her sister, who does not suffer from congenital heart lesion. Almost equally favorable are the conditions in valvular lesions after acute diseases, especially in the mitral insufficiencies after chorea. It is known that valvular lesions develop not infrequently in chorea; most frequently, mitral insufficiency, rarely aortic lesions. The former are not alone stationary, but recover quite frequently, while aortic lesions remain stationary.

(A. Clark makes the following statement: "The murmurs of mitral insufficiency after chorea disappear usually in eight to nine days after the attack.")

Among febrile acute diseases, articular rheumatism, as is well known, leads not infrequently to endocarditis and valvular lesions. More rarely, but also undoubtedly, it appears after measles, scarlatina, diphtheria, and typhoid fever. The endocarditic process in these cases is benign and has no tendency to progressive advance. The articular rheumatism presents the danger of a relapse of this disease with which an exacerbation of endocarditis is often associated. On the other hand, the heart lesions after measles and scarlatina, which almost without exception attack the patient only once, remain stationary and furnish a relatively favorable prognosis. It goes without saying that here, other things being equal, mitral insufficiency is also the most favorable, aortic insufficiency the most unfavorable, valvular lesion.

To the progressive, and therefore, unfavorable forms of

valvular lesion belong all those which result from arterio-sclerosis: those arising from strain are also often progressive. Nevertheless, it must also be remarked that many cases of aortic lesions in older people which undoubtedly are due to arterio-sclerosis, may remain stationary for years.

Finally, mention is to be made of febrile or septic endocarditis. That this form is comparatively malignant is evident. It is often, though not always, connected with pronounced valvular disease, especially of the aortic valves, and develops without existing or pre-existing articular rheumatism. The fever is continued, remittent or intermittent, and characterized by its long duration, despite all the measures employed. The consumption arising from the persistent fever entails an unfavorable prognosis. Occasionally embolism of the brain, limbs, etc., is also superadded. Unfavorable as the prognosis is, nevertheless, as I have already reported, there are cases which terminate in recovery after a prolonged febrile course. Only lately I have seen a case of this kind in which the fever continued nearly seven months and in which there was insufficiency of the aortic valves in addition to rheumatic affections of the joints. Finally the fever gradually ceased and the patient is in a tolerable condition. As a matter of course, the general conditions to which I refer above, must also be considered in the prognosis of valvular lesions. Age, sex, constitution and nutrition, character and intelligence, are also not devoid of importance, in a greater or less degree, for prognosis. In brief, a great variety of conditions, which not alone influence prognosis, but at the same time indicate the danger which the patient runs, and thus point out the ways and means to avoid such dangers and disturbances, and, on the other hand, seek the favorable influences, and make them effective.

I now come to the stage of disturbed compensation, in which the conditions for compensation that have developed in the diseased heart no longer act completely. To the physician this stage is by far the most important, more important than the stage of complete compensation, inasmuch as in the latter we can only act in a prophylactic way, by prescribing the suitable mode of life, while the stage of disturbed compensation requires the cautious activity of the physician. In view of the manifold character of the symptoms and their significance in

the stage of disturbed compensation, it is advisable to distinguish several phases:

1. The phase of mild disturbances of compensation, which consists in the fact that the patient is no longer as capable as a healthy individual; that he is more apt to suffer from dsypnœal disturbances, palpitation, perhaps slight stases in the lungs, slight attacks of hæmoptysis. With a few restrictions, however, he may still live like a healthy individual. He may be active and enjoy all the pleasures of life in proper measure.

2. The second, more severe phase, is characterized by the occurrence of dropsical symptoms. These entail a graver prognosis, but are by no means absolutely unfavorable. This stage also lasts for a longer or shorter time with many variations.

3. The third phase, in which the prognosis is desperate, is characterized by severe dropsy, which no longer disappears under any treatment; by marked dyspnœa, so that the patient is no longer able to do anything, and by the other sequelæ of stases in the liver and lungs. Even in this stage the end may be somewhat deferred by treatment or by the forces of nature, but we can no longer expect decided and permanent improvement, and can only delay the catastrophe. The end occurs either from the results of dropsy or from a sort of a uræmic condition with delirium, or from paralysis of the heart (asystoly) or, finally, from complications on the part of the lungs, kidneys, or brain. In contradistinction to this latter phase, the second form offers a wide and not unfavorable field for the activity of the physician and furnishes by no means always an unfavorable prognosis. The first phase affords most room for the hope that we can succeed in again compensating the disturbances, and securing to them compensation for a longer period. But even the second (dropsical) phase by no means excludes in all cases the hope of complete restoration of compensation. At all events it may be hoped that the disturbances will disappear temporarily and the continued advance of the disease be materially delayed.

In the individual case the prognosis must depend upon the intensity of the sequele. The more advanced the stage of disturbance of compensation, the more difficult will it be to restore the compensation completely for any length of time. Then, the intensity of the changes in the heart itself, which

permit an inference concerning the intensity of the heart lesion, must also be considered. Thus, an aortic insufficiency, with pronounced dilatation of the left ventricle, will furnish a more unfavorable prognosis than another with slighter dilatation. The affected valve and the nature of the endocarditic process, as remarked above, must also be considered.

Of great importance in the prognosis is the condition of the heart muscle. This is determined essentially by the tension and frequency of the pulse, the character of the renal secretion, the degree of dyspnœa.

A practically very important question is whether the present disturbance of compensation or the exacerbation of former disturbances have developed gradually, without any special cause, or from the presence of factors which may be removed and relieved. In such patients we not infrequently see a very decided aggravation of the condition by an intercurrent bronchitis, pleuritis, pneumonia, etc. But if we succeed in overcoming this danger, the old favorable condition may again return. This is also true of the action of pregnancy, to which I have previously referred.

Finally, the prognosis depends upon the efficacy of drugs. As we possess a large number of drugs and curative methods, we will choose another when the patient no longer reacts to one remedy. According as the patient reacts well to one of these remedies the prognosis improves. Sometimes it is a matter of accident to find that drug which has a good effect. In general we can expect success so much the less when the patient has already been treated with caution by all suitable remedies—so much the more, the less therapeutic aids have been exhausted. This is also true of the dietetic, mechanical methods of treatment. Oertel himself states that, in cases in which no increase of the renal secretion can be obtained by limiting the ingestion of fluids and in which the difference between the absorption and discharge of fluids amounts to fifty per cent, the outlook for the success of these methods of treatment is very unfavorable.

III. With regard to the different relations of the size of the heart, its hypertrophy, dilatation, and diminution, I have already spoken sufficiently—but I must add a few remarks with regard to the prognosis of heart murmurs. Systolic and diastolic murmurs in particular must be kept distinct in this

respect. Systolic murmurs offer the best prognosis, both in regard to their recovery as well as to the other disturbances in the heart. In the first place, they are often non-organic in their nature, although endocardial in their development, as may happen without organic disease of the organ. They are partly blood murmurs, partly accidental murmurs, or also systolic pulmonary murmurs. In one of the last numbers of the New York "Medical Record" is found a table of examinations by Dr. Morton Prince, of Boston, who carefully examined for cardiac murmurs seventy-seven individuals suffering from no cardiac symptoms, and in twenty-five found a systolic murmur, that is, in a third of the cases. These conditions *per se* necessitate a favorable prognosis for systolic murmurs. Even those which must be attributed to diseases of the heart permit a more favorable prognosis than other cardiac murmurs. Although I hereby mean to imply that systolic heart murmurs, without other symptoms of a heart disease, cannot be at once regarded as signs of a cardiac affection, nevertheless, I desire to warn you, on the other hand, against neglecting them under all circumstances. Here, also, everything depends upon the other conditions, especially the age of the patient. At an advanced age, blood murmurs, anæmic and other accidental murmurs, are rare; the organic murmurs more frequent. In this respect there is also an essential difference between the systolic murmurs at the apex and those at the aortic orifice. The former are often accidental; the latter very rarely; and in older individuals enable us to infer sclerosis of the aortic valves or dilatation of the ascending artery.

The matter is entirely different in diastolic and pre-systolic murmurs, which, with very few exceptions, permit us to infer an organic change in the heart. Accidental diastolic murmurs which occur without other cardiac symptoms and again disappear, are extremely rare, so that they need not be considered in the prognosis. The only exceptions are the venous murmurs in anæmic individuals, which are heard below the manubrium sterni and which are sometimes of a distinctly diastolic character. As a rule their differentiation from the diastolic endocardial murmurs is not difficult, with a little care. Their significance is similar to that of the anæmic venous murmurs.

I will now mention a few abnormalities of rhythm: inter-

mittences, and arhythmia of the most varied kinds. The patients are especially terrified by the intermittence of a pulse beat, followed, as a rule, by a more vigorous pulsation and extremely disagreeable feeling. Nevertheless, this symptom has no serious significance and does not depend upon an organic lesion of the heart. It is found in healthy individuals, in febrile, young and older persons. It depends most frequently upon reflex irritation in the abdomen (irritation of worms in children) and upon mental excitement which has lasted for some time. Of still less significance are the arhythmias which consist, not in intermittence, but in irregularity of the heart's action. According to my experience, these phenomena are usually of an individual nature. In some persons they occur often without special cause, and I have known such patients in my practice in Koenigsberg, who suffered from irregular pulse during their whole life. As a matter of course, these facts do not exclude the occurrence of intermittence, as well as of arhythmia, in heart disease, and other notable affecions, but this symptom has no grave significance, and therefore admits of a favorable prognosis—perhaps not entirely good with regard to recovery, because arhythmia often becomes habitual—but, at all events, with regard to any possible danger. Nor am I aware that in the course of heart disease arhythmia has any material influence upon the prognosis. It decides nothing, even when it is observed in the later stages. A more important condition is delirium cordis, an arhythmia which is so considerable that the rhythm can no longer be distinguished. The name was first introduced among us by Traube, but is found at an earlier period among the French writers, who speak of "délire du cœur" and "folie du cœur." Allied to this is another symptom, called tremor cordis by the older physicians—that is, the contractions of the heart are either so irregular, or so feeble or incomplete, that the hand when applied to the heart seems to feel a trembling of the organ. Both of these irregularities are much more important than those previously mentioned. Delirium cordis, if at all marked, cannot be tolerated for a long time, but leads to circulatory disturbances which threaten life. I remember a case in an old man, in whom, as the result of psychical excitement, delirium cordis, with anxiety, had developed, and severe disturbances of circulation, with dropsy, were the result. For some months the

patient was very ill, but recovered completely. Slight irregularity of the pulse continues to the present time. Tremor cordis, insomuch as it indicates a frequent and feeble heart's action, is naturally a symptom of grave significance.

In conclusion, I will mention the symptoms of changed frequency, tachycardia and bradycardia. The former indicates conditions of considerably increased frequency of the pulse. This also presents a threatening character. Physiologically we consider it as a condition in which the pneumogastric nerve is either paralyzed or approaching paralysis. We know that the condition of the pneumogastric of the heart of higher animals, for example, the dog, will produce such tachycardia. This is not always directly fatal. It may be borne for a long time, but is nevertheless associated with danger to life. The question, how long such a paralysis of the pneumogastric may be tolerated, is answered with difficulty, because other disturbances, as the result of the deviation of the nerve, also appear in the operated animals. According to the physiological doctrine, we assume that the regulating nervous system of the heart which is situated in the pneumogastric, has the effect of saving the heart's action in such a way that a paralysis of the pneumogastric is able to use up the power of the heart. *Per se*, it may be said that in a rapid action of the heart the work which the organ performs has not become greater—that is, the blood is merely propelled in the normal manner—but the diastoles are shortened, and then the nutrition of the heart is impaired. Indeed, it is not improbable that a frequency of 120 or 150 contractions requires a greater consumption than one of 60 beats, although both only perform the same work. Without deciding the question here I accordingly come to the conclusion that the generally entertained medical opinion is justified, namely, that a high frequency of the pulse consumes the heart and therefore entails danger to life. In the individual case, however, the limit at which the dangerous exhaustion must occur cannot be determined. Under certain circumstances the heart tolerates astonishing conditions in this respect, and we are unable to say what limit can no longer be tolerated, nor how long, in individual cases, such abnormal frequency can be borne. As a general thing, a frequency of more than 120 must be regarded as a serious symptom, but there are cases in which a frequency of 140 to 160 is tolerated

for a long time. In my article on sclerosis of the coronary arteries I have reported a case in which the patient, who suffered from angina pectoris and weakness of the heart, had a pulse of 140 for four weeks. This is extremely rare. The frequency did not diminish, but rather increased to 160, and the patient died with severe heart symptoms and delirium. Finally, we observe remarkably high frequency of the pulse in Basedow's disease, in which a pulse of 120 is considered small, while the patient hardly feels the palpitation. It is only when the frequency reaches 140 to 160 that we become somewhat alarmed, and, so far as my experience goes, danger is present at those limits and the prognosis becomes dubious. In one case I even observed a frequency of 200, in a woman who had suffered for years from Basedow's disease, but only experienced this high frequency of the pulse for a short time. She died a few days later.

Febrile tachycardia is an important symptom. It is the ordinary practical view that a frequency of the pulse of more than 120 in fever makes the prognosis serious. On the whole this statement is correct, but marked differences must be considered. In acute exanthemata a very high frequency is the rule; 120 to 130 in scarlatina of young people has by no means a serious significance, while the same frequency in adults is dangerous. The frequency of the pulse plays a specially important part in pneumonia, and Dr. Fraenzel, in a lecture on pneumonia, has justly stated that in this disease the high frequency of the pulse is more important than respiration. During this winter I saw two cases in which the frequency of the pulse exceeded 160, and yet recovery occurred. But in two other cases, with the same frequency, death set in, although the other symptoms of the pneumonia were confined within moderate limits. Allied to this is the occurrence of high frequency of the pulse in the third stage of meningitis (120 to 160 beats), which depends upon the direct paralysis of the pneumogastric, and has an immediately fatal significance.

I will also mention paroxysmal tachycardia (Gerhardt, Nothnagel, and others), a very interesting disease, of which I have seen quite a number of cases. It consists in the fact that patients from time to time, usually as the result of some excitement, or from excess in drink, but occasionally without any special cause, are attacked by remarkably violent palpita-

tion of the heart. I recently observed a case of this kind in the Charité, in a peddler who endured much bodily and mental strain, and who entered the hospital with a frequency of the pulse of 160 beats. The attack lasted six days; then the pulse sank nearly one-half. A few days later a similar attack ocurred, and again lasted for several days. The majority of cases of paroxysmal tachycardia are similar. The prognosis is not bad, because the high frequency of the pulse is only temporary. Moreover we know from the former attacks that the patient tolerates this frequency well. So far as my knowledge goes, we have to deal chiefly with nervous, especially psychical, influences, or with severe alcoholic excesses. Allied to this is the acceleration of the activity of the heart by psychical impressions, terror, fright, which we observe particularly in the female sex, and which is unattended with any special danger. The tachycardia of convalescents may also attain a high degree. Convalescents are often attacked on the slightest mental or bodily excitement, by high frequency of the pulse, with a feeling of oppression and syncope.

Considerable slowing of the pulse (bradycardia) is a rarer symptom. Moderate slowing, which *per se* has no notable prognostic significance, occurs, for example, in convalescence, also in jaundice, etc. In practice, slowing of the pulse is justly regarded as a symptom of feeble heart's action and is prognostically and therapeutically important. From it we draw the indication for cautious stimulating treatment in which digitalis is usually not to be employed. More pronounced slowing of the heart's action is observed after the use of digitalis, and in angina pectoris. I remind you of the investigations of the late Dr. Samuelson, of Koenigsberg, and Schultheiss-Rechberg, who had worked under Cohnheim. Both experimental investigations are not entirely in harmony with one another, but both furnish the result that ligation of the coronary arteries, especially of the left, produces considerable diminution in the frequency of the pulse, which in some cases even falls to eight per minute. The frequency of the cardiac contractions also falls considerably after prolonged use of digitalis. This can be observed in the most exquisite manner in cases of angina pectoris and dilatation of the heart, myocarditis fibrosa. We then notice, as I have shown in my article on sclerosis of the coronary arteries, a diminution of the

frequency of the pulse to 32, even to 24 beats, as a rule, in such a manner, that after a strong heart beat accompanied by a distinct arterial pulse (radial pulse) there follows rapidly a second feebler heart beat which does not correspond to a perceptible radial pulse. Now follows a longer cardiac diastolic interval. This phenomenon of ictus cordis bigeminus is in itself of great interest, which is increased by the fact that, as it appears, it gave rise to the discovery of the action of digitalis. This remedy celebrated its jubilee two years ago. According to an article which I found at this time in an English journal, it was first a secret remedy against dropsy, until the Scotch physician Withering determined to find out the active ingredient in this remedy. He recognized as such digitalis (then called foxglove) and discovered the pulse-slowing action of this classical drug. At the same time he describes a case of heart disease in which considerable improvement occurred after the use of digitalis, and the pulse also fell to 32 beats. I infer that he had to deal with a case of dilatation of the heart as the result of myocarditis fibrosa.

The last form that I will mention is permanent bradycardia, those remarkable and not sufficiently explained cases in which the frequency of the pulse in an individual sinks quite suddenly, as it seems, to 30, 24, or even 20, and remains at this height permanently with very slight variations. As is well known, Stokes was the first to describe these cases with the accompanying epileptoid attacks. He attributes the affection to sclerosis of the coronary arteries. From further observations, however, of which only a small number have yet been made, it appears that this assumption holds good only in a few cases, in others, this lesion cannot be assumed. The disease is observed chiefly in old people, but also in men in the best years of life. The prognosis has hitherto been quite uncertain. There is a decided predisposition to attacks of dizziness, syncope, and even to a sudden fatal termination, particularly in old people. Nevertheless, such patients may live for a long time, and feel tolerably well. I have known for ten years a Berlin gentleman who goes about with a frequency of the pulse beats of 24, and now, hardly 20, and, apart from occasional attacks of dizziness, feels in good health.

My review is now finished. I beg pardon if I have strained your patience too long. Much could have been expressed,

perhaps, more precisely; some matters perhaps more exhaustively; some more briefly; but complete exhaustion of the subject in all directions could hardly be attained. In conclusion I will make two remarks. In the first place I wish to emphasize that it has been my endeavor to discuss the prognosis, not alone in so far as it may be termed a good or a bad prognosis, but I have devoted more attention to the cases which entail an uncertain prognosis. The field of doubtful prognosis is the real field for the activity of the physician. When the individual is struggling with this disease and its sequelæ, when the termination is doubtful—there lies the real activity of the physician and the medical art. Here the physician may not idly fold his hands and confine himself to the rôle of an observer. He also must enter the combat, must fight the disturbances and dangers and make use of the favorable conditions which are presented for the patient according to the best of his endeavor. He must fight with all medical remedies and for the cure, or at least the favorable course, of the disease.

Secondly, although I dealt only with the prognosis of diseases of the heart, I could not avoid, nor did I wish to, touching in certain places upon treatment and calling attention to those conditions in which therapeutics could take a part, and where at the present time we find new problems for our therapy. In this respect I mention the progress which the treatment of diseases of the heart has made in the last few years, partly from the introduction of new drugs, partly from the dietetic, gymnastic methods which, in a certain sense, have induced a reform in this field of therapeutics. Although the discussions still continue, and the results and indications are not yet clear on all sides—and this is not to be wondered at, in view of the complicated objects in internal therapeutics—nevertheless, we have undoubtedly made a decided advance. We find with grateful satisfaction that internal therapeutics, as in other fields, has not stood still in the department of diseases of the heart; that, free from dogmas and foreign authority, it pushes its own way, and, conscious of its final and chief purpose, causes medical activity to become more and more fruitful and useful. Without claiming that we effect miracles and that we can reverse the laws of healthy and diseased life, yet we can look back with satisfaction upon the results of in-

ternal treatment and of the medical calling. We are justified in refusing the pessimism which long bore heavily upon internal therapeutics, and whose sources are found, on the one hand, in unjustified modesty and timidity, and, on the other hand, in an insufficient knowledge of what is to be effected, and can be effected, by internal treatment.

THE SPUTUM.

A Contribution to Clinical Diagnosis

AND

PRACTICAL EXAMINATION FOR TUBERCLE BACILLI.

BY

DR. PETER KAATZER,

Bad Rehburg, Germany.

THE SPUTUM.

CHAPTER I.

DEFINITION AND DIAGNOSTIC SIGNIFICANCE OF THE SPUTUM.

THE appreciation of the sputum with regard to its diagnostic and prognostic significance for affections of the respiratory organs may be historically traced back to the beginning of medicine.

The views of Hippocrates and Galen concerning the sputum must be considered as having been the dogmatic property of physicians for centuries. They are characterized by keen powers of observation, in so far as they are not influenced by humoral-pathological errors and philosophical speculations.

During the middle ages and until the seventeenth century, there could, of course, be no thought of newer views; even in the eighteenth century the humoral pathology of ancient times still found credence among the majority.

A new epoch for estimating the importance of the sputum from a diagnostic standpoint dawned with the beginning of the present century, when great advances were made in pathological anatomy, and physical diagnosis was introduced in the study of respiratory diseases. Chemistry and microscopy were soon added as diagnostic handmaidens. The limits of observation were enlarged by the work of Henle, Virchow, Traube, etc., as well as by microscopical and chemical inquiries. But above all new points of departure were opened for us by microscopical investigations, combined with bacteriological technique, the utilization of which has been of the utmost importance in the diseases of the respiratory apparatus. Even

though the investigation of the sputum for certain diseases has not been established so firmly as to serve for an absolute criterion—as, for instance, a certain amount of reserve is still necessary in deciding the infectious nature of pneumonia from the character of the sputum—yet the mere fact that we are enabled, at a time when other diagnostic evidence is wanting, to characterize a lung disease as unquestionably of a bacillary nature, should suffice to demonstrate the very great value of the investigation of the sputum in clinical diagnosis. Continued studies in this field of diagnosis will not permit microscopical and bacteriological sputum-culture to take an inferior place among the clinical methods of investigation.

Since the time of the monograph of Biermer[1] in 1855, no comprehensive treatise on the sputum has appeared. It, therefore, appeared to me timely to classify the results obtained in this field in the last few years, since it seems to me they have not been estimated as they deserve, in order that the practitioner might make himself thoroughly acquainted with the subject in a short time.

I thought myself the more justified in this determination, because the copious clinical material at Rehburg gave me special opportunities for cultivating this branch of diagnosis.

As regards the engravings, it should be stated that Figures 1, 2, 3, 4, 7, 8, 9, 10, 11, 12, and 14 were drawn from my own preparations by Mr. Otto Peters, academical painter and draughtsman of the University of Göttingen, with his accustomed accuracy, for which I hereby extend my thanks; Figs. 5, 6, 13, and 15, are borrowed from Eichhorst's "Handbuch der Untersuchungsmethoden," Flügge's "Micro-organisms," and Eulenburg's "Encyclopædia."

I request the indulgence of my confrères for some of the imperfections, of the existence of which I am aware, especially in the bacteriological portion of this essay, and solicit their kind criticism.

In general we consider the sputum the product of the secretions of the mucous membrane of the respiratory apparatus, from the opening of the oral cavity to the finest bronchial twigs.

A differentiation into laryngeal, bronchial, or pulmonary sputum cannot be effected, so long as we are unable to deter-

mine with certainty the origin of the expectorated masses brought to our notice.

When the sputum consists of the secretion of the respiratory mucous membrane, formed under the usual physiological circumstances, it is to be designated as normal; when, however, it is composed of products determined by a local inflammatory process, we speak of it as a pathological sputum; if it contains micro-organisms, which by culture and inoculations can be shown to possess pathogenetic properties, an infectious sputum will also have to be differentiated.

Phthisical sputum may be classed as a special representative of this class.

The examination of the sputum has become one of the most valuable and not infrequently decisive aids in clinical diagnosis. "It brings to light substances as carriers of pathogenic products which are developed in the invisible well-springs of disease" (Biermer).

The sputa warrant us in procedures which we would not resort to at all, or do so at too late a period, after other methods of diagnostic investigation. Proof of this is apparent, among other things, in the discovery of elastic fibres at a time when physical exploration fails to give evidence of the destruction of pulmonary tissue; but the most decisive instance, and at the same time one of the utmost practical importance, is the demonstration of the bacilli of Koch.

The spit-cup and the microscope, relegated to a secondary place in respiratory diseases by percussion and auscultation, have thereby become indispensable.

The value of the momentous discovery of Koch[2] to the practice of medicine consists, for the present, in the incontrovertible proof that a bacillary process is taking place at some portion of the respiratory tract whenever we find the bacilli of Koch in the sputum. The nature of those affections which formerly were not recognized as so-called tuberculosis—and probably could not be so recognized, because the clinical picture, which their course presented, more frequently simulated that belonging to other affections—is particularly made clear to us by this means. I call to mind in this regard the so-called "innocent" catarrh, the catarrh of the apices, "which require for their cure only a journey into the country or to the mountains;" the variable, apparently gastric and chlor-

otic disturbances, the well-known cases of initiative hæmoptysis, pulmonary syphilis, laryngitis, bronchiectasis, etc. The presence or absence of the Koch bacilli in all these diseases establishes the diagnosis, determines the prognosis more or less, and prompts us, by reason of practical experience and a knowledge that not every bacillary affection must inevitably have a fatal termination, to take such steps, provided it is not too late, as are capable of healing the cavities and eliminating the parasites. I have frequently had the opportunity of noting the serious consequences which followed such errors in diagnosis, in cases of consumptives who were treated for pulmonary syphilis (?) with antisyphilitics, with sweat-cures, vapor-baths, etc., and who were then sent to Rehburg to improve their reduced conditions.

The discovery of bacilli in the sputum removes every doubt. I have had numerous similar experiences in so-called stomach-cough, chlorosis, "colds," etc., and could illustrate them by innumerable histories. I was once enabled to diagnosticate, by means of the sputum, an acute tuberculosis which presented the picture of typhoid fever, and which ended fatally on the ninth day. Analogous data were obtained in doubtful cases of laryngeal syphilis and phthisical ulceration. Testing the expectoration, or eventually removing it from the larynx by means of a brush or sponge, will always definitely decide the question. "Of course a reliable and decisive diagnosis can be made only by him who possesses a good optical apparatus and is a practised microscopist, and who can devote the necessary time to the investigation, otherwise the whole procedure is valueless. This is not, and cannot, be everybody's task, and we therefore have to content ourselves, in practice, to advise those who have not the requisite qualifications, to seek assistance as soon as possible from a competent source in every doubtful case" (Ewald[3]). With sufficient patience and skill, it is hardly possible to miss the bacilli; according to Gaffky,[4] they are present in ninety-seven per cent of the examinations. I arrived at the same result in my independent researches, which embraced more than one thousand cases. It must not be forgotten that, even in ulcerative affections of the pulmonary tissue (cavities), the bacilli need not be demonstrable in the expectoration day after day—a fact noted also by Koch[4]; that a *transient* freedom from parasites may

occur when the communications with the bronchi are either temporarily closed by tough plugs of secretion, or merely the spores are present, which attain further development only under favorable circumstances.

Notwithstanding that such cases are to be regarded as rare exceptions, we should derive the hint from them not to base a positive diagnosis upon a single investigation of the sputum.

CHAPTER II.

GENERAL TECHNIQUE OF THE EXAMINATION OF THE SPUTUM.

THE examination of the sputum consists of two parts, of which the one supplements the other in equally valuable degree —the macroscopical and the microsocopical. The macroscopical test must necessarily precede the microscopical, or we should be guilty of great negligence, which frequently enough shows itself by inaccurate and negative results; despite its incompleteness, the former method will often give us very useful diagnostic landmarks.

We first scrutinize the appearance of the whole expectorated mass (the sputum of the previous night and that first voided in the morning) with regard to color, consistence, and transparency, and in general seek for pathological products, such as blood, pus, etc., according to their quantity and various sources. If it be made a principle to make our examination of the substance not alone as it is voided in the vessel for its reception, but to distribute it as thinly as possible over a preferably blackened plate or black paper, we shall not be likely to overlook important admixtures, and will further be enabled to distinguish indifferent matters, as bread crumbs, fibres of meat, etc. For some cases, as in those in which we seek for coagulated fibrin or elastic fibres embedded in the mucus, it is advisable to agitate the sputum in water in a glass cylinder; after removal of the water the parts may be more readily isolated from the sediment; however, I may remark that for microscopical examination it is not desirable to have the patient expectorate into water, as some elements become altered or dissolved by its action.

Each macroscopically distinct part is now to be examined under the microscope. For this purpose it is to be "teased" with platinum needles in glass handles, which must be sterilized at white heat before use; as small a particle as possible is detached and transferred to a cover-glass. It is here to be spread out in the thinnest layer, by being either sep-

arated with the needles or pressed between two cover-glasses, which are to be gently drawn apart, not lifted. Additional fluid for dilution is generally superfluous; when employed, a 0.75% saline solution or sterilized distilled water suffices.

The microscopical scrutiny at first consists of a low power (60 to 80 diameters); after this the different portions of the preparation are viewed under a higher power (500 to 600 diameters), and eventually, when schizomycetes are sought, with immersion lenses, and more accurately analyzed. If dry preparations are to be made, we must proceed in the manner described for the Koch bacilli (page 118).

Of reagents and staining fluids which are generally employed, the following may be mentioned:

1. Acetic acid, generally a ½–1% solution; in many cases the preparations are clarified and rendered more transparent by this substance; nuclei, fat-cells, and micro-organisms are made to appear more distinctly; elastic fibres are resistant to the action of the acid.

2. Potash or soda-lye. For most cases a 1 to 3% solution is used. By these the cells are also dissolved, but the elastic fibres resist, as well as the fats, spores, and schizomycetes. In a 33% solution the form of the red blood-corpuscles is preserved, while they disappear in more dilute solutions. Both reagents are above all valuable in the diagnosis of micro-organisms which are not affected by them; the colonies of micrococci known as zoöglœa, particularly are made more pronounced in appearance, and the forms of single individuals, rods, etc., are accurately characterized.

3. Solutions of aniline dyes. Of these, I use almost exclusively a concentrated alcoholic solution of gentian violet (20–25 gm. : 100 alcohol), which is properly diluted by aniline-water. In the event of a probable double staining, I have at hand a concentrated aqueous solution of vesuvin, which is preserved from moulding by the introduction of a small piece of camphor, and may be kept for quite a long time. The quantity necessary for each use is to be filtered through a moist medium.

Aniline water (¼ aniline, ¾ distilled water) is to be always freshly prepared by shaking for a minute in a test-tube.

4. Compound iodine solution (iodine 1, iodide of potash 2, distilled water 300). For employing the Gram method for isolated staining of schizomycetes.

CHAPTER III.

THE CONSTITUENTS OF SPUTUM.

Pus-Corpuscles (White Blood-Corpuscles).

Pus-corpuscles, besides blood serum, fibrin, and albumin caused by transudation from the vessels, are the physiological products of inflammation, and, according to the researches of Cohnheim, are to be regarded as white blood-corpuscles which have emigrated from the blood-vessels in large quantities; they therefore also possess the same dimensions. The protoplasm of pus-cells is partly impregnated with finer and coarser granules, and partly, as it were, dusted over with these bodies; numerous nuclei, often three or four, are embedded in the protoplasm, which in a fresh condition of the cells are generally covered by the granules, and therefore but dimly visible; only after the latter have been made to disappear by the addition of dilute acetic acid, do they become clearly defined and recognizable. Dead cells are round and immovable, and frequently contain finely granular matter originating in fatty metamorphosis. This increases the opacity and causes the yellowish tinge of the sputum.

Mucous and Salivary Bodies.

These are round cells similar to pus and lymph cells, of a cloudy but not granulated appearance, with one or two roundish nuclei, and granular contents. The salivary bodies come mostly from the submaxillary and sublingual glands, and in the course of their exit become associated with the sputum. In fresh living cells, their granular contents, under a more powerful lens, show a peculiar amœboid phenomenon, a distinct "dancing" of the granules. This motile property, similar to the so-called Brownian molecular movement, has so far been unexplained; however, it is not a vital manifestation originat-

ing in parasitic organisms, which could in this manner simulate spontaneous movement; it is a purely physical phenomenon; for this reason, it is impossible to stain the granules with aniline dyes.

Epithelium.

The two chief groups found in the sputum are pavement and cylindrical epithelium. They constitute a normal admixture from the mouth and respiratory tract. Pavement epithelium—so-called from its resemblance to the stones of street paving—is the product of the oral mucous membrane, the tonsils, the tongue, the salivary glands, a portion of the larynx (the vocal cords), and the small bronchial glands. Their origin may be suspected from their size and shape. The large, flat, usually irregularly hexagonal cells, occurring isolated and cohering in larger patches, come from the oral cavity. They undergo a peculiar transformation in form and by chemical combination (keratin) — a so-called cornification — by which they become broader and flatter, and the normally large and vesicular nucleus and one or more nucleoli become less distinct and smaller and disappear. By the addition of a 5% potash solution we can cause them to swell up and resume the tesselated form. They are generally surrounded by a dark wide mass, which under a higher power appears as heaps of schizomycetes (cocci). In catarrhal conditions of the mouth and pharynx, ptyalism, etc., the sputum contains them in large quantities and thereby gains an increased opacity and flocculant appearance. (See Figs. 8 and 9.)

The smaller pavement epithelium is principally excreted from the bronchial mucous glands in bronchial affections, but otherwise shows similar properties. No especial diagnostic importance attaches to these formations.

The second form, cylindrical epithelium, the simple as well as the ciliated, is far less frequently found in the mucous portions of the sputum, notwithstanding that the entire respiratory tract, from the larynx (with the exception of the vocal cords) to the finest bronchial twigs, is furnished with ciliated cells; this may be due, as surmised, to the firm insertion of these cells by root-like processes. When found in the sputum, it generally comes from the nasal mucous membrane or the posterior surface of the velum palatinum; according to the

view of Biermer, this may take place in those individuals who have the inelegant habit before expectorating of bringing the mucus from the palate together with the sputum by an inspiration (nasal screatus), in such cases not infrequently the movements of the ciliæ may be observed in freshly voided sputum; according to an interesting experiment of Virchow, the ciliæ may be re-animated, after they have ceased to move, by the addition of very dilute alkalies. They are not essentially changed in contour by the expectorated substances.

So far no agreement has been arrived at concerning the nature and significance of a third form of epithelium in the sputum, the so-called alveolar epithelium. These round cells resembling epithelium are found in almost all sputa. They are from two to four times as large as white blood-corpuscles, and are further distinguished by a sharp spheroidal contour, a vesicular nucleus with granular contents, and one or more nucleoli (Fig. 11). They take on a spherical shape by endosmosis during inflammatory processes. Buhl[5] regarded these bodies as desquamated pulmonary alveolar epithelium, and their presence warranted him in diagnosticating a beginning phthisis. According to the researches of Guttmann and Smidt[6] and Bizzozero,[7] and in my own experience, they are not absent in any affection of the respiratory apparatus, not even in simple bronchial catarrh, and in the early morning sputa of apparently healthy individuals.

The pathological degenerative changes which affect all cells are not lacking in this variety of epithelium. The alveolar cells are most frequently subject to fatty metamorphosis, which destroys them; the nucleus becomes completely covered by the round, sharply refracting fat granules—fat granule cells, which then become masses of fat aggregation and finally are changed into structureless detritus.

In addition, myeloid degeneration is a characteristic feature of these cells (mixture of the fatty acid with an alkali), through which homogeneous, dull, or nacreous, glistening granules become embedded in the cells, which for purely extrinsic reasons have been called "myelin granules." By treatment with 0.1% osmic acid, they take on, like the fats and nerve axis-cylinder, a deep black color by reduction of the metal; the reaction occurs in a few minutes; they probably have no diagnostic significance.

Black pigment is quite frequently found in the protoplasm of the alveolar cells, in the form of minute dark-brown or deep-black granules very resistant to chemical influences, the value of which has been greatly overestimated or erroneously explained by earlier investigators; the pigment also occurs in greater quantities freely scattered about (Fig. 8). Morgagni considered black sputum as a symptom of beginning phthisis. It may be recognized macroscopically in the mucous portions of the sputa, appearing as gray or grayish-black particles, and as such are frequently distributed throughout the expectorated mass. These formations consist of those particles of dust or soot which gain access to the mucous membrane of the respiratory organs during inspiration, and may afterward be carried into the interstitial tissue of the lungs and the lymph channels by means of amœboid cells. This pigment is to be found in almost all individuals, varying in quantity according to position in life and occupation.

I observed it in the greatest profusion in the sputa of a saddler who suffered from chronic bronchitis, and who, during his stay at Bad Rehburg, expectorated this black matter for weeks at a time. Although in this case the bacilli of Koch were never found present, we must recognize, according to Koch's investigations regarding the quantitative presence of fungi in the air, that in a predisposed individual these dust particles could, cæteris paribus, be the vehicles of infection.

This black pigment can be readily distinguished from the genuine (that is, originating in the organism) brownish (not black) pigment, which rarely exists in the sputum and which depends upon a transformation of the coloring matter of the blood (hæmatoidin), as a sequence of preceding hemorrhage or stasis in the pulmonary venous system with diapedesis of red blood-corpscles and pigmentary transformation of the latter; this condition is prone to complicate certain cardiac diseases.

Red Blood-Corpuscles.

In a few diseases of the lungs and mouth the red blood-corpuscles give the predominant characteristic to the sputum, and become mingled with it during expectoration. Their presence is macroscopically recognizable by its color, and this again is dependent upon the thoroughness or otherwise of

the admixture. The sputum to the unaided eye may appear spotted with red color and permeated with puncta and streaks, or by more intimate mixture it may appear yellowish-red, or light red after severe hemorrhage, or of dirty red, or rusty like prune-juice color after longer sojourn in cavities, etc. In the course of this work I shall refer to this matter more in detail.

Microscopically, the blood-corpuscles normally appear as the well-known biconcave discs, with a central depression marked by shading, a sharp contour, and yellowish-red color. If the sputum has stagnated for some time in the respiratory tract before expectoration, the position which the blood-corpuscles occupy strikes the observer; they do not as usual adhere by their flat surfaces in the form of rouleaux of coin, but by their borders. Biermer sees the cause of this in the fact that "the blood-corpuscles are separated during the mechanical act of expectoration, becoming, so to speak, soiled by mucoid or other expectorated matters, and thereby are less adapted to readhere to one another."

If bloody sputa be allowed to remain immersed in water for some time, the corpuscles become spherically swollen, shrunken, or decolorized, and perish; they can no longer be demonstrated by the microscope, notwithstanding that the yellowish-red color has been preserved by the hæmoglobin remaining.

Elastic Fibres.

Their presence is always dependent upon destructive processes in the pulmonary parenchyma or the bronchi. Overlooking those isolated cases of ulcerative bronchitis or pulmonary abcess in which elastic fibres are found in the sputum, we must consider them as the product of necrosed tissue of the lungs and bronchi, induced by the action of Koch's bacilli. The severity of the affection may be measured by the quantity of fibres present; we may also approximately determine their source by the form under which they appear, they are, for instance, finer and more longitudinal in phthisical laryngeal ulcerations than the bent and twisted lung fibres. According to Dettweiller-Meissen,[8] they occur in more than 80% of phthisical affections, and, from my rich experience, I can in general coincide with their statement that where elastic fibres

exist, the bacilli of Koch are also present. The presence of elastic fibres, which formerly was regarded as the most significant and positive criterion of pulmonary phthisis, must now, since Koch has given us an indisputable test in its place, be regarded merely as a diagnostic auxiliary.

With a little practice one is enabled even in the macroscopical examination of the sputum spread out in a thin layer, to surmise the existence of elastic fibres in the opaque dirty-white or reddish-yellow particles, which appear like bread-crumbs to the unaided eye. For the microscopical examination, the darkest spot is removed by means of two platinum needles and pressed between two cover-glasses; it is to be remembered that the elastic fibres have a tendency to be displaced toward the edge of the cover-glasses when pressed upon, in which situation they may be readily found. By the addition of dilute solutions of potash or strong acetic acid, which the elastic fibres are well known to resist, the remaining image is destroyed and the fibres appear the more distinctly prominent. If the entire expectorated mass is to be tested for elastic fibres, the sputum should be thoroughly agitated in double its quantity of distilled water, allowed to stand for twenty-four hours, carefully decanted, and after filtration, the precipitate examined; or the sputum may be boiled in a test-tube for several minutes, after the addition of an equal quantity of potash or soda lye, three or four times the quantity of water poured over it, and after twenty-four hours' standing, examined with the microscope under from 300 to 400 diameters. The double contour of the fibres prevents mistaking them for cotton fibres; in order not to mistake needles of fatty acids for elastic fibres, gentle heat should be resorted to, which melts them and transforms them into small globules.

FIG. 1.—Elastic fibres with pus-cells, finely granular detritus, and a large epithelial cell with nucleus from phthisical sputum. (Author's observation.)

FIBRINOUS COAGULA.

These formations, already described by the oldest observers as bronchial or fleshy polypi, were accorded their true position

in 1845 by Remak.[9] According to him, they are a product of inflammation, and appear, when the sputum is thrown upon water, as macroscopically visible, large or small grayish-white masses, more frequently composed of filaments rolled together; they form from the exudate by means of fermentative influences (death of white blood-corpuscles, fibrin ferment), and apply themselves loosely to the mucous membrane. We observe them in three forms of disease: croup, fibrinous bronchitis, and

FIG. 2.—Large bronchial coagulum from the sputum of a man æt. 54, having a healthy appearance and good appetite, who had coughed up these formations for six months. The varicose swellings are quite pronounced. Drawn from nature; full size. (Author's observation.)

fibrinous pneumonia. In general they appear as dichotomously branched, cylindrical, sometimes more flattened, at times more tubular, large and small coagula which float in water when containing air, but when freed of all frothy mucus and containing no air bubbles, they sink to the bottom. Rather numerous enlargements may frequently be seen in the smaller branches, which give the impression as if they originated in bronchiectasiæ (Fig. 2). According to Biermer, they are caused

by the exudate, during coagulation, meeting with obstacles, or by unequal quantities of coagulable exudate having been excreted in various situations. Their size is variable; according to the intensity of the process and the site of their formation, we meet with tree-like examples with widely-branching offshoots, which may reach a length of 12 cm., or short, non-branched and thinner ones. The former will come from the lower widely-ramifying branches of the bronchial tubes, the latter from the bronchi of the upper portions of the lungs.

The microscopical examination, which is best made with alcoholic preparations, shows us a mainly structureless material, in which principally mucus and pus-cells, but also fat-globules, are embedded. Fränkel succeeded in cultivating the streptococcus pyogenes (Rosenbach) from such a source. According to Riegel,[10] a lamellated structure, a concentric arrangement, may be recognized on transverse section, especially at the thicker portions, from which he concludes that the process takes place in stages; in one case of Kretschy,[11] four tender, transparent lamellæ of different thickness could be separated in an alcoholic preparation. Charcot was the first to describe colorless crystals in the form of double pyramids, which appeared in them, which closely resemble the asthma crystals of Leyden, or those found by Neumann in leucæmic spleens. They have also been repeatedly found by other investigators, recently in copious masses by Escherich, together with spirals (p. 115).

The chemical characteristics of these coagula indicate their belonging to the albuminoid substances; swelling in salt solutions and dilute acids to a mucoid mass, sparingly soluble in alkalies, particularly lime-water.

Crystalline Formations.

These always indicate that chemical processes have taken place in the pathological products of secretions. The following principally appear in the sputum:

1. *Needles of Fatty Acid* (margarin crystals).—These products, first observed and described by Virchow,[12] which occur wherever putrid decomposition of animal substance takes place, are "long spear-like crystals, always very narrow, occasionally varicose, frequently sharply bent and coiled upon them-

selves, colorless, not infrequently occurring in sheaves or thick bundles. Moistened with ether and warmed, they dissolve entirely, and on cooling are precipitated in drops as a white, acid fat, of a peculiar unpleasant odor. The entire mass being treated with iodine, becomes at first greenish-yellow, in the middle of the lump somewhat bluish, later, however, thoroughly brownish-yellow."

The varicosity of the crystals may, according to Traube, be used for a hasty differentiation of the former from elastic tissue; no less important, however, is their chemical reaction, in that, as above mentioned, margarin crystals are dissolved by ether, while elastic fibres remain unaffected. They are found in the sputum during putrefactive processes within the lung, in the shape of small shreds or plugs of dirty gray color, which, when rubbed with the fingers, disseminate a foul, cheesy odor. They are by no means rare.

Much rarer are

2. *Crystals of Cholesterin and Tyrosin.*—I have several times observed them in sputum from cavities, Biermer in empyema. They also most probably result from fatty metamorphosis of pus-corpusles, and are sufficiently characterized by their appearance. They consist of large, completely transparent, thin, rhombic plates, frequently so thin that one has to select the smallest possible diaphragm for examining them microscopically with 200 to 300 diameters in order to clearly discern their contour. The addition of boiling alcohol, ether, or petroleum dissolves them.

Their rare occurrence may be attributed to the fact that cholesterin, probably held in solution, is either dissolved in the protoplasm of the cellular elements or suspended in minute particles.

Tyrosin, the occurrence of which, in a purulent pleural exudate which perforated into the lung, is described by Leyden,[13] I believed I had once seen copiously crystallized in characteristic sheaf-like masses of needles, after long keeping and drying out, in the sputum of a young woman; the patient was the subject of advanced bacillary phthisis, with the formation of cavities (Fig. 4). The macroscopical view permitted hardly any doubt. A chemical investigation, with sulphuric acid, carbonate of barium, and a solution of chloride of iron, undertaken by myself and Dr. Hartmann, failed to give the usual

tyrosin reaction. Professor Meyer, of Marburg, who was kind enough to pursue a further chemical investigation, found that the crystals consisted of oxalate of ammonium.

The microscopical examination of the sputum in this case, in addition to numerous Koch bacilli, showed pus-cells undergoing fatty disintegration and innumerable bacteria (putrefactive organisms?). It may be surmised that the origin of such products of decomposition depended upon the existence of certain saprophytes.

Deserving of especial recognition are

3. Pointed octahedral *crystals*, first discerned by Charcot and Neumann in the spleen, in leucæmic blood and in bone marrow, later in human semen by Böttcher,[14] and in the sputum

Fig. 3.

Fig. 4.

Fig. 3.—Crystals of cholesterin with pus-cells from a phthisical sputum. (Author's observation.)

Fig. 4.—Oxalate of ammonium crystals, deposited from a phthisical sputum after prolonged keeping in a test-tube. (Author's observation.)

of *asthmatics* by Leyden.[15] If the glassy, often jelly-like, tough, grayish-white expectoration of asthmatics be examined, peculiar, whitish opaque balls, worm-like threads of coagula or plugs will be often noted in the colorless basis substance, the centre of which incloses more or less of a yellowish, sago-like, brittle body, which contains the crystals. These threads or strands have been very closely studied by Curschmann,[16] and were called spiral threads or spirals by that observer. They are 0.5 to 1 mm. thick, and 2 to 3 mm. long, very firm and resistant to the pressure of the cover glass, and often traversed by a thin central filament. According to Pel they dissolve in potash lye and baryta water, from which he concludes that they consist largely of mucous filaments. Cursch-

mann convinced himself by sections that they are hollow, and concludes that they are very probably bronchial discharges.

The crystals are generally very small, so that it is necessary to employ a higher (600 diameters) magnifying power, and to previously rub the sputum particles thoroughly between the cover-glass and slide. If the microscopical preparations are allowed to stand twenty-four to forty-eight hours, according to Eichhorst,[17] the crystals become much larger, but their number decreases. The large crystals not infrequently, per-

FIG. 5.—Asthma crystals from sputum. (After Leyden.)

haps in consequence of the pressure exerted, show defects, as if broken, which, according to A. Fränkel, points to a soft consistence (Fig. 5, right above). They are demonstrable for weeks afterward; they may also be colored by alcoholic solutions of aniline dyes (fuchsin), and may be preserved in xylol-Canada balsam.

Their chemical nature is not yet sufficiently established; they appear to be insoluble in ether, alcohol, and chloroform, but they disappear very rapidly on the addition of warm water, alkalies, and acids; they swell until they become invisi-

ble when brought in contact with glycerin. Oertel believes that micrococci lead to the disintegration of round cells, and that the crystals separate from the granular detritus. From the

FIG. 6.—Spiral fibres; *a*, magnified 80 diameters; *b*, a portion of *a* magnified 300 diameters. (After Leyden.)

investigations of Schreiner and Fürbinger it appears probable that they incite a decomposition of albuminoid bodies, and probably are related to ptomaïnes.

It should also be mentioned that no special pathological

significance for bronchial asthma can be attributed to these crystals, since we find them in the sputum of bronchial catarrh (bronchiolitis) and in the coagula of fibrinous bronchitis and in bacillary phthisis (Kaatzer), pleuritic exudations, and in the nasal mucus.

Whether we look upon them, as Leyden does, as an etiological factor in asthma owing to the mechanical irritation they exert on the sensory nerves of the bronchial mucous membrane; whether we regard them, with Curschmann, as the product of time, dependent upon the protracted stagnation of the sputa in the bronchioles and an accompanying regressive metamorphosis of the cellular elements—at any rate, we will have to adhere to the fact that they are among the almost constant concomitants of asthma.

Among the rarer substances found in the sputum are yet to be mentioned: crystals of hæmatoidin (small rhombic columns, of dark brownish-red color), oxalate of lime, and ammonio-magnesian phosphates (coffin and envelope shaped). All three formations have been frequently found in phthisical sputa. In the interesting cases of hæmatoidin sputa reported by Schulze, Friedreich, and Renz, tuft-like and needle forms predominated as compared with rhombs; these forms particularly have been described with great accuracy by Virchow. Ungar demonstrated oxalate of lime in characteristic octahedral form in the sputa of asthmatics, together with asthma crystals, during the time of the paroxysms.

Entozoa.

Of these, so far as is now known, only the ecchinococcus hominis appears in the expectoration. According to a compilation by Frey,[18] it was found in the lungs in 12%. If the ecchinococcus cyst ruptures and empties its contents into a bronchus or a cavity produced secondarily, a large quantity (as much as two quarts) of a clear, saline fluid of peculiar taste is expectorated, in which ecchinococci in the form of small, whitish-gray, gravel-like granules, as also daughter cells, or finally white membranes curled up on the borders may be macroscopically noticed; microscopically hydatid débris, hooklets, and scolices are discovered. The membranes show a lamellated texture, layers of regular parallel strips, which Jul. Vogel has compared to the oblique cross section of a book.

Remnants of Food.

During the transit of the expectoration through the mouth, the most diverse remnants of ingested food, etc., which have remained behind in the dental interstices, become mixed with it. Among these, the first to be mentioned are the residue of vegetables, potatoes (starch granules), bread crumbs, particles of caseous milk, snuff, etc.; of the animal articles of diet, are principally meat fibres, elastic fibres, free fats after indulgence in milk and emulsions, which surprise the observer of limited experience and may bring him to despair. Both forms have frequently led to error. It is therefore necessary that the observer should be familiar with the microscopical appearance especially of vegetable tissue and the granules of starch-flour, as they occur principally in potatoes and seeds. Vegetable cells are angular bodies placed side by side, which form rhombo-dodecahedra, and appear as six-cornered meshes on section; granules of starch are transparent, colorless bodies, presenting an appearance of concentric layers; they give the well-known iodine reaction.

All these admixtures will most frequently be found when the sputum comes from badly cleaned mouths, and on the bottom of the receptacle. The greatest possible cleanliness of mouth and teeth of patients should therefore be aimed at; the investigation is rendered easier thereby and time is saved.

Micro-organisms.

Fungoid formations in the expectoration are long well known. This applies to the leptothrix form, the thrush fungus, and some foul-smelling, smeary particles resembling the eggs of spiders, discussed more fully in another place, which are principally composed of micro-organisms. The presence of the latter has, for instance, given rise to the expression, "spider-cough," when speaking of the respiratory disturbance associated with them. Until recent times they were regarded as non-essential constituents, which accidentally became mixed with the expectoration from the coating of the tongue or the tonsillar crypts. The perfection of microscopical technique and the advanced state of artificial bacterial culture brought about by R. Koch made it possible for us to more correctly

interpret the importance of the discovered fungi to diseased processes, and to positively demonstrate their morphological and biological significance in the causation of certain affections.

First in importance are the bacilli of Koch, the presence of which in the sputum unqualifiedly proves that in such cases bacillary infection must exist. They belong to the class of schizomycetes (fissure fungi, from σχίζειν, to split), that is, that botanical species which perpetuate their species by division. They are true parasites, which can only thrive and multiply upon their host; they require a temperature of 86° F.; their existence is therefore bound to animal and human organisms, in which they go through the states of their development, in contradistinction to other bacteria, as, for instance, the bacilli of anthrax.

In length they are, according to Koch, about one-quarter to one-half the dimensions of red blood-corpuscles, about 0.0015 to 0.0035 mm.; extremities cannot be demonstrated. When stained, they appear thinner than when colorless. This is because before staining they must be viewed through refracted light, by which the lines of interference which appear at the borders apparently enlarge the diameters, while the investigation of the stained bacilli takes place under full light obtained from all sides, which excludes all phenomena of interference.

The little rods are in general not perfectly straight, but show slight kinks or bends. Their principal point of distinction from other, in form and size very similar micro-organisms consists in their great affinity for certain aniline colors, which the bacilli of Koch retain with great tenacity, so that they do not readily yield up their stain, even in the presence of strong acids, and retain it moreover astonishingly well when doubly-colored.

The originally so-called Koch-Ehrlich staining method, which, besides demonstrating the presence of these bacilli also proves their specific character, has been variously modified, but these changes do not embrace any differences in principle. Skill, light, instruments, staining fluids, etc., must above all be adequate.

The procedure as I now employ it, and which I have tested as the most feasible for practitioners, after an experience with it embracing thousands of sputum investigations, is as follows:[19]

I spread the first sputum voided in the morning, which I

caused the patient to expectorate into a wide test-tube fitted with a cork of absorbent cotton, upon a blackened plate or a piece of black paper, and remove by means of platinum needles, previously sterilized in a flame, the smallest particle from the darkest or seemingly most prolific spot. I place this between two cover-glasses (0.12 mm.), distribute it upon their surfaces, best by drawing them apart (not lifting them), or by gently rubbing them until dry, and put them aside to dry. I then proceed to prepare the necessary solutions.

After filling the bottom of a test-tube with aniline oil to the depth of about 2 cm., it is shaken for a minute with about ¾ of a test-tube of sterilized water and allowed to stand. This constitutes saturated aniline-water. [Distilled water should be cooked for a long time when it is to be used in bacteriological investigations, or should be sterilized in a sterilizing apparatus, because it always contains bacteria and germs.]

I prepare my staining fluid preferably of gentian violet, because it gives the clearest pictures; the gentian violet is added in excess to 95% (acid free) alcohol; generally about 20 to 25 gm. of coloring matter are required for 100 gm. of alcohol. This solution may be kept for months, while the aniline-water must be freshly prepared for each use. The aniline solution is then filtered through previously moistened filter-paper into a watch-glass, and from 12 to 15 drops of the staining fluid added to it by a drop counter or pipette and then stirred with a glass rod.

By this time the cover glasses containing the sputum are air-dry, and, after being slowly passed three times through an alcohol flame, sputum side up, are laid upon the aniline staining solution. They are either allowed to float here for twenty-four hours (longer does no harm), or I proceed, according to the time at disposal, with the completion of the examination. For this purpose I place the watch-glass containing the preparation upon a tripod with a wire net, under which a spirit flame of moderate intensity is placed, raise the fluid to the boiling point, and allow it to cool. I then begin the decolorizing. The solution which I prefer for this purpose consists of a mixture of 100 gm. of 90% alcohol, 20 gm. of distilled water, and 2 gm. of concentrated muriatic acid. I fill a deep capsule (No. 1) with this solution, a second one with 90% alcohol (No. 2), and a third with distilled water (No. 3.) After removing the cover-

glass with the forceps, I rinse it from one-half to one minute in No. 1, then just as long, or until the residue of the blue color has disappeared, in No. 2, and then in No. 3, constantly agitating while immersed in the different fluids.

If the double staining for differential diagnosis is to be at once utilized, which with greater experience may be dispensed with, the cover-glass, after becoming dry, may be immersed one-half to one minute in a filtered concentrated solution of vesuvin, mixed with water, and placed under the microscope with a drop of water. In such cases the bacilli of Koch will be doubly stained with violet and black, the remaining portions

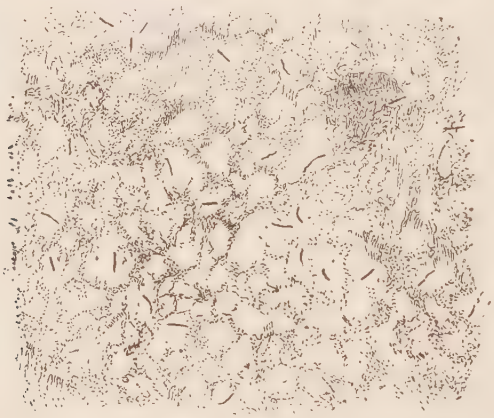


FIG. 7.—Dried sputum, prepared fifteen months previously, containing Koch's bacilli, which present partly a granular appearance. (Author's observation.)

of the preparation brown. The black color is due to the fact that the aniline brown absorbs the blue portion of the spectrum, and therefore a blue object viewed through a brown solution must appear black.

As regards the microscopical examination, I cannot concede the propriety of a test which is regarded as sufficient when made under a power of 300 to 400 diameters. Even though it may be possible for one familiar with bacilli to attain his object under such conditions, in general, and where small quantities of Koch bacilli are to be examined, apart from the great saving in time, it is a sine qua non for securing accurate data that an immersion lens and an Abbé condenser be used. As already mentioned, the bacilli when double stained appear as

dark violet to black little rods, which occasionally seem to be slightly bent or sharply cut off. Under very powerful enlargement (1,000 to 1,200), one is often able to note spore formation upon them. One then sees in the rodlets at regular intervals two to four egg-shaped uncolored portions, bordered on their edges by a finely-colored line.

Those rodlets which present a bead-like granular appearance (a phenomenon which is pretty frequently observed in stained Koch bacilli, and which was noted by me in the "Deutsche Med. Wochenschrift," No. 17, 1885), were formerly considered as bacilli containing spores. The spores, however, never become stained, but are marked, as above mentioned, in the colored rodlets as colorless round areas. The "granulation," according to my view, occurs most frequently in progressive ulcerative processes in the lungs, and is perhaps the expression of a granular disintegration of the bacilli, a special form of regressive metamorphosis. I have not been able to confirm a special influence of fresh concentrated nitric acid upon the development of this modification, as suggested by Voltolini.[20]

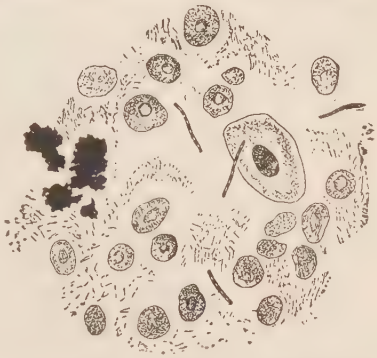


Fig. 8.—Portion of fresh phthisical sputum. Koch's bacilli with spores; the latter are unstained. Adjoining them are round cells; to the right above, a pavement epithelial cell with nucleus and nucleoli. The black portion on the left consists of inhaled dust (free pigment). × 1500. (Author's observation.)

If examined without double staining, the bacilli lie as unmistakable, sharply-defined, violet-black rodlets upon a faintly-tinged violet ground. An absolute decolorization cannot be brought about by the acid alcoholic solution; the preparation always retains a violet tinge, and in some fields of view shows, besides the bacilli, other deeply-stained violet bodies, for instance impurities, epidermal scales, etc. According to Celli and Garnieri, fine fat needles also occasionally appear, which react to staining agents almost exactly like the bacilli of Koch —"pseudo-bacilli." They may be readily distinguished, however, by their size and their reaction to ether and chloroform.

An almost perfect decolorization of the violet with substitution of a yellow tinge and at the same time an almost black

staining of the bacilli, may, in my experience, be attained by allowing the cover-glass to float ten to fifteen minutes in the compound iodine solution, after washing it in the acid solution and alcohol (Nos. 1 and 2), after which the superfluous fluid is to be removed with filter-paper and the preparation at once examined in pure glycerin.

If the preparations (gentian-vesuvin double stain) are to be preserved, they are to be mounted in Canada balsam, and, after they have dried, surrounded with a ring of lacquer. They may be kept for a considerable time. This keeping quality may perhaps be explained by my employing alcohol with muriatic acid instead of nitric acid (Ehrlich), and therefore support the statement of Brun,[21] according to which the nitric acid remaining in the preparation is the cause of the bacilli finally losing their coloring. Orth[22] declares that the bacilli become decolorized in Canada balsam because of the chloroform contained therein, and even attempts to explain in this way the non-success of Spina. I must contradict this opinion, because I can still demonstrate well-stained bacilli in preparations which I have kept for three years.

As regards the question, for how long the bacilli may be demonstrated in sputum containing them, I can say that I have been able to stain them in the dried sputa of phthisis almost four years old.

Occasionally I saw large cocci-like spots in sputum-preparations which had yielded up the violet staining; these bodies may be spores of other bacilli, resembling or like those observed and described by Gaffky[4] in intestinal phthisis.

The so-called pneumonococci, which have, according to the experiments of C. Friedländer,[23] a specific pathogenic action in the etiology of pneumonia, are another form of bacteria found in the sputum. Those cocci (single, diplo- and triplococci) are surrounded by a characteristic coating of a jelly-like substance with sharp external contour, which is more feebly stained in coloring dry preparations than the coccus itself. Their detection may be the most readily accomplished by the procedure recommended by C. Friedländer[23] in 1885; the preparations are to be drawn through a flame three times, and then immersed for several minutes in 1% acetic acid; after the latter has been removed by blowing through a glass tube with a pointed end, they are rapidly dried in the air, immersed for a few seconds

in a saturated solution of aniline-water and gentian violet, rinsed off with water, and examined with an immersion lens. It is then seen that the basis substance has remained completely or almost completely colorless, whereby the stained portions, among others the capsule, when it exists, appear more distinct. If the staining action has lasted too long, so that both are too intensely colored, decolorization may be carefully effected by means of dilute acetic acid or alcohol. Ac-

FIG. 9.—Portion of the rusty sputum of fibrinous pneumonia (4th day), from a man æt. 38. Preumonococci with distinct capsule; some in which it is not marked are in groups of one, two, and four. *a*, pavement epithelial cell inclosing numerous oral (?) cocci; *b*, rod-shaped bacteria: *c*, common bacillus from the oral cavity. (Author's observation.)

cording to the method of Ribbert,[24] with which I have no experience, the procedure is as follows: the preparation is brought in contact with a staining fluid resembling that used by Ehrlich[25] for coloring mast cells (100 gm. distilled water, 50 gm. alcohol, 12½ gm. acetic acid), saturated with dahlia while hot, then at once rinsed in water and examined with immersion lens and condenser. The cocci have a deep blue color, while the capsules are light blue.

If decolorization of the capsule occur during the technical

manipulations of the preparation, we have intensely colored cocci with colorless capsules before us. In such cases it will be difficult to decide by microscopic examination whether we are viewing Friedländer's pneumonococci or other encapsulated forms of schizomycetes, such as have long been recognized as round or elliptical cocci, and diplococci of the size of pneumonococci, and as I have encountered them, although isolated, in phthisical sputum. [While these sheets were passing through the press, I examined the sputum of a gentleman, æt. 75, affected with chronic bronchitis and bronchiectasia. The muco-purulent sputum at times (first morning expectoration) consisted of innumerable cocci, mostly in pairs, with a bright areola, and a few pavement epithelial cells inclosing large quantities of cocci and bacilli in swarms. Koch's bacilli were absent.]

Accordingly, only cocci with stained capsules can be accorded diagnostic value; the presence of elliptical cocci surrounded by light halos alone will not suffice. It should here be mentioned that Pasteur had already in 1881 experimented with such cocci with hyaline halos—petit halo transparent. As he found them in the sputum of a child suffering from hydrophobia, he erroneously claimed them to be pathogenic micro-organisms producing the virus of that affection.

The bodies first found in the sputum by R. Koch, and cultivated by Gaffky [4] and declared to be pathogenic micrococci, also belong here; they are generally surrounded by a rather distinct halo, and by their peculiar arrangement and almost regular grouping in fours, possess a certain similarity to sarcinæ (micrococcus tetragenes). In addition A. Fränkel [26] has shown that, besides the pneumonococci of Friedländer, other pathogenic micro-organisms exist in the sputum, of which one closely resembles the former coccus in morphological characteristics and possesses the same capsular formation. Because of its experimentally established blood poisoning and deadly influence he called it the coccus of sputum-septicæmia. [If inoculations be made with such sputum, after being rubbed up with four or five times its quantity of water and filtered through linen or gauze, before injection into a number of rabbits, some of them regularly die within twenty-four to forty-eight hours. In the blood of these animals, this septicæmia coccus is exclusively found, often in immense numbers. Those

inoculated with the blood of the dead animals show the same parasitical affection.] Another form may be biologically distinguished from Friedländer's cocci, in that it requires a higher temperature for cultivation, that it possesses a short-lived vitality of but a few days, and that it is easy to deprive it of pathogenic properties by the influence of temperature (107.6° F.).

Of other micro-organisms in the sputum, which are either not pathogenic or the infectious nature of which is doubtful, the various forms of schizomycetes remain to be mentioned, which vegetate in the oral cavity and its adnexa, in the coating of the tongue, the tonsillar crypts, dental deposits, etc., as well as the putrefactive organisms which develop in stagnating sputum. These are found in almost every preparation, and

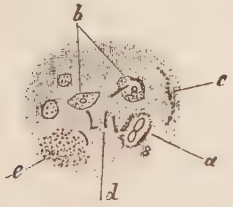


FIG. 10.

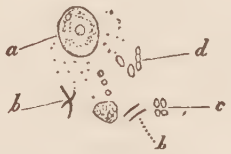


FIG. 11.

FIG. 10.—Pneumonic sputum kept two days. *a*, pneumonoccocus with bright halo; *b*, epithelial cells with nucleus and granular contents, one containing a Koch bacillus; *c*, chain of cocci; *d*, Koch bacilli; *e*, zoöglœa swarm. (Author'sobservation.)

FIG. 11.—Portion of fresh phthisical sputum. *a*, alveolar epithelial cell; *b*, Koch's bacilli; *c*, coccus tetragenes, resembling sarcina; *d*, oval cocci. (Author's observation.)

some of them have given rise to the most daring conclusions. I call attention only to the comma bacilli found in the mucus of the mouth by Lewis,[27] declared by him to be identical with the cholera bacilli of Koch, which are frequently present in great quantities in hyperæmia of the gums, caries, etc. A fungus defined by Miller[28] as ε-shaped, often forming the letter S by lying in apposition, is according to Flügge[29] probably identical with the Finkler-Prior spirillum. In addition, Miller has in recent years isolated 25 diverse forms in the mucus of the mouth, 12 cocci and 13 bacilli, the characteristics of each of which have not yet been completely reported.

For differentiating these numerous forms of bacteria, morphologically so similar, and for determining their essential biological and infectious properties, only examinations in pure cultures and experiments upon animals can serve as a guide.

Regarding certain putrefactive micro-organisms, Rosenbach[30] succeeded in finding one and the same microbe in the mucous plugs in recesses of the mucous membrane of the lateral pharyngeal wall while in a catarrhal state, the almost insufferably fetid odor of which prompted him to undertake an investigation. He calls it the bacillus saprogenes No. 1. He considers this bacillus a decidedly putrefactive microbe, possibly the most widely diffused, which in itself produces the odor of decomposition, even in those cases where it cannot penetrate its host and decompose it.

If sputum from a cavity be allowed to stand for some time, it becomes, as I have now and then observed, light green in its upper layer and resembles in odor trimethylamine. Similar observations were made by Curschmann and Escherich in the sputa of asthma and fibrinous bronchitis. Doubtless the green color and the odor are the product of saprophytic bacteria which produce coloring matters and in addition cause a more complete decomposition of albuminoid substances. We have the best-known analogue in the fungus of greenish-blue pus (bacillus pyocyaneus). A fungus was cultivated at the Göttingen Hygienic Institute from a putrefying substratum, which caused the green color and the odor mentioned (bacillus fluorescens patidus, Flügge[29]).

As better-known fungi which occur in the sputum I have to mention the species of leptothrix, the most frequent forms of which have been called leptothrix buccalis, and oïdium albicans. The former, according to Flügge[29] consists only of a developmental form, a filamentous condition of certain bacilli, in the formation of which others, for instance, the bacillus butyricus or some anaerobic bacteria, not rarely participate. The common property of both, under certain circumstances, of forming a blue, darkening to violet-black colored combination with iodine (granulose) in the plasma would seem to substantiate this. According to Leber, this coloration ensues upon the simultaneous addition of very dilute muriatic acid, acetic acid, or lactic acid to iodine. If acid reaction already exists in the medium, the addition of iodine alone will suffice. Under the microscope leptothrix are seen as very long, thin and apparently non-segmented filaments, often united in close bundles or felted masses, either alone or in company with spirochæte from the gum-mucus, most often embedded in thick

masses of micrococci. Pure cultures of the leptothrix buccalis have so far not been effected.

The thrush fungus, saccharomyces albicans (formerly described as the oïdium albicans) is, according to Grawitz [31] identical with S. mycoderma. It occurs most frequently as "thrush" in the saliva of children who are fed with milk; also in adults very much reduced in nutrition, and in phthisical subjects. It consists of partly globular, partly oval or cylin-

FIG. 12.—I. Leptothrix buccalis. II. Saccharomyces albicans (thrush fungus) with sprouting colonies. (Author's observation.)

drical cells, which proliferate, according to the abundance of saccharine matters, into long filaments; between these are copious oval spores, which form between the epithelial cells of the uppermost layers and upon the surface itself, a mycelium of varying thickness.

Both of these, the leptothrix and the S. mycoderma, have been found in large quantities in the sputum of putrid bronchitis, so that it appears probable that, having gained entrance to the lungs by respiration, they had there multiplied in a stagnant secretion (cavities).

Of other fungi, which interest more from their incidental occurrence in the sputum, a few varieties of the aspergillus must be mentioned. Guided by experiments upon animals,[4] according to which birds (doves, geese, and smaller birds) which were exposed for several minutes to an air filled with spores of aspergillus fumigatus, died of pneumonia within five days, one does not go astray in attributing their influence to the causation of pneumonic focal diseases.

Fig. 13.—Eurotium Aspergillus glaucus. × 300. (After Liebermann.)

Herterich found in the gray hard sputum particles of a patient with redness and excoriations in the trachea mycelium formations of eurotium aspergillus. Rother also reported a case from Leyden's clinic, in which a patient with symptoms of infiltration of the pulmonary apices, occasionally expectorated grayish-green crumbs of an asbestos-like lustre, which upon microscopical examination proved to be necrotic shreds of pulmonary tissue, copiously traversed by the stems and heads of the aspergillus. The case recovered. In general, however, it is reasonable to believe that a number of the schizomycetes and mycelium forms, often found abundantly in the sputum, are simple saprophytes, and have no etiological significance.

CHAPTER IV.

CHEMISTRY OF THE SPUTUM.

I. Organic Constituents.

Water.—Water forms the main constituent of the sputum, and exists, acording to Biermer, in the proportion of 882 to 979 per thousand; it depends chiefly upon the character of the diseased process, the composition of the secretion caused by it, and upon the quantity of saliva which becomes associated with the expectoration in the mouth. The sputum is most rich in water in pulmonary œdema (serous sputum); then come the sputa in the first stages of a catarrh, in emphysema, and the bronchial affections associated with convulsive paroxysms of cough.

Albumin and Sugar.—Next to water, albuminoid bodies predominate in the formation of the sputum, in that they are more or less abundantly present dissolved in water, according to the inflammatory grade of the affection of the mucous membrane.

The sputum in the last stages of phthisis contains the greatest quantities of albumin; bronchitis, the least. According to the investigations of Bokay,[32] serum-albumin and globulin are generally sparsely present; an albuminous material resembling myosin is, on the other hand, generally abundant. It appears from the analyses of Bokay, that the quantity of albumin depends upon the number of formed elements. As regards myosin we know that very probably it is formed by the death of the protoplasm, in like manner as we may observe it on the cessation of muscular contractility in cadaveric rigidity. Albumin may be detected in the sputum by acidifying it strongly with acetic acid, adding about a like volume of concentrated saline solutions (table salt, sulphate of soda or magnesia), and then heating to boiling. A white flocculent precipitate forms if albumin be present.

It might occasionally prove of diagnostic value to test the sputum for sugar, which is known to be present in diabetes and ecchinococcus. The above albumin test may be utilized for this purpose; the albumin coagulum is merely filtered off, and the filtrate treated with Fehling's solution in the usual way.

Mucin.—Mucin, a derivative of the albuminoids, is a product of the secretory cells of the mucous glands (mucous cells). It gives the sputum its tenacious, glassy, often jelly-like, colorless character, and is mingled with the sputum in considerable quantities from the mucous glands of the mouth; microscopically it appears amorphous. Its detection in the sputum may be accomplished by alcohol, extraction of the coagulum with hot water, and filtration; the filtrate is to be mixed with several drops of acetic acid; this causes a turbidity or precipitate insoluble in the excess; by this and by the sparsity of the precipitate on the addition of ferrocyanide of potassium mucin may be distinguished from the other albuminous substances resembling it (Biermer).

Fats.—The appearance of fats in the form of crystals and granules and their microscopical detection has already been mentioned. The microscopical demonstration allows us to approximately estimate the quantitative relationship.

Pathology gives us the conditions for the amount of the fatty contents. The larger the quantity of pus in the sputum, the more advanced the regressive metamorphosis of the cells, the sparser will the quantity of nutritive fluid intended for the cells become, and the more intense, of course, will be the fatty degeneration of the pulmonary tissue. The fatty contents of the sputum are thus the standard of the lack of nutriment of the pulmonary parenchyma. The appearance of free fatty acids in the sputum leads us to infer that they are formed either in the cheesy foci of the diseased lungs or in the air-passages, and that their quantity is determined by the existence, disintegration, and dissemination of caseous infiltration, and further by the consistence and adhesiveness of the sputum, which, again, induces the stagnation of the latter, and, finally, by the fact that the atmosphere in which the patients breathe is relatively more or less infected with micro-organisms.

Concerning their chemical detection, reference must be made to the physiological and pathological chemical analyses of Hoppe-Seyler.[33]

II. Inorganic Constituents.

These appear in the sputum either in combination with morphological formations, or in free solution, or solid masses, concretions (pulmonary calculi). From the comparative investigations of Bamberger [34] and Kussmaul [35] regarding the ash constituents of the sputum, it appears that chlorine, sulphuric acid, phosphoric acid, potassium, sodium, lime, magnesia, salts of ferrous oxide and silicon compounds are present in it, and that a few substances such as phosphoric acid and potassium, show a variable behavior. The maximum of phosphoric acid in the ash of sputum was 14%; in catarrhal and purulent sputum from 10% to 13% was found; on the other hand, the ash of lungs showed a constant, very large proportion of phosphoric acid, 36% to 48%. While the potassium in lung ash played a very subordinate rôle, in catarrhal purulent sputa 16% to 24% was found.

With regard to concretions, which to the great astonishment of the patient are from time to time voided with the sputum, they may be of various origin. They are formed in the lungs, when a further metamorphosis, particularly a softening, is retarded in the smaller cheesy foci by the copious deposition of phosphate and carbonate of lime. If a purulent inflammation develops in the neighborhood of this foreign body, which acts as an irritant, and if this finds an avenue to a sufficiently large bronchial tube, it becomes expectorated with the sputum. Under similar circumstances, concretions form in the mouth upon the teeth (tartar) and the tonsils, the products of which appear as admixtures in the sputum. It is of especial interest, that according to the investigations of Klebs,[36] the tonsillar concretions are apparently to be attributed to the action of bacteria; they appeared as several mixtures of inorganic and organic constituents; upon dissolving the lime salts, compact heaps of bacteria remained as the organic basis. Neuwerk succeeded in detecting the bacilli of Koch in pulmonary concretions.

CHAPTER V.

THE CLASSIFICATION OF THE SPUTUM.

I. The Mucous Sputum.

This is feebly transparent, of a glassy, colorless, or faintly whitish-gray appearance, can be drawn into strands and is so adherent to the vessel that the latter may be inverted without losing its contents (sputum crudum of the ancients). It loses its purely mucous character by the copious admixture of fluids and air bubbles; this may occur when the salivary secretion is mechanically increased by violent coughing fits and is mingled with the sputum, or when, as in pulmonary œdema, the alveoli and bronchi participate in the expectoration by a sero-mucous exudate.

The main constituent is mucin, the secretory product of the mucous glands of the mouth and bronchial tubes.

Microscopically we find very few formed elements in the mucus. On the addition of acetic acid, a whey-like veiled cloudiness becomes apparent, induced by coagulation of the mucin (mucin reaction). The mucous bodies sparsely embedded in the mucus are somewhat larger than pus-corpuscles, with one or two round nuclei, granular contents, and sharply defined surrounding membrane; excess of acetic acid renders them transparent and the nuclei particularly prominent. The granular contents never become stained by basic aniline dyes because of the mucin, while the nuclei take up the coloring matter with avidity.

In addition, mucous watery sputum contains as accidental and non-essential constituents, epithelioid cells, among which pavement epithelium from the oral cavity and sometimes the vocal cords is frequent, and rarely isolated ciliated epithelial cells from the bronchial mucous membrane, with and without ciliæ.

II. The Muco-purulent and Purulent-mucoid Sputum.

By a more or less copious admixture of mucus and pus-corpuscles as observed in long-standing bronchial catarrh, the sputum loses its watery color, it becomes thickish, of a whey-like white to whitish gray and yellowish cloudiness (sputum coctum of the ancients), non-transparent in areas, so much so that opaque puriform points or streaks of varying size may be distinguished next the clear spots containing mucin (homogeneous sputum). If the proportion of pus-corpuscles preponderates (purulent-mucoid sputum), the sputum appears as a yellow or yellowish-green opaque mass which has no tendency to adhere to the vessel, sinks to the bottom in separate portions, taking on globular, so-called nummular forms, and in this manner may constitute the greater part of the total expectorated mass (heterogeneous sputum). This form gives a diagnostic characteristic of bronchiectasis and cavities. In the first-mentioned affection, great masses of such sputum (several quarts daily) are expectorated, which peculiarly recall the odor of acid perspiration. The expectoration takes place in gushes, very often on changes of position, and fills the entire mouth (mouthful expectoration of Wintrich). The latter is never the case in phthisial cavities.

After the purulent-mucoid sputum has stood for some time, it has a tendency to confluence, and then generally forms three layers, which are explained by the difference in specific gravity. The upper layer is muco-purulent, frothy and filled with air bubbles, after longer standing of yellowish-green or dirty-green color (bacterial action, p. 126); the middle layer is richer in water, mixed with dirty-white mucous flakes containing albumin, which float about freely; the lowest heaviest layer is thicker, purulent, and forms a homogeneous sediment, which consists chiefly of partly swollen, partly fatty or granular disintegrated round cells and the most diverse bacteria.

If black, brown-streaked, dung-colored decomposed masses are mingled with the sputum, which appear microscopically as a conglomeration of elastic fibres (signs of ulcerative processes in the wall of the ectasis), crystals of fat, fat-globules and putrefactive bacteria, then it is in general not lacking in a pronounced odor, which points to a protracted sojourn in the bronchial cavities, or mycotic decomposition and destruction

of the tissue and vessels. Schultze[37] in a rare case found therein copious quantities of hæmatoidin crystals in the form of single needles, bundles of needles, and rhombic plates (p. 115). As evidence that these putrefactive processes have taken place in the lungs, freshly-voided sputum is requisite, as well as experiments with other sputa, which after several weeks' standing yield the same products.

Chemical examination of these masses gives putrefactive products of albuminoid substances (sulphuretted hydrogen, ammonia, leucin, and tyrosin), partly products of the decomposition of neutral fats, copious fatty acids, and traces of glycerin. Escherich[17] has several times demonstrated a peptonizing ferment resembling trypsin.

III. The Purely Purulent Sputum.

Purely purulent sputum only comes to our view when the contents of a pulmonary abscess or an empyema cavity have been evacuated. It does not differ from ordinary abscess pus, and like this separates into two layers, one of which contains the pus-serum, the other the cellular constituents. Microscopically it consists of round cells, partly in a state of fatty degeneration, pus cocci, and bacteria.

IV. The Bloody Sputum.

Bloody sputum shows all shades of color, from a light yellow to dark black, according to the quantity of blood-discs mingled with the expectoration. The form and grade of the blood admixture leads us to speak of sputum streaked with blood, sputum spotted with blood, sputum intimately mixed with blood, and pure bloody sputum.

The two first-mentioned characters obtain in acute and milder forms of chronic catarrhal inflammation of the bronchial mucous membrane and the pulmonary parenchyma; they appear not rarely as accidental admixtures from the gums, or in general disturbances of nutrition and infectious diseases.

The so-called vicarious bronchial hemorrhages and the sputum of fibrinous pneumonia also belong in this class.

The more profuse hemorrhages characterize attacks of hæmoptysis in phthisis, gangrene of the lungs, abscess of the

lungs, and the so-called hæmoptysis nervosa, as occasionally observed in the mentally deranged and in nervous disturbances of the cerebrum and spinal cord; in ecchinococci of the lungs also, the expectoration of their fragments is not infrequently preceded by hæmoptysis. An especial form of hæmoptysis of parasitic nature appears to occur among the Japanese, in the sputum of which Bälz [17] found psorosperms. He calls the disease itself Gregarinosis pulmonalis.

The quantity of the bloody sputum may vary greatly. It will not be easy to draw differential diagnostic inferences therefrom for pulmonary or gastric hemorrhage in case the history of the case is of doubtful character. The quantity of the blood coming from the lungs is generally less than when coming from the stomach. The composition of the blood-coagulum may also be of importance; the latter becomes more perfect and aërated in pulmonary hemorrhage, where the blood comes in contact with the acid stomach contents, so that on section, small hollow air-containing cavities are encountered, which may give it a porous, sponge-like appearance and greater lightness. For the same reason, the blood in hæmoptysis will have an alkaline reaction, while in hæmatemesis, especially if it be not too profuse, it will show a neutral or acid reaction.

Of less practical importance is the question whether the quantity of the blood voided may point to a capillary or an arterial source. To definitely determine the origin of a hemorrhage from the respiratory tract will not always be possible.

In the most fortunate case, the microscopical examination of the sputum will give data regarding the nature of the process (*e.g.*, the Koch bacilli), but not the locality of the hemorrhage. Its most frequent site is in the pulmonary parenchyma and the bronchi. It has frequently been my experience at health resorts to have patients bring me their bloody sputum in great trepidation, while their history, as well as the physical examination of the lungs and the microscopical scrutiny of the expectoration, gave negative results. Later continued observation of the patients in question confirmed my conclusion, that in these cases we had to do with a mechanical admixture of blood from the mucous membrane of the mouth, the nose, and the pharynx by the probably violent efforts at expectoration, or, else, that loose, readily-bleeding gums un-

consciously sucked during the night, were the source of the sanguineous admixture. Such frightened patients are often hard to convince of the innocent nature of their bloody sputum. In general, however, the smallest bloody admixture requires careful, continued observation. It occurs not very rarely, that sputa for days at a time colored with blood are but the precursors of a severe hæmoptysis. The therapeutic measures (physical and mental rest, ice-bags) acquire increased importance for this reason.

The assertion has been made by French authors that it is possible to recognize a hemorrhagic infarction by the peculiar acid, garlicky odor of bloody sputa, calling to mind the smell of tincture of radishes; so far as I know, this diagnostic critical refinement has not yet found confirmation in Germany.

CHAPTER VI.

THE SPUTUM IN SPECIAL DISEASES OF THE RESPIRATORY APPARATUS.

I. The Sputum in Bacillary Phthisis.

It has always been the endeavor of practitioners to utilize the expectoration as a diagnostic criterion in the clinical history of phthisis.

It was believed that the granular, half-soft, crumbling masses in the sputum were to be regarded as tubercular material; but Laennec[38] had already called attention to the fact that these originated in the mucous follicles of the tonsils, and differ from tubercular material in that, when rubbed between the fingers, they give off a fetid odor, and when warmed upon paper leave fatty stains.

To Virchow is due the credit of having demonstrated that the tubercular material is nothing else but caseous masses, the product of regressive metamorphosis, which may attack the various tissues in different parts of the organism. That the so-called tuberculous material contained a specific virus was not yet known.

The reports of autopsies by Buhl, in 1857, first pointed out that phthisis was brought about by the absorption of a virus existing in the primary cheesy foci, and Niemeyer succeeded in utilizing this view, if in somewhat modified form, for clinical purposes. Experiments did much toward clearing up this question. After Villemin, Klebs, Cohnheim, and Damsch had succeeded in producing the so-called inoculation-tuberculosis by inoculations of tubercular masses upon animals, and, in 1877, Tappeiner and Schweninger had caused tuberculosis of the lungs by the inhalation of finely powdered phthisical sputum, the study of the infectious nature of tuberculosis was placed upon a firm basis; there could hardly be any doubt

that the tubercular substances contained an organized uniform infectious matter. The nature of this matter was surmised by Klebs in 1877; and he gave the weight of his authority to the micro-parasitic character of tuberculosis. Although the existence of such a micro-parasitic virus would readily explain the nature of tuberculosis, it remained for R. Koch to furnish incontrovertible evidence thereof by the discovery of the Koch bacilli. The experiments of Koch, essentially pursued in the field of pathological anatomy, I may premise, have been confirmed by all investigators who have tested the question of tuberculosis *sine ira et studio* in this direction. The experiences and observations which clinical research derived from the new dogma, have called forth a copious literature, which principally discusses the diagnostic significance of the Koch bacilli in the etiology of phthisis, and thereby testify to the overwhelming practical importance of the detection of these infection-producers. The results of the more important works almost unanimously agree that the detection of these bacilli in the sputum not only establishes an infallible sign of the existence of a bacillary affection of the respiratory organs, but is also capable of making a certain diagnosis possible in initial processes before the physical signs are of any value, and to remove all doubts in uncertain cases of differential diagnosis. The few works, which, probably by reason of insufficient technique in investigation, arrive at different contrary conclusions, cannot be regarded as objectively critical productions. Even Brehmer,[39] who felt compelled to dispute the exclusively bacillary nature of phthisis in the light of his clinical observations, in his latest paper, "Die Terapie der chronischen Lungenschwindsucht," expresses himself on this point in the following language: "I place the utmost weight upon the tubercle bacilli, in proving the existence of phthisis. I may even say, more than any physician (? K.); for almost all physicians occupy a purely platonic position [I hope not!] regarding the tubercle bacillus and its relations to pulmonary phthisis, although they recognize it as the cause of tuberculosis. In practice one does not hesitate to declare an individual healthy, without having examined the sputum, because the physical examination of the lungs has proved negative. Phthisical patients are declared to be cured, likewise without the sputum being examined for tubercle bacilli and proof obtained that

such no longer exist. [I unfortunately must confirm this statement.—K.]

"But can we still say consumption of the lungs is cured without having demonstrated, besides other signs, that the tubercle bacillus has gradually become less in quantity in the expectoration and finally permanently dsappeared? According to my view, such evidence is now a condition sine qua non; for what is the meaning of the doctrine: the tubercle bacillus is the cause of phthisis, and where the bacillus exists, there tuberculosis is present? The appearance of health is very relative, therefore very deceptive; it does not warrant us to consider the individual healthy. The residues, which, according to the severity of the case, remain behind, give grounds for the hope of recovery; but they do not justify our declaring the patient to be cured, so long as the sputum has not for months been free from bacilli and remained so. Only on these conditions do I declare a consumptive to be cured" (see p. 100).

The results of the investigations which I have made in a copious material of pulmonary diseases since 1883, and which in part coincide with the special investigations of Balmer and Fräntzel, Gaffky, Dettweiler, and others, are briefly stated in the following:

1. In the sputum of every consumptive (the greatest certainty is obtained by using the first sputum voided in the morning), the bacilli of Koch may be demonstrated.

2. If after repeated examinations, made at different times and as before described (p. 118), one is not able to demonstrate the tubercle bacilli, the bacillary nature of the pulmonary affection may be excluded.

3. The quantity of bacilli in the sputum does not stand in an absolute relationship to the severity of the affection (see "Miliartuberculose und Larynxphthisis").

4. Rises of temperature do not demonstrably cause an increase in the number of bacilli in the sputum, and vice versa.

5. The quantity of the bacilli does not possess any prognostic value.

6. A cure is begun or completed with the temporary or permanent disappearance of the Koch bacilli from the sputum.

Next to the tubercle bacilli, it is the elastic fibres, the débris of the destroyed pulmonary tissue, which occupy our attention in the sputum of consumptives; concerning these,

the reader is referred to the portion of the work devoted to them (p. 108).

The chemical examination of phthisical sputum has as yet not yielded any material disclosures; it shows a composition similar to the sputa of catarrhal bronchitis, but shows, as regards the difference in the proportions, a dissimilar division of its constituents. The lowest figures were encountered in bronchitis, the highest in the last stages of phthisis. The investigations of Renk, which embraced three consumptives in the middle stage of the disease, who voided a mean average of 125 gm. per day, gave the following composition of the sputum: water 24 to 25 parts; solid constituents, 5 to 6; among the organic portions of which were 1.80 to 2.84 of mucin, 1.26 to 2 extractive matters, 0.2 to 0.49 albumin, and 0.30 to 0.52 fats; of the inorganic, 0.76 to 0.90 of chloride of sodium and phosphate.

Bokay [32] determined quantitatively the organic substances in the sputum of a patient in an advanced stage of phthisis, and obtained the following figures:

The quantity voided in twenty-four hours was 25 to 30 cubic centimetres; (*a*) fatty acids of the fats, 0.9725%; (*b*) free fatty acids, 0.0902%; (*c*) fatty acids combined with an alkali, 0.3973%; (*d*) cholesterin, 0.0141%; (*e*) lecithin, 0.1245%; (*f*) nuclein, 0.0489%; (*g*) albuminous substances (serum-albumin and globulin, sparse; myosin, copious), 0.5115%.

The other diagnostic criteria of phthisical sputa, which were formerly regarded as essential, have been relegated to a subordinate position since the discovery of the tubercle bacillus; they can, however, aid us in determining the extent of the phthisical process. The gross scrutiny of the sputum, its extensive, lumpy form (nummular sputum), its uneven, eroded-looking upper layer, its quantity and color, its weight (*sputa fundum petentia*), etc., will give the practitioner additional information of the state of the disease.

Acute miliary tuberculosis, with rare exceptions, yields either no expectoration at all, or only a sparse, colorless, tenacious secretion.

II. The Sputum in Pneumonia.

The surmise that pneumonia, like other infectious diseases, is due to an individual poison, has for more than a dec-

ade been brought to our attention by clinical observation. The development of bacteriological research, and the perfection of the culture methods introduced by R. Koch, and the experiments upon animals associated therewith, would justify the expectation of an elucidation in this field of investigation.

In 1883 Friedländer and Frobenius succeeded, in a large number of cases of pneumonia, in finding and isolating those micro-organisms which I have described in detail in another place, and to which the rôle of producing pneumonia appears to belong. Although strict proof of their being the sole cause has not yet been obtained, and though, according to the researches of other observers, it appears as if besides the encapsulated Friedländer cocci, other organisms have similar pathogenic action, the clinical observations of Ruehle,[40] Ribbert,[24] Emmerich,[41] and others justify the assumption that pneumonia is an infectious disease. How far in a concrete case the Friedländer coccus alone forms the etiological factor, or whether other pathogenic micro-organisms participate in its action, and in what way, it is for the present not possible to decide.

It has already been mentioned that A. Fränkel isolated from pneumonic sputum a coccus differing both morphologically and biologically from the Friedländer coccus, by the inoculation of which he produced hepatization. Guttmann, who succeeded in cultivating a third coccus, differing from the preceding, does not consider the number of pneumonia-producers thereby exhausted. The future will have to furnish further enlightenment. It would be a very daring procedure, as recommended and practised by Ruehle, to base a diagnosis of pneumonia alone upon the presence of pneumonococci in the sputum, since similar-looking cocci undoubtedly appear in the sputa of individuals not affected with pneumonia; other data, as we shall see, are here much more important.

With a little practice in the technique of staining, one may readily succeed, in the incipient stage of the disease, in detecting Friedländer's cocci (bacilli, Flügge), especially in the fibrinous-exudate portions of the blood-colored sputum.

If we regard the sputa of a normally progressing fibrinous pneumonia according to its clinical stages, we must separately discuss the three types entering into it. As a rule they possess such characteristic signs as to alone suffice, in general, for the diagnosis.

In the first stage, that of engorgement, the sputa, if present, are very scanty, tenacious, adhesive from the large amount of mucin and albumin contained in them, confluent, transparent, and aërated, and forming a frothy layer upon water. The quantity of contained blood is generally moderate, the mixture not intimate, and indicated by a light-yellow color. These peculiarities are of considerable diagnostic import when the local symptoms are still ill defined, and the physical signs are lacking, as, *e.g.*, in the case of a central lobular pneumonia. Microscopically we see larger or smaller quantities of blood-discs in various pathological forms, occurring either singly in the field of view or lying in apposition, more with their borders than their surfaces.

If the pneumonia, after the third or fourth day, has entered upon the stage of progressive exudation, in which the exudate coagulates, the blood-corpuscles disintegrate, and the coloring matter of the blood emulsifies, in varying quantity in the mucus, then the sputa, owing to the increase of their morphological constituents (blood-discs and leucocytes), become opaque and tenacious. Patients expectorate the tough mucus with great exertion; the sputum possesses such coherence that it sticks to the vessel, and on the inversion of the latter remains upon its bottom, or the whole tenacious mass escapes in a lump. The intimate admixture of blood imparts to the sputum the appearance which has given it the name rusty or brick-colored. The shade of color is due to the quantity of the admixed blood-discs and their chemical transformation, further by the length of time the blood has remained in the alveoli or the fine bronchi. The brownish or even black sputa of older date has been compared to prune or licorice juice.

Besides these characteristic colors of pneumonic sputum, the latter may present still another appearance, *e.g.*, the grass-green color in so-called bilious pneumonia. This color is caused either by the addition of bile pigment and the transformation of cholepyrrhin into biliverdin by the access of atmospheric oxygen, or it is the green coloring matter of the last stage of oxidation of hæmatin similar to that occurring in contusions, where the same play of colors takes place under the skin. In the first case the bile pigment may be detected in the sputum by the familiar reactions. (The addition of a

few drops of fuming nitric acid induces the usual reaction, although it is not so distinct.) Traube and Nothnagel have, however, encountered grass-green sputa in pneumonia not complicated by icterus, which ended in lysis instead of crisis.

Saffron-colored sputum does not always, as Eichhorst [17] con-

FIG. 14.—Large fibrinous coagulum from the rusty sputum of the patient referred to in Fig. 9; it was expectorated on the fifth day of the disease. (Author's observation.)

tends, point to the beginning of resolution; I have seen the light-yellow saffron color complicate the serous sputum of pulmonary œdema in a case of pneumonia which terminated fatally.

To what extent special micro-organisms (masses of lepto-

thrix?) are implicated in the coloring of the sputum must be elucidated by future investigations.

On examining the sputum microscopically, many red, partly disintegrated blood-discs and numerous pus-cells mixed with a few epithelial cells will be seen. With a little attention and patience it will not be difficult, under low powers, to find the dendritic branchings of the above-described fibrinous casts of the bronchioles, which on the addition of acetic acid swell up like jelly. The pneumonococcus generally lodges in these.

Another certainly rare discovery in pneumonic sputa are Curschmann's spirals (p. 115). Curschmann found them four times in twenty-seven cases of pneumonia, twice associated with fibrinous coagula. According to him, they result from two causes: either the individual has for some time suffered from chronic exudative bronchiolitis, which was superseded by acute fibrinous pneumonia, and shows the spirals in the rusty sputum as evidence of the chronic affection; or the individual was previously healthy, the exudative bronchitis accompanying the pneumonia as an acute affection, to disappear with the latter. The spirals are then generally very sparse and never contain crystals.

If the pneumonia tend toward a favorable termination, the sputum of the last stage will correspondingly change. The rusty color disappears, the sputum becomes lighter, more opaque, the fibrinous coagula are wanting or suffer fatty disintegration, the coherence diminishes, it "rolls" in the vessel and resembles almost completely the muco-purulent character of the expectoration in bronchial catarrh. The expectoration ceases entirely with recovery, or, more frequently, a short time after recovery a sparse, mucoid bronchial secretion is voided. If the pneumonia terminates fatally with the supervention of pulmonary œdema, the sputum toward the close becomes thready or sero-sanguinolent, thin-fluid, and of "dark, dirty, red-brown" color (Guttmann). It contains many air-bubbles in the form of froth, blood constituents, blood-discs, and serum, and has been designated as prune-juice (jus de pruneaux) by French authorities.

Quantitative and qualitative modifications in the character of the sputum are frequently produced by divergences from the normal course of pneumonia; in children and insane individuals, who swallow it, the sputum is lacking entirely, or the

color, consistence, etc., from the onset require a prognosis differing from the usual.

Abnormal terminations of pneumonia may also cause essential anomalies in the sputum.

If the pneumonia merges into the formation of abscesses, the deviations of the sputum from the normal alone suffice for diagnostic criteria. Considerable quantities of perfectly purulent sputa, intermixed with elastic fibres, and not seldom with hæmatoidin crystals paroxysmally voided, will admit no doubt in the matter.

If under special, still obscure circumstances (parasitic infectious influences?) the affection terminates in pulmonary gangrene, the sputa, mingled with fetid, dirty-colored shreds of pulmonary tissue in a state of putrid decomposition, will also establish the diagnosis.

The former view that acute pneumonia may terminate in chronic pulmonary consumption must be discarded. It must, moreover, be concluded that this view depended upon erroneous diagnoses, and that the affection regarded as a pneumonia was from the beginning of bacillary nature. Microscopical examination of the sputum makes such errors at the present time impossible.

III. The Sputum in Pulmonary Gangrene.

Gangrene of the lungs is a necrosis of the pulmonary tissue. It is brought about by the action of schizomycetes, which enter the lungs partly through the air passages and partly by way of the blood-channels, and there induce local infection; the expression of this infection is necrosis. Rosenbach [30] was successful in cultivating a chain coccus, named by him streptococcus pyogenes, from this source, and by inoculation with it produced progressive gangrene. A recent work from the Pathological Anatomical Institute at Turin by Bonome [42] attempts to prove the etiological significance of the streptococcus pyogenes aureus et citreus in the causation of pulmonary gangrene. It terminates in the conclusion "that in the investigated cases of pulmonary gangrene found in literature the infectious agent was the two microphytes mentioned."

Traube,[43] as early as 1864, designated the partly rod-shaped,

partly round vibrios found in the sputa of pulmonary gangrene as the excitants of the putrefactive process. Leyden and Jaffé[44] found such great masses of leptothrix that they could not regard them as simple mechanical admixtures from the mucus of the mouth, but as co-workers in the putrid decomposition. The same is true of the various monads found in the sputum (peach-blossom colored, putrefactive organisms).

In order to make a diagnosis of gangrene of the lung from

FIG. 15.—Sputum of pulmonary gangrene. (After Leyden.)

the sputum, one must be thoroughly acquainted with its characteristic features.

If communication has been established between the gangrenous focus and a large bronchus, and expectoration rendered possible, a large quantity (as much as two cubic centimetres) of foul-smelling, dirty-gray, greenish, or, in case it contains blood, greenish-yellow sputum is voided. After a time, the homogeneously expectorated sputum is prone to separate into three distinct layers in the vessel, the upper layer is frothy,

dirty yellowish-green, opaque, and contains single flakes of pus and mucus; the middle layer is strongly translucent, albuminous, almost serous, and permeated by stray flocculi, floating about in the fluid; the lowest layer forms a yellow, purulent, or brownish sanious sediment, composed of broken-up pus-cells and products of decomposition; the latter contain cork-like, very offensive crumbly masses, which present a villous or ragged appearance in water (Dietrich plugs). Microscopically they consist of a very finely granular detritus, mingled with larger fat-globules, within which needles of fatty acids, first observed by Virchow and described in another place, may be detected. In addition, the detritus contains smaller masses or heaps, which upon closer scrutiny or staining appear to be almost entirely composed of leptothrix or saprophytic elements.

Traube has called attention to a factor of great importance in the differential diagnosis between gangrene of the lung and other affections accompanied by destruction of lung tissue, namely, the absence of elastic fibres in the sputum. He surmises that they are rapidly destroyed by an unknown putrefactive ferment. As the sputum, by virtue of the development in it of fluid fatty acids, after some time shows acid reaction, it is probably that the acids dissolve the elastic fibres. Filehne and Stolnikow succeeded, by extraction with glycerin, in isolating from gangrenous sputum a ferment resembling trypsin, which is said to rapidly destroy elastic tissue.

Chemical examination of the sputum gives evidence of the presence of a great number of most diverse products, which form by the decomposition of albuminoid substances and fats. Future research must determine to what extent the poisonous basic bodies, caused by bacterial influences, the so-called ptomaïnes, are capable of detection, and what significance is to be attached to their presence in the sputum; it will be the task of bacteriological investigations to isolate by culture the various forms from the sputum, to test their specific nature, and give more complete explanations of other questions as yet shrouded in obscurity.

CHAPTER VII.

THE DISINFECTION OF THE SPUTUM.

SINCE the living organism offers an unsuitable channel through which to direct destructive attacks against bacteria, and since it is not possible for us to annihilate, before their discharge, the micro-organism known to be the indisputable producers of infection by any internal agents—in other words, to rid the body of poison—it should be our mission and unceasing endeavor to combat them externally and to remove them, or so to influence them as to frustrate their deleterious effects—in fact, to antagonize infectious disease by prophylactic measures. The investigations of R. Koch and his pupils have had the most momentous influence in awakening a recognition of this duty, and have led to the development and perfection of methods bearing upon the practical fulfilment of this object. From them we learn that the so-called disinfecting measures hitherto employed were ineffectual, and prevented neither the development of the bacteria nor their more dangerous spores.

Of the excrementary substance which may be the vehicles of pathogenic material, the sputum, next to the fæces, possesses the utmost significance. The sputum is the carrier of infection of one of the most destructive maladies—pulmonary consumption; the bacilli of Koch and their spores are thereby brought into the external world in innumerable quantities, and conveyed to others again under particular circumstances.

We need not enter upon the much-discussed and still undecided question as to how the bacilli or the spores capable of development enter the body. The facts that both are contained in the expctoration; that they retain their infectious quality after being kept for months in a dry state; that nearly one-seventh of all living people are phthisical, and discharge their poison-laden sputa externally, where abundant opportu-

nity exists for their drying up and mingling with the ever mobile dust, speak clearly enough for their importance and diffusion.

With this so-called ubiquity of the sources of infection, principally rendered possible by the sputum, and the duty it imposes upon us of adopting general protective measures, such as isolation of the sick, ventilation, lessening the disposition (conferring immunity), etc., we must, first of all, attempt to grapple with the enemy in its lair, the sputum, and to destroy it.

"Principiis obsta, sero medicina paratur."

The method of disinfection, as practised by Schill and Fisher[4] in the Imperial Health Office under the direction of Koch, is briefly as follows:

For larger objects, which are exposed to becoming soiled with sputa, as bedsteads, bed and body linen, handkerchiefs, etc., of consumptives, dry heat alone will not suffice; the practice, therefore, which generally prevails of conveying the objects into hot baking ovens for cleansing is insufficient. For this purpose the action of steam at the temperature of 100°C., for larger objects for an hour, for linen and clothing for half an hour, is necessary, as available in the diverse practical and effective disinfecting apparatus now offered the profession. [The Imperial State government has had a large Schimmel disinfecting apparatus erected at Bad Rehburg, which all landlords are compelled to use, as regulated by special rules.] Linens, feather-beds, mattresses, clothing, prints, etc., remain fully intact under this treatment—a fact generally disputed by the laity. The only exceptions are leather (shoes), rubber and cautchouc wares, furs, and book-binding. Fresh bacillary sputum is disinfected in about fifteen minutes, dried sputum in about thirty minutes to an hour.

Of chemical bactericides, carbolic acid has alone proved efficient for disinfecting the sputa. The addition of a 5% solution in equal proportion to the quantity of sputum suffices for the certain destruction of the bacilli and spores after twenty-four hours' contact; for the disinfection of a quart of sputum, therefore, 50 gm. of carbolic acid are necessary.

The employment of sublimate solutions (1:1,000), which in general possesses the strongest bactericide properties, is not to

be entertained, as the sublimate enters into combinations in fluids containing albumin, and does not remain in solution in sufficient quantity.

We, therefore, possesses in carbolic acid a sovereign means, which commends itself, besides its certain action, by its cheap price, and renders its introduction in daily practice vastly more feasible.

It is hoped that all those who in the midst of practical life retain a warm interest in hygienic efforts—the country doctors, the teachers, the heads of learned institutions—will instigate all those coming within the scope of their humane professional duties, to preach and explain the proper conception of the hygienic importance of the sputum and its disinfection. In this manner, according to the wish of Virchow, "a part of the mission of humanity to overthrow tuberculosis as scorbutus has been overthrown, would be fulfilled."

CHAPTER VIII.

PRACTICAL METHODS FOR THE EXAMINATION OF SPUTUM FOR TUBERCLE BACILLI.

SINCE I have been engaged in the examination of the sputa of patients with pulmonary diseases for the bacilli of Koch, a large number of inquiries have been made of me by colleagues, who from time to time had sent me sputa or preparations for investigation; I therefore gained the conviction that only a very detailed description of the technique of the examination would enable practitioners to acquire that satisfactory dexterity which, in the differential diagnosis between tuberculous (bacillary) pulmonary diseases and other forms, will exclude all doubts. Every one who is much engaged in this form of sputum investigation will be desirous of perfecting whatever skill in this regard he may possess; of course, however, on the basis and plan of the original Koch method and its modification by Ehrlich.

While the author is conscious that he is not in a position to offer any essentially new points, he cherishes the hope that the full description of manipulations so far tested and approved by individual experience may serve as a useful guide and welcome aid to many seekers after light.

The direct microscopical examination for small fungi (schizomycetes) was beset with many difficulties while we lacked a knowledge of staining methods, so that it was only possible to demonstrate their existence from the form of the single micro-organism and its characteristic aggregation in properly prepared specimens. It was not until the methods of staining were successfully cultivated in the examination of micro-organisms, particularly by Weigert, R. Koch, Ehrlich, and others, that a proper differentiation was rendered possible and we are now enabled to make nearly all known micro-parasites accessible to investigation by their reactions to staining.

Compared with other schizomycetes, the bacillus of tuberculosis requires special treatment, as it possesses specific peculiarities with reference to the staining-method employed.

R. Koch, to whom is due the great honor of having discovered the method for detecting the bacilli, by numerous experiments came to the conclusion, first, that only definite basic anilin colors were suitable for staining fungi, and, secondly, that the reaction of the staining fluid was decisive for the tubercle bacilli, or, in other words, that the tubercle bacilli would take on the aniline stain only in the presence of alkalies. He[45] subjected cover-glasses containing bacillary sputum to the influence of a coloring fluid of the following composition:

"Two hundred cubic centimetres of distilled water were well shaken in 1 cc. of concentrated alcoholic solution of methyl blue, with the addition, during continued agitation, of 0.2 cc. of a 10% potash lye. This mixture, even after standing for days, must not yield a precipitate. The cover-glasses remain in it from twenty-four to forty-eight hours. They are then rinsed in a concentrated watery solution of vesuvin, which must be filtered before each use, and after one or two minutes are washed in distilled water until the blue color has disappeared." In such preparations everything is now stained brown, with the exception of the rod-like bodies, the tubercle bacilli, which retain their blue color with remarkable tenacity. This, according to Koch, is due to the fact that certain aniline colors possess the peculiar property of mutually supplanting one another—in this case, methyl blue by vesuvin.

Ehrlich, who sought to simplify the procedure of Koch, found a suitable substitute for the potash-lye in aniline oil (a mixture of two similar bases of aniline, toluidin and pseudo-toluidin), a brown or yellowish oily fluid, the saturated watery solutions of which will dissolve more coloring matter than dilute potassium solutions. Based on his investigations, he presents the hypothesis that the bacilli are surrounded by envelopes pervious under alkaline reaction, but impervious to acid media. Proof for this statement is that stained preparations after rinsing in nitric acid become entirely decolorized; the bacilli, on the other hand, access to which by the acid is prevented by the surrounding envelope, retain their color. The modified method of Ehrlich was as follows:[46]

"He pressed a particle taken from the sputum with needles

between two cover-glasses of 0.10 to 0.12 thickness in order to obtain a uniform thin layer. After he had allowed the preparation to become air-dry, he drew it by means of forceps three times through the flame of a Bunsen gas-burner. For staining, he makes a saturated aniline water by agitating water with an excess of aniline oil, and filters it. To the transparent fluid so obtained, he adds, drop by drop, a saturated alcoholic solution of methyl-violet or fuchsin, until the fluid becomes distinctly opalescent, which indicates saturation. He allows the cover-glasses to float upon this fluid for a quarter to half an hour. Instead of vesuvin, he employs an acid mixture (1 volume of officinal nitric acid and 2 volumes of water), by the action of which after a few seconds the preparation becomes blanched or white, and only the bacilli retain their blue color." This procedure is superior to that of Koch in that it is more expeditious, and as Ehrlich believes, also because of the greater number of bacilli presenting themselves. The method of Koch was therefore discarded, and is now only occasionally employed for comparative purposes.

Several months after Ehrlich's publication, Ziehl[47] denied the view of the former that the envelope of the bacilli was only permeable to coloring matters by the aid of alkalies, and substituted carbolic acid for the aniline oil, which, according to him, possessed, like aniline oil, substances belonging to the aromatic order, of definite reaction.

Although, as I have convinced myself by numerous experiments, the method with carbolic acid is successful, results are more quickly obtained with aniline oil, a fact conceded also by Ziehl.

Balmer and Fräntzel[48] recommend further immaterial modifications, namely to allow the cover-glasses to float in the methyl-violet aniline-water solution for twenty-four hours, then to immerse them for one-half to one minute or until completely decolorized in a nitric acid solution (1: 3), and after washing in distilled water, to place them for half to one minute in a concentrated watery solution of Bismarck brown for staining the field.

Every one who has to make such investigations for diagnostic purposes daily, knows how important it is for the busy practitioner to possess a method which combines at once the greatest celerity with the utmost reliability. From this

standpoint Rindfleisch[49] recommended his pupils to proceed as follows:

"As much aniline oil is poured into a flask as will fill up the opening to the fundus, it is then filled one-third full with water, the aniline oil and water thoroughly agitated, and at once filtered through a filter carried in the free hand into a second flask; eight drops of a concentrated alcoholic solution of fuchsin are added to the filtrate; then the operator places before himself: 1, a watch-glass half filled with spirit to which two drops of dilute nitric acid are added, placed upon a piece of white paper; 2, a watch-glass half filled with the above-mentioned solution of fuchsin; 3, a burning spirit lamp. The cover-glass upon which the suspicious sputum has been dried is now grasped at its edge by a pair of forceps, and drawn three times, about as rapidly as one cuts bread, through the flame of the spirit lamp, the sputum-side up; the albumin is by this made homogeneous for further manipulation. The cover-glass is now laid upon the staining fluid free side up and allowed to float; the watch-glass is then grasped with the forceps and held over the flame until the fluid begins to give off vapor. After this the cover-glass is removed from the fluid with the forceps, washed off in a handy cup of water or under a stream, and immersed in the acidified spirit. Here very soon violet clouds liberate themselves from the preparation, and after ten or fifteen minutes the glass, with the exception of a few traces, appears decolorized. It is now removed with the forceps, at once again rinsed in water, dried, and mounted in Canada balsam. In the examination the highest power is used and all glare of light avoided."

In July, 1883, Orth, of Göttingen, published "Notizen zur Färbetechnik,"[50] in which he recommended muriated alcohol (1:100, 70% alcohol) for washing out stained preparations, "because it acts better than nitric acid, attacks the preparations to a lesser degree, and is more agreeable than the latter in manipulations."

The most recent modification of Ehrlich's procedure is described by Petri-Gorbersdorf.[51] He considers the aniline oil superfluous, and employs instead blue or brown aniline colors with fuchsin or malachite green, and does not decolorize with mineral acids, but with glacial acetic acid. The author, after several hundred sputum investigations since Koch's publica-

tion, according to the different methods described, has come to the conclusion that, from a practical standpoint, a combination of Ehrlich's method, with some modifications recommended by Rindfleisch and Orth, is the most expeditious and certain, and that the preparations produced thereby are the best, and so permanent that after eight months' keeping they have lost nothing in distinctness.

In general, the author believes himself justified in declaring that of all aniline colors gentian violet is the most serviceable for staining tubercle bacilli. From this substance an alcoholic solution should be prepared so concentrated that it will show an excess of coloring matter. For decolorizing, the author uses a mixture of 100 ccm. 90% alcohol, 20 ccm. water, and 20 drops of concentrated muriatic acid, with subsequent rinsing of the preparation in 90% alcohol; for after-staining, by which the bacilli stand out in characteristic manner, he employs a concentrated watery solution of vesuvin. The bacilli present themselves now as dark violet rods, occasionally slightly bent or in the process of forming spores, upon a brown background. As a matter of course, the most careful scrutiny of every individual particle of the preparation is requisite for detecting the bacilli. Specimens thus prepared, as already mentioned, may be preserved for considerable periods. This keeping property may be explicable by the employment of muriated alcohol instead of nitric acid, and thus substantiate the statement of Brun,[52] according to which the nitric acid remaining behind in the preparation is the cause of the bacilli finally losing their color. Whether Canada balsam, damar-resin, or glycerin be used as the preservative medium, depends upon individual predilection; when, however, Orth declares that bacilli kept in Canada balsam lost color because of the chloroform contained therein, I must dispute this view upon the ground that my own preparations, which have been mounted in Canada balsam for eight months, have not lost a vestige of their original color. One can, too, as I have tried it, allow dried preparations to lie for months without the addition of preserving fluids and still be able to detect the bacilli plainly.

Regarding the microscopical examination, I cannot concede that an investigation under a power of 300 to 400 diameters is sufficient. Although it may be possible for the practised eye

to work with this power, in general, and particularly where a small number of bacilli are present, apart from the great saving in time, an immersion lens and an Abbé condenser must be regarded as indispensable for securing accurate diagnosis.

Attention should also be called to the fact that the quality of the dyes is not always uniform, and possibly this will partly explain the non-success of many colleagues. Another cause of failure appears to me to be due to the fact that the layer of sputum is too thick and too irregularly spread upon the cover-glass, it may also not be superfluous to advise the investigator, in order to gain certainty, to prepare several specimens of one sputum. The question as to how long the bacilli may be demonstrable in bacillary sputum may be answered by the statement, also made by Koch,[53] that dried sputum shows bacilli eight weeks after being voided, and that I have identified numerous bacilli and spores in fluid phthisical sputum now five weeks old.

Based upon the preceding explanation, I would formulate my procedure for the certain, rapid, and thorough staining and detection of tubercle bacilli, in the following order:

1. Spread out a quantity of sputum upon a dark object, preferably a blackened plate, and pick out by means of platinum needles (always previously sterilized) an opaque white or grayish-white particle.

2. Spread out, or better still, rub this particle until dry between cover-glasses of 13 to 18 mm. thickness, so that a uniform thin layer becomes fixed; the layer must be about as thin as that required for examining the blood (Friedländer). The cover-glasses are not to be lifted, but drawn apart.

3. Slowly draw the cover-glass four times through a spirit flame of moderate intensity, "about as rapidly as one cuts bread."

4. Shake good aniline oil for several minutes with distilled water about in the proportion of 5 : 100, then filter through moist paper into a porcelain capsule (aniline water).

5. Add (to solution No. 4) a filtered supersaturated solution of gentian violet in 90% alcohol, enough to cause decided opalescence, and stir with a glass rod. About 15 drops of staining solution are requisite for 10 ccm. of aniline water.

6. Either allow the prepared cover-glasses to float 24 hours

(longer does no harm) upon this staining solution, or the capsule with the cover-glasses (the author generally makes six preparations from one sputum at the same time) is placed upon a tripod with a wire net having a spirit lamp underneath, heated with a moderate flame until crackling bubbles form on the fluid, or, more accurately, to 80° C., and allowed to stand forseveral minutes.

7. Removal and preparation in the following order: After the cover-glass has been removed with the forceps, the superfluous staining solution is removed with a piece of blotting paper; then immersed in a capsule filled with muriated alcohol (100 ccm. alcohol, 20 ccm. distilled water and 20 drops concentrated muriatic acid) for about a half to one minute; then stirred in a capsule containing only 90% alcohol until the last remnant of the blue color has disappeared (one to two minutes), and finally thoroughly washed with distilled water.

8. The cover-glass, specimen side up, is now laid upon blotting paper and allowed to dry. If the latter is to be accelerated, it may be most effectually accomplished by blowing through a glass tube. Then drop upon it with drop counter or a Lambert drop-glass, four to five drops of a concentrated watery solution of vesuvin. (In preserving this fluid, the addition of a small piece of camphor will prevent moulding.) After two minutes, thoroughly wash in distilled water, dry the preparation, remove it to a slide of white plane glass about 1 to 2 mm. thick and examine in a drop of distilled water, or, if it is to be preserved, mount it at once or later on in Canada balsam or damar varnish.

BIBLIOGRAPHY.

1. A. Biermer, Die Lehre vom Auswurf, Ein Beitrag zur mediz. Klinik von. Würzburg, 1855, Stahel's Verlag.—2. R. Koch, Die Aetiologie der Tuberkulose, Berl. Kl. Wochenschr., 1882, Nr. 15.—3. C. A. Ewald, Vortrag über Aetiologie, Diagnose und Therapie der Tuberkulose auf dem internationalen Congress zu Kopenhagen, 1884, Berlin. kl. Wochenschr., No. 44.—4. Mitteilungen aus dem Kaiserl. Gesundheitsamte, Band i. und ii., 1881 u. 1884.—5. Buhl, Lungenentzündung, Tuberkulose u. Schwindsucht, 12 Briefe, München, 1872.—6. Guttmann und Smidt, Zeitschrift für Klin. Medizin, Band 3.—7. Bizzozero, Centralblatt für klin. Medizin, 1881, page 529.—8. Dettweiler-Meissen, Der Tuberkelbacillus und die chronische Lungenschwindsucht, Berlin kl. Wochenschr., 1883, No. 7.—9. Remak, Diagnostische und pathogenetische Untersuchungen, Berlin, 1845.—10. Riegel, Krank-

heiten er Trachea, Ziemssen's Handbuch, Band iv.—11. Kretschy, Wiener mediz. Wochenschr., 1873, 14, 15, 16.—12. Virchow's Archiv, i. —13. Leyden, Ueber Tyrosin im Auswurf. Virchow's Archiv, 1878, Band 74.—14. Virchow's Archiv, Band 32, p. 525.—15. Leyden, Zur Kenntnis des Bronchialasthma, Virchow's Archiv, Band 54, p. 324. —16. Curschmann, Deutsches Archiv für kl. Medizin, Band 32, p. 1. —17. Eichhorst, Handbuch der speciell. Pathol. und Therapie, Wien und Leipzig, 1887. p. 376.—18. Frey, Beitrag zur Lehre von Taenia Echinococcus, Dissertat., Berlin, 1882.—19. See Chapter VIII., Practical Method for the Examination of Sputum.—20. Breslauer ärtzl. Zeitschrift, 1885, No. 15.—21. Revue de la Suisse Romande, 1882, viii.—22. Berlin. klin. Wochenschrift, 1883, p. 421.—23. Fortschritte der Medizin, 1883, 1884, 1885.—24. Deutsche mediz. Wochenschrift, 1885, No. 9.—25. Archiv für microscopisch. Anatomie, xiii., 263.—26. Verhandlungen des Congresses für innere Medizin, Wiesbaden, 1884.—27. Lancet, 1884, p. 513.—28. Deutsche mediz. Wochenschrift, 1885, No. 9.—29. Flügge, Die Microorganismen, 1886, p. 317.—30. Rosenbach, Microorganismen bei den Wundinfectionskrankheiten, Wiesbaden, 1884. — 31. Virchow's Archiv, Band 70, 1877, and Band 73, 1878.—32. Bokay, Pester mediz. chirurg. Presse, S. A., 1879.—33. Hoppe-Seyler, Handbuch der physiol. und pathol. Chem. Analyse, 1883, p. 113.—34. Bamberger, Beitrag zur Lehre vom Auswurf, Würzburg. mediz. Zeitschrift, 1861, Band ii.—35. Kussmaul, Aschenbestandteile der Lungen, Deutsch. Archiv für klin. Medizin, Band ii.—36. Archiv für experiment. Pathologie, Bd. v., p. 350. — 37. Virchow's Archiv, Bd. 61, p. 130. — 38. Laennec, Traité de l'auscultation, 1826, i.—39. Brehmer, Die Therapie der chr. Lungenschwindsucht, Wiesbaden, 1887.—40. Centralblatt für kl. Medizin, 1885, 42. — 41. Archiv für Hygiene, Bd. 2, Hft. 1. — 42. Bonome, Beitrag zum Studium des Lungenbrandes, D. M. Wochenschrift, 1886, No. 52. —43. Berlin. kl. Wochenschrift, 1864, No. 17.—44. Deutsches Archiv für kl. Medizin, Bd. 2, p. 488.—45. Berlin. Klin. Wochensch., 1882, p. 221.—46. Deutsche Med. Wochensch., 1882, p. 269.—47. Deutsche Med. Wochensch., 1882, p. 451.—48. Berlin. Klin. Wochensch., 1882, p. 679.— 49. Deutsche Med. Wochensch., 1883, p. 16.—50. Berlin. klin. Wochensch., p. 421.—51. Berlin. Klin. Wochensch., 1873, p. 739.—52. Revue de la Suisse Romande, 1882, No. viii.—53. Verhandlung des Congresses für innere Medicin, 1882 p. 66.

HYPNOTISM.

ITS SIGNIFICANCE AND MANAGEMENT BRIEFLY PRESENTED.

BY

DR. AUGUST FOREL,

Prof. of Psychiatry and Director of the Cantonal Lunatic Asylum at Zurich, Switzerland.

HYPNOTISM.

CHAPTER I.

GENERAL REMARKS.

FACTS, theories, notions, and terminology are unfortunately thrown together in a state of terrible confusion in the field of hypnotism, as in everything that pertains to psychology.

Data.—The chief fact in hypnotism is the changed condition of the mind (or condition of the brain when viewed from the physiological, that is, the objective side) of an individual. To distinguish it from ordinary sleep, with which this condition presents great similarity, it may be called hypnosis. The second series of facts consists in the mode of production (or removal) of this condition. Here false interpretations have given rise to the most erroneous conceptions. Hypnosis may be produced apparently in three ways: *a*, by the physical action of one individual upon another by means of ideas which he communicates to him. This mode of hypnotization has been called suggestion (Nancy School); *b*, by the direct action of living or inanimate objects or of a mysterious agent, upon the nervous system, in which a great part has been ascribed to exhaustion by a long concentration of a sense upon one point; also by the specific action of magnets on the human mind; of drugs inclosed in bottles, and the like; *c*, by the reaction of the mind upon itself (auto-hypnotism). In complete unison with Bernheim I think it may be maintained that in reality only one mode of production of hypnosis has been established scientifically, namely, its production by ideas (whether by the suggestion of another or by auto-suggestion). The possibility of suggestion or auto-suggestion has not been excluded with scientific certainty in any of the other assumed or apparent modes of production of hypnosis.

The third series of facts is that of the actions of the hypnotized. It is settled that in this condition by means of suggestion, the most extensive reactions upon almost all the functions of the nervous system (with the exception of a few spinal reflexes and ganglionic functions) are possible—including such bodily functions as digestion, defecation, menstruation, the pulse, redness of the skin, etc., whose dependence upon the cerebrum has been forgotten commonly or under estimated.

The more or less complete dependence of the mental activity of the hypnotized upon the suggestions of the hypnotizer is also undoubted. Finally, and of the greatest importance, is the certainly established fact that the action exercised in hypnosis may extend post-hypnotic to the normal condition of the mind in its entire domain, and that this may occur for a long period, including the influence of the hypnotizer upon the hypnotized.

Doubtful, on the other hand, or at least, not explained or ascertained with sufficient scientific certainty, are the so-called super-sensual facts, such as so-called clairvoyance, the action of drugs in closed bottles, the so-called direct transmission of thought, and the like. In the extremely rare somnambulists in whom these experiments are said to succeed, a strictly scientific control which excludes all possibility of unconscious suggestion usually appears to have been absent, and when present the complete fiasco of the experiment seems to have been the ordinary result. Nevertheless, this question must be regarded as an open one and as worthy of careful investigation, because a series of statements of trustworthy individuals of good judgment answer it in the affirmative.

Theories and Conceptions.—The conceptions entertained concerning hypnotism depend upon the theoretical views which prevailed concerning it. If we throw out, as far as possible, the ballast of undigested and superstitious nonsense which has been furnished in regard to the question under discussion, there remain on the whole three distinct theories or explanations of the facts which have been briefly mentioned above.

1. An external invisible agent (a fluid, as it was formerly called, and as the laity call it up to the present time; a yet unknown natural force, as it would be called in modern language) enters the body, especially the nervous system, influences the organism and communicates to it something foreign

—perhaps even the knowledge of inanimate nature or of other living beings. Or the thoughts, the mental processes of another individual, without the medium of communication by speech, writing, or signs on the part of the first individual and of the sense organs of the second, pass, by means of such an agent, to the cognizance of the mind of another individual. This is Messmer's theory. Messmer called the supposed agent magnetism, or animal magnetism, when it appeared to be derived from the human organism itself (from that of the magnetizer). This theory, which still has enthusiastic, even fanatic adherents in certain circles, is based upon the phenomena which, under the heading "*b*" have been called "doubtful super-sensual facts." It is clear that if this were true, our scientific knowledge would have to be restricted considerably, because the previous ignorance of this unknown something, this unknown force, by science, necessarily would entail errors in our previous knowledge, as would a forgotten important component. But inasmuch as science, by its colossal practical results, daily furnishes more and more the proof of its inner truth, we have every reason to mistrust Messmer's theory and to demand of it undoubted, incontrovertible proofs. Let us see what is really the case.

Messmer and his school were so thoroughly controverted by Braid and Liébault, in regard to the undoubted facts first mentioned (see below) that it would be useless to waste another word concerning them. The fluid theory fortifies itself at the present time behind the assumed facts which are championed by the spiritualists, and which, according to the circles in which they are produced, are permeated to such an extent by blind fanaticism, by mental disturbance (hallucinations), by misunderstood suggestion, by fraud and by superstition, that a scientific test is extremely difficult. The spirits and fourth dimension of the spiritualists are conceptions which would correspond to this unknown agent. A series of apparently supernatural phenomena, as we have already remarked, is constantly being adduced again and again by honest, trustworthy individuals, and which would favor Messmer's theory or allied theories. I mention the so-called transmission of thought, also called briefly mental suggestion, clairvoyance, the seeing or divination of processes occurring in a remote locality, the so-called presentiments and prophecies, etc. A

remarkable book in this respect is "Phantasms of the Living" by Gurney, Myers, and Podmore, in 2 vols., 8vo. Truebner, London, 1887. No less than 600 observations of visions, dreams, presentiments and the like, which were fulfilled are here collected. Concerning the authenticity of the sources of these statements, careful examination was made and only clear statements of trustworty individuals were accepted. A review of this work in found in the "Revue des deux Mondes," May 1st, 1888. Every individual, in the circle of his acquaintances, will stumble across several such observations in thoroughly reliable people. Vide also Liébault, "Le Sommeil Provoqué," 1889, p. 295. Interesting, furthermore, are the experiments of Charles Richet ("Revue Philosophique," 1884), who attempts to prove the influence of the thought of one individual upon the thought of another in a definite direction, without any external phenomena which can be perceived by the senses. As it seems to us, however, the proofs are very incomplete, and the calculation of probability employed is not very convincing. The results of the Society for Psychical Research in England appear to be more positive. But in all these experiments it is extremely difficult, apart from accident and fraud, to exclude with certainty self-deception of the hypnotized or the subject (perhaps also the hypnotizer) and particularly, to exclude all unconscious suggestion or auto-suggestion, so that these results must be accepted with the greatest caution.

2. Diametrically opposed to the first-mentioned theory is the notion of suggestion, formulated originally by Braid, but first grasped in its entire significance and its practical results by Liébault in Nancy ("Du Sommeil et des États Analogues," 1866). This may be formulated after the following fashion:

Production of all the phenomena of hypnosis by the induction of the corresponding ideas, especially of conceptions of the imagination. Here it is to be remarked that the purpose is effected most readily and certainly when the hypnotizer by means of speech declares with positiveness that the condition to be produced is present at the very moment when he expresses it, or will develop at once (verbal suggestion). If an individual convinces himself, we speak, with Bernheim, of auto-suggestion. But even Braid did not recognize sufficiently the full bearings of suggestion, and in its stead attributed excessive importance to the continued stimulation of the senses

(fixation, etc.) By means of suggestion sleep is usually first produced and inasmuch as the sleep condition of the brain considerably increases its suggestibility (*i.e.*, its susceptibility to the influence of suggestion) we obtain the desired power at once. But suggestions result not alone from speech but also from everything that can produce conceptions, especially from everything that produces vigorous images of the fancy. Liébault (p. 347, op. cit.) correctly writes:

"The disposition to fall into these conditions is proportionate to each one's faculty of mental representation. We may be sure that the man who, in concentrating his attention upon an imagined idea (for example, that of a tactile perception), does not fail to perceive it as if it were actual, is capable of sleeping deeply (*i.e.*, of being deeply hypnotized)."

Still more, a suggestion may occur unconsciously, or the corresponding conception may appear so feebly or for such a short time in consciousness, that it at once disappears from it forever, inasmuch as conscious memory can never recall it, although this suggestion acts powerfully. On account of the complete amnesia in such cases, as a general thing, it cannot be proven that the conception in question was ever a conscious one. Yet closer examination shows that it was undoubtedly present. Herein lies the crucial point for the comprehension of an immense number of self-deceptions and so-called Messmer's effects. A country girl who was hypnotized for the first time and who had no knowledge of physics and prisms, has a prism placed before her eyes during hypnosis after she had been allowed to look, by suggestion, at a candle in the air which was not present. She is then asked what she sees, and answers "Two candles." This, as Bernheim has demonstrated, is an (unconscious) suggestion. Through the prism the girl saw the real surrounding objects in the room double, and thus unconsciously suggested that the suggested candle was also double. If the experiment is made in a perfectly dark room with an individual who has never been hypnotized and who is not conversant theoretically with the facts in question, the suggested image is never doubled by the prism (Bernheim). It is hardly to be assumed that the girl during hypnosis was conscious that the candles were seen double, because all at once the other objects were seen double. This duplication occurred instinctively, automatically, below the level of conscious-

ness. The other objects were not fixed by her (but only the fictitious candle). Nevertheless its duplication was perceived and utilized (very probably unconsciously). But the mechanism of suggestion, that is, the manner in which the heard and understood words of the hypnotizer (or the perception itself) produces the actual result, always remains unconscious.

Liébault's suggestion theory of hypnosis, by its practical results, especially in medical therapeutics, but also in education and in many other fields, has given such striking proofs of its truth, that its victory can now be regarded as perfectly assured. While other theories, with their corresponding methods, were only able to produce a part of the symptoms of hypnosis in a few hysterical or nervous individuals—exceptionally in a few healthy ones, with more or less trouble—and, at the same time, were constantly confronted with riddles and contradictions, and were compelled to resort to the most astonishing and obscure attempts at explanation, suggestion succeeds easily in almost every healthy individual, and explains everything from a single standpoint, with the exception of the facts mentioned above as doubtful. The number of mentally healthy individuals hypnotized in Nancy alone by Liébault and Bernheim amounts to several thousands. In 1887 Dr. Wetterstrand, in Stockholm, subjected 718 persons to suggestion, of whom only 19 remained uninfluenced. In three months Dr. Van Renterghem, in Amsterdam, successfully hypnotized by suggestion 162 individuals out of 178, and effected 91 cures of various diseases. Fontan and Ségard, in Marseilles, had very few failures in about 100 individuals. Despite my extreme pre-occupation by other duties, I have been able, in a little more than a year, to subject 205 individuals to suggestion, among them a number of insane. Of these 171 were influenced; 34 were not. Of the 105 persons last hypnotized, however, only 11 remained uninfluenced. Three individuals whom formerly I was unable to influence, were recently hypnotized with ease. Dr. Ringier, a physician in Canton Waadt, who studied the suggestion method with me last summer, attempted suggestion in a short time in more than 60 individuals, and of these only three or four were unsuccessful. Among all these hypnotized persons are a large number of perfect somnambulists with post-hypnotic phenomena, etc. How different are these numbers from the few hysterical pa-

tients of the Salpêtrière in Paris, not much more than a dozen, who for years have been demonstrated to every one, who serve as the basis for Charcot's theory, and have evidently succumbed to the most complete automatism of suggestion. If we grasp what has been said, it appears evident that the hitherto confused notion of hypnotism must disappear in the notion of suggestion. Herein lies the key of the greater part, perhaps of all, of the phenomena under discussion.

3. As the so-called somatic theories of hypnosis we may group together all those which, as it were, form the link between the two mentioned. No fluid or spirits are conjured up, it is true, but the attempt is made to attribute a part, if not all of the symptoms of hypnosis to known elementary forces without the medium of mental activity. A principal part is attributed to the action of peripheral stimuli (from without) upon the nerve terminations, so that, in part, the necessity of an external agent again becomes prominent. It is especially the School of Charcot or of the Salpêtrière which believes in the direct hypnogenous action of metals and magnets upon the nervous system (without the medium of conceptions); which believes in *transfert* (jumping of a paralysis, catalepsy, hemi-anæsthesia, etc., from one side of the body to another by the action of magnets); in the direct stimulation of the motor cortical centres of the brain, by stroking the scalp, etc. This school believes that by different peripheral mechanical irritations (1, fixation of the gaze; 2, raising off the lids; 3, stroking of the forehead) typically distinct stages or forms of hypnosis, namely, lethargy, catalepsy, and somnambulism, can be produced with specific reactions of the muscles and sensibility. It is important to note that Charcot's school believes that in so-called lethargy, the hypnotized are entirely unconscious and cannot be influenced by the suggestions which are conveyed to them by conceptions through the medium of the organs of sense. This school also believes that hysterical individuals are almost the only ones accessible to hypnosis and regard hypnosis as a neurosis. Bernheim, in Nancy, has demonstrated most conclusively to what confusion of thought this theory gives rise. All the facts which for years have been demonstrated at the Salpêtrière upon the few prepared hysterical individuals, are easily explained by old, in part unconscious, suggestions, inasmuch as, for example, the assumed

lethargic individual undoubtedly hears and mentally utilizes everything that is done or said in his presence. Braid's fixation of a shining object, to which so much importance is attached in Paris and in Germany, in itself produces no hypnosis. If any one is hypnotized by this unsuitable method it is done by means of the notion that this procedure will cause sleep, not by the procedure itself, which, *per se*, usually produces merely a nervous excitement (sometimes hysterical attacks in hysterical individuals); at the most, in a few of these cases, a tired feeling, and thus the falling of the lids may act as an unconscious suggestion.

It was formerly the general custom to awaken hypnotized individuals by blowing upon the face. For a long time I have not done this, and instead, have often associated the blowing with the suggestion of the disappearance of headache, etc. Hence, I may blow upon the face of my hypnotized individuals as much as I please without awaking them. This is also an argument against the assumed action of such mechanical stimuli on the part of the Somatic school, which regards the blowing as a specific waking irritant.

Liébault himself ("Étude sur le Zoömagnétisme," Paris, chez Masson, 1883) published 45 cases, in which, in little children, he claims to have obtained wonderfully favorable results by placing both hands upon a diseased part. In 32 of these cases the children were under three years, whence Liébault himself believes that suggestion may be excluded. They include several cases of whooping cough, with really astonishing curative results. Yet unconscious suggestion does not appear to be completely excluded in view of the enormously suggestible nature of children. Nevertheless, these observations remain extremely remarkable and are worthy of thorough investigation.

Chazarain and Dècle ("Le Courants de la Polarité dans l'Aimant et dans le Corps Humain") believe they have discovered a polarity of the human body, and attribute hypnotic phenomena as well as the results of so-called metallo-therapy to them. Suggestion and auto-suggestion can be excluded still less than in Liébault's children, and least of all, if we read the author's superficial observations, almost all of which appear to me to be suggestions of hysterical individuals.

Finally, mention is to be made of the assumed action of

drugs at a distance, or by the application of hermetically sealed bottles to the neck, etc. (Luys and others). But these results, which were proclaimed by Luys with great éclat, made a miserable fiasco before a commission of investigation, when all unconscious suggestions were prevented. They show with what want of criticism the experiments had been conducted and how nothing had been done to exclude the possibility of suggestion, that explains everything.

At the wish of my friend, Prof. Seguin, of New York, I have recently performed with his aid Luys' experiments with the closed medicine bottles, upon four of my best somnambulists. Prof. Seguin had seen Luys' experiments. The result was absolutely negative, as I confidently expected. The only interesting feature was the following: A hypnotized woman who had an alcohol bottle on her neck, and had previously stated that she felt nothing, was asked whether she did not have a headache. This was answered in the affirmative. Then, if she was not dizzy, as if drunk, and this was also answered in the affirmative, and was followed by symptoms of drunkenness. Hence it appears that a single insinuating question may act suggestively. I need not add that I also produced all symptoms of the corresponding drugs (even vomiting) by suggestion with wrong or empty bottles (as a control experiment).

If we make a résumé of the third group of theories which are said to be somatic and rational, it is found that they are the most unfortunate of all and produce the most dire confusion, and that all the facts claimed for them can be explained by suggestion. Liébault's results in children at the most form a noteworthy exception. The chief error in these theories is that they depend for their results chiefly upon observations on hysterical people. But the latter are, in the first place, the most unreliable; the best, because the most unconscious simulants and actors, and at the same time those individuals who often have the finest special senses and at the same time possess a considerable plastic imagination which makes them very suggestible, particularly auto-suggestible (usually unconsciously or only semi-consciously). At the same time, it is also forgotten that the psychical processes are somatic.

We thus have only one theory, namely the suggestion theory of the Nancy school, which harmonizes with the scientifically established facts of hypnotism and explains them satisfactorily.

Everything else is still immature or doubtful, or depends upon the misunderstanding of certain phenomena which are yet obscure and await a strict and methodical investigation.

Hence we have to deal only with the notion of suggestion and of suggestive sleep, as coördinate with that of hypnotism.

Terminology.—The expressions animal magnetism and mesmerism must be left to the fluid theory. The term hypnotism (Braid) may be applied to the entirety of the phenomena associated with conscious and unconscious suggestion. Hypnosis best defines the changed condition of the mind of hypnotized individuals in suggestive sleep. The hypnotizer is that individual who produces the condition of hypnosis in another. The term suggestion we apply, according to the Nancy school, to the production of a dynamic change of the nervous system in an individual (or of such functions which depend upon the nervous system) by another individual by means of conveying the conviction, conscious or unconscious, that this change occurs, or has occurred, or will occur. Verbal suggestion is suggestion by spoken language. Suggestibility is the individual receptiveness for suggestions. There are very many individuals who are suggestible in the waking condition. In them the notion of hypnosis can hardly be limited, because their normal condition in waking passes into the condition of hypnosis by imperceptible gradations. Auto-suggestion (Bernheim) is the suggestion which an individual produces consciously or (usually) unconsciously in himself.

The notion of "suggestion" and especially "auto-suggestion" may easily be enlarged to too great an extent, and made synonymous with the notion of instinct, habit, reflex, automatism. In fact, the distinction is difficult. The notion of suggestion may be better defined by including the active suggesting hypnotizer (the association of one individual with another). But if the hypnotizer acts unconsciously (when the suggestion is made to another by my yawning, for example) or when the suggestion is made by objects, the notion passes into that of auto-suggestion. The latter runs the risk of being extended so as to give rise to confusion and misconstruction of former truths and investigations. I will content myself with this hint.

Suggestion and hypnosis as phenomena and potencies are as old as the history of man, probably even much older phylogenetically. Entirely new are only two additional factors: 1,

the recognition of these phenomena, of their conditions, cause, their range in the consciousness of mankind, especially scientific mankind, and no longer as doubtful mysticism, but as scientific truth; 2, the remarkable facility with which hypnosis can be produced by Liébault's method in almost every individual. These two factors, it may be remarked in passing, also give to hypnotism a new criminal significance.

CHAPTER II.

SUGGESTION.

Hypnotizability.—In the "Revue de l'Hypnotisme" (May 1st, 1888) Bernheim says: "Every hospital physician who, in his hospital service, is unable to hypnotize eighty per cent of his patients, should say that he has not yet sufficient experience on the subject and should refrain from a hasty judgment."

This statement I subscribe to completely, and with it agree thoroughly the above-mentioned statistical data. Insanity must alone be excepted.

Every individual is more or less suggestible, and thus capable of hypnotization. Many, it is true, plume themselves upon believing only what is clearly and consciously proven by reason, or, at least, is made very plausible. But they only prove by this that they are lacking in the most elementary self-criticism. Involuntarily and unconsciously we believe constantly in things which entirely or in part are not real. For instance, we believe in the reality of our sensory perceptions, which are only subjective conclusions. Every individual makes mistakes; trusts other individuals, statements which do not justify his confidence, etc., etc. These are proofs of our intuitive faith, without which thought would not be possible. If we waited until every motive of our thought and action were proven mathematically or only by sufficient induction before acceptance, we would never arrive at real thinking or action. But we can neither think nor act without possessing a certain feeling that our thought and action are correct, without believing more or less in them. Herein lies the key to suggestibility.

When we wish ardently for something that we do not possess, the opposed notion of the unattainability of our desire not infrequently develops so much more intensely. This psychological condition is especially evident when wishing for sub-

jective feelings. If we try to force them they flee from us. The one who attempts to sleep with consciousness and at all hazards becomes sleepless. The one who attempts in the same way to perform coitus is temporarily impotent. The one who wishes to be joyful, whether or no, becomes angry, etc. The greater the violence that consciousness attempts to exercise the greater is its defeat, while the same wished-for feelings develop spontaneously when, without concentration, we yield to the belief in them, especially with the aid of the corresponding conceptions of the imagination.

If an individual is extremely anxious to be hypnotized, and is at the same time, clear concerning its character and the results of suggestion, his consciousness cannot be deflected from the psychological process and he is hypnotizable with difficulty, or not at all, at least until he becomes psychically passive; and the more often and the more anxiously any one attempts to become passive, the less does he become so. Intense mental excitement, insanity, the firm resolve to resist the hypnotizer, are also conditions which make hypnosis impossible. Every mentally sound individual is hypnotizable. It is only certain temporary conditions of the mind which may prevent hypnosis.

It was often regarded as an axiom that one who did not desire to be hypnotized could not be so, at least not at the first trial. In my opinion too much importance should not be attached to this statement which depends more or less upon the psychological erroneous assumption of an essential freedom of the human will. Man must first be able not to will, in order actually not to will.

Suggestion acts most rapidly and certainly by surprising the imagination. As we have just seen, it is disturbed by long previous consideration. In a few seconds an easily suggestible individual who has never been hypnotized can be converted into the willing puppet of another individual, and I have observed that by a sort of contrast those individuals who mock at hypnotism and who declare ostentatiously that they cannot be hypnotized are often hypnotized most rapidly, if they do not offer direct resistance, and often even despite resistance. It seems as if the gauntlet thrown to hypnotism gives them an anxious notion of their own uncertainty, which delivers them up so much more surely to suggestion.

As a rule, uncultivated individuals are hypnotized with extreme ease by suggestion, without their noticing what is really done. They do and believe what is suggested to them, and sleep in one or two minutes before they are aware of it—often when only a moment before they had regarded other hypnotized persons as simulants and the physicians as dupes. The majority of the insane are undoubtedly hypnotized with the greatest difficulty.

It is also an important fact that, not infrequently, we can influence by suggestion a normally sleeping individual, and thus, without waking, lead him into hypnosis. To convert hypnosis into ordinary sleep is still easier.

Finally, there are very suggestible individuals who, in the complete waking condition, manifest all the phenomena of hypnosis without previous sleep, or yield thoroughly to the suggestions of an expert hypnotizer. In these cases there is no question of "I do not want to." This succeeds not infrequently even in an individual who has never been hypnotized heretofore.

As a general thing, the sleep produced by suggestion remains the chief measure for bringing the suggestion into complete action. It acts like a landslide after the first blow which it has delivered. The more it grows, the greater becomes its violence. Suggestion produces sleep or slumber; hardly is this present, when the suggestibility increases as the result of the sleep, and usually so much the more, the deeper the sleep becomes.

We said at the start that every individual is suggestible. If a man cannot be hypnotized, this can only be due to the fact that consciously or unconsciously he has made the auto-suggestion of non-hypnotizability. Prof. Bernheim has communicated to me in a letter the following case from his clinic, which he permits me to publish, and for which I express my thanks:

"A few days ago a peasant woman entered my service with pains in the stomach and abdomen which I regard as hysterical. I am unable to hypnotize her. She maintains, moreover, that Dr. Liébault vainly attempted to hypnotize her in her childhood. After two useless attempts, I say to her: 'It is immaterial whether you sleep or not; I will magnetize your abdomen, chest, and stomach, and thus relieve the pains.' I

close her eyes, and continue suggesting in this way for about ten minutes. The pain disappears without the sleep, but reappears after supper. On the following day I repeat the same procedure with the same result. The pain appears very slightly at night. To-day I begin again, and now, coincident with the disappearance of the pain I produce deep hypnotic sleep with amnesia." Dr. Bernheim adds: "Everything depends upon the suggestion. We must only find the spring in order to arouse each individual suggestibility into activity." This statement I can only confirm. On one occasion, Bernheim was unable to hypnotize an individual, and it appeared that this individual had been hypnotized by Beaunis, who had given him the suggestion that he alone could do it. I myself caused a lady to fall into a profound sleep with post-hypnotic suggestion, after Prof. Bernheim had only succeeded in producing somnolence—because she had formed the auto-suggestion that I alone could influence and cure her.

Sleep and Hypnosis.—The relationship of hypnosis to normal sleep is undeniable, and I agree with Liébault when he says that it is only distinguished as a matter of principle by the association of the sleeping individual with the hypnotizer. I may here be permitted to adduce a few of the principal facts.

It has always been said in physiology that sleep is produced by exhaustion; but this is not correct. Although real exhaustion of the brain usually produces the subjective sensation of being tired, we must, on the other hand, remember: 1, that severe exhaustion not infrequently causes sleeplessness; 2, that we often become still more desirous of sleep after sleeping; 3, that the tired feeling, drowsiness, and real exhaustion often occur independently of one another; 4, that drowsiness, as a rule, appears at a certain customary (auto-suggested) hour, and when it has been overcome, disappears despite increasing exhaustion.

These facts are entirely inexplicable by the very unsatisfactory chemical theories of the physiologists (lactic-acid theory of Preyer, etc.). The physiologists have attempted to measure the intensity of sleep by the intensity of the sounds necessary to cause waking. How little this proves is shown by the fact that an habitual sound soon ceases to waken, even when it becomes very loud (for example, an alarm-clock), while slight

unusual sounds awaken at once. Many anxious mothers are awakened by the slightest noise on the part of the child, while they sleep through the snoring of their husbands or other accustomed noises.

Quiet, as well as tiresome, monotonous processes, which do not stimulate a change of conceptions, make us sleep; also a comfortable position of the body, and darkness. At the same time, associated phenomena set in, such as yawning, nodding, stretching of the limbs, which increase still further the subjective feeling of drowsiness, and, as is well known, are very infectious from man to man.

We have said that the habit of sleeping at a certain time daily produces pronounced drowsiness at that time. A certain locality, the voice of a certain individual, lying in a certain rocking chair in which we usually sleep, listening to a sermon, lying in a certain definite position of the body, etc., etc., but particularly closure of the lids, are very easy methods of producing sleep—and why? It has hitherto been called habit, or associated habit; but we must acknowledge that these facts are entirely co-ordinate with unconscious auto-suggestion. My two-year-old son had become accustomed to sleep with a handkerchief in his right hand applied to the face. When it was taken away, for a long time he could not fall asleep. In some individuals, indeed, certain actions must be performed before sleep will follow (reading, winding the clock, etc., etc.), but the most vigorous of all these associations is heaviness of the lids, their uncontrollable drooping; hence this is the best suggestion of sleep. If we observe sleeping individuals we soon notice that they move; that they react to sensory irritation; that they cover themselves when exposed; not infrequently speak, groan or stop snoring at command; even answer questions; occasionally rise and walk about. Certain individuals sleep very lightly and awake at the slightest noise. These show more communication with the external world. Subjectively we know our sleep only by the dreams. We feel that our dreaming consciousness is different from our waking consciousness, but approximate the latter so much the more, the lighter the sleep. This sleep consciousness is distinguished from waking consciousness particularly by the following facts: 1, there is no sharp distinction between conception and perception; all conceptions are more or less hallucinated, that is,

they have the subjective character of perceptions, and simulate real events. 2. While these sleep or dream hallucinations usually lack the clearness and precision of the waking perceptions produced by external processes, nevertheless, they are associated with very intense emotions and may exercise vigorous reaction upon the central nervous system. A dream may produce perspiration and spasmodic contractions of the muscles, intense anxiety, etc. Erotic dreams produce pollutions without the mechanical rubbing of the penis, a thing which erotic perceptions during waking are rarely able to do. 3. Dream hallucinations, in opposition to thought and perception while waking, are associated very imperfectly. They are connected usually by very loose external associations. Thought during sleep lacks the logic of waking thought, which, in the course of life has become organized unconsciously and instinctively in the gradually automatized psychical dynamism. The greatest nonsense is therefore dreamt, associated in the dream in time and space, and at the same time believed. Usually only in light sleep, rarely in a deep sleep, is there a more or less high degree of logical correction.

These three characteristic properties of dream life are at the same time the criteria of hypnotic consciousness; hallucinations of the conceptions, intense emotional action, dissociation of the organic logical associations. At the same time these are the best conditions for intense suggestibility.

Awakening, the reverse of falling asleep, shows entirely the same suggestive phenomena as falling asleep. A light sleep often forms a gradual transition to waking and leaves behind memories of dreams. Not infrequently dreams cause waking. It is a peculiar characteristic of many individuals to be able to awake at a certain desired time, and thus to measure time accurately during sleep. This is also found in hypnosis.

As in hypnosis, so in normal sleep, Liébault distinguishes a light sleep with memories of dreams, from deep sleep without such memory. A characteristic of the latter is total amnesia on waking. Nevertheless we find in individuals with profound sleep the phenomena of somnambulism, in which the sleeping individual walks and acts (often in a very irregular and complicated manner), speaks and even exercises violence—a phenomenon which has been recognized in the statute books as a cause of irresponsibility. This shows that the amnesia after

deep sleep is simple amnesia, and by no means proves that consciousness is lost in deep sleep, only that it is changed. In Wundt's "Philosophische Studien," Friederich Heerwagen, under the direction of Kraepelin, has published "Statistical Investigations concerning Dreams and Sleep" which are based upon the statements of many individuals. The statements of these individuals that they dream a good deal, or little, or not at all, are decisive, according to Heerwagen, and form a basis for his statistics. But inasmuch as the study of hypnotism and many experiences concerning normal sleep prove that no reliance can be placed upon these subjective memories or non-memories of dreams, because many individuals simply forget all their dreams and almost all forget the greater part of them (auto-suggestion of amnesia), I can attach no weight to these statistics, and, on the contrary, believe that all individuals dream constantly during sleep.

This brings us to the consideration of amnesia as one of the most important, indeed as, forensically, the most practically important phenomenon of sleep and hypnosis. As a rule, one who normally sleeps deeply is also a deep sleeper in hypnosis, and such a one is usually left to the mercy of the hypnotizer. In him we can produce at will remembrance and amnesia for this or that period of his life, or, at least, of his sleep. Bernheim produced deep sleep in more than half of his hospital patients. In order to show the enormous degree of profound hypnotizability among normal individuals, I will merely state that among the present twenty-six female nurses in the Burghoelzli Asylum I have attempted in twenty-three, and in all successfully, to produce hypnosis. In one I produced only somnolence; in three, light sleep without amnesia; in nineteen, deep sleep with amnesia, post-hypnotic phenomena, and a suggestive condition when awake. In two of them catalepsy and anæsthesia were at once produced in the waking condition by affirmation; neither had ever been previously hypnotized.

Degree of Hypnosis.—The celebrated Charcot's phases, lethargy, catalepsy, and somnambulism, undoubtedly depend upon self-deception. Bernheim has attempted a division into many grades, but there are no boundaries. I find that it is sufficient to assume three grades, which, however, exhibit transitions: 1. Somnolence, in which the slightly influenced indi-

vidual, by employing all his energy, can resist the suggestion, and open his eyes. 2. Light sleep or hypotaxia or charme. The influenced individual can no longer open his eyes and must submit to a part of the suggestions or to all, with the exception of amnesia. He does not become amnestic. 3. Deep sleep or somnambulism, characterized by amnesia after waking, and by post-hypnotic phenomena. According to my experience, post-hypnotic phenomena also occur not infrequently after light sleep. Under certain circumstances the suggestibility may be very slight, or even almost nil (very rare cases) during very deep sleep. We can, however, also produce sleep with open eyes, successful suggestion in the waking condition as well as the amnesia, and, on the other hand, can produce memory by suggestion, so that all these three grades are very imperfectly defined. The main feature depends upon what is suggested at the beginning. With practice or training, by means of suggestion we can also convert the somnolence into hypotaxia, and the latter into somnambulism, at least not infrequently, by suggestion of amnesia.

Training.—Much has been said of the training of the hypnotized. It is certain that frequent hypnosis increases the suggestibility of the individual, and especially may cause him to do without verbal command everything that he had done in the first hypnosis, apparently instinctively. As Bernheim so truly says, the somnambulist appears to concentrate his entire attention upon divining the wishes of the hypnotizer. In recent times, however, particularly in Germany, the part played by training has been very much overestimated, and the extent of the individual suggestibility of the majority of normal individuals has not been recognized. Where is the training, for example, when I yesterday hypnotized for the first time a perfectly normal female nurse; I look at her for a few seconds, suggest sleep, then let her look at the two fingers of my left hand (according to Bernheim's method). At the end of thirty seconds her lids fall; I suggest amnesia; catalepsy of the arms; turn the latter and suggest anæsthesia. Everything succeeds at once. I prick deeply with a needle; she feels nothing. I administer water as a bitter mixture, which tastes bitter; then successfully suggest appetite, and tell her that after awaking she will, of her own volition, place a paper basket standing under the table upon the lap of one

of the individuals present, and that at six o'clock at night she will again voluntarily come to me. I then awaken her by allowing her to count up to four. She remembers absolutely nothing of what has happened, but looks steadily at the paper basket, which she places, shamefacedly and blushing, upon the lap of the individual in question. She is angry at this action, to which she has been driven uncontrollably without understanding why. At six o'clock she is alone in the ward, and therefore cannot leave, but feels the desire to visit me, becomes very excited and anxious because she cannot yield to this desire. Who can call this training? The young peasant girl only entered here recently and was hypnotized for the first time, and yet she acted almost exactly the same as a somnambulist who had often been hypnotized.

Phenomena of Hypnosis.—It may be said that by suggestion during hypnosis we can produce, influence, prevent (inhibit, modify, paralyze or stimulate) all the known subjective phenomena of the human soul, and a large part of the objectively known functions of the nervous system. The purely ganglionic functions and spinal reflexes, as well as the equivalent reflexes of the base of the brain alone appear to be uninfluenced by suggestion. Yes, even more. Suggestion may influence certain so-called somatic functions, such as menstruation, digestion, even the formation of epidermic vesicles, so that in this manner the dependence of these functions upon the dynamism of the cerebrum is most clearly proven. We do not mean to imply that these results can be effected with every hypnotized individual, but with patience the greater part can be produced in profound sleep.

These phenomena are produced by the simple affirmation that they are present, at best by touching the part of the body where they are to be felt subjectively and by describing (in a loud, convincing voice) the process of their development. We begin by placing the one to be hypnotized comfortably in a chair, looking at him, and assuring him that his lids are growing as heavy as lead, that they close, etc.; in short, by suggesting the phenomena of falling asleep. Illustrations:

Motor Phenomena.—Raising the arm, I say that it is stiff and cannot be moved. The arm remains in cataleptic rigidity (suggestive catalepsy). The same thing succeeds for every conceivable position of the muscles of any part of the body.

I say, the arm is paralyzed, and it falls like a mass of lead. This happens at once, and the hypnotized individual can no longer move it. I explain to the patient that he can speak and answer me. He begins to answer questions. In like manner he can walk, act, command, suffer from spasms, mumble, etc. I tell him he is drunk and totters; at once he walks like a drunken man.

Sensory Phenomena.—I say: "A flea is upon your right cheek; it itches." At once a grimace follows, and the hypnotized scratches himself upon the described spot. "You feel an agreeable warmth in the legs and arms." At once he affirms it. "You see before you a vicious dog; he barks at you." The patient jumps back in fear and drives away the supposed dog, whom he sees at once and hears. I place air in his hand with the assurance that it is a bouquet of violets. With joy he breathes in the aroma of the violets that are not present. From one and the same glass of water I can in a few seconds and sips make the hypnotized drink bitter quinine, salt water, raspberry juice, chocolate, and wine. Nor does he require either a glass or water. The statement that he has a glass of the corresponding drink in his hand suffices. Pain can be easily suggested, and when present, it can, above all, be suggested away. For example, headache can be relieved with ease in a few seconds, at the most, minutes.

Anæsthesia, amnesia, blindness, color blindness, double vision, deafness, inability to taste, may be easily suggested. I have had teeth extracted during hypnosis, abscesses opened, corns extirpated and deep incisions made, without sensation on the part of the hypnotized. The assurance that the affected part of the body is dead and insensible is sufficient. Surgical operations, even parturition, are possible during hypnosis, which then advantageously and without any danger supplants chloroform narcosis.

Bernheim properly applies the term negative hallucination to the remarkable deceptive perception of the disappearance of a present object. To a hypnotized individual who sleeps with open eyes I say that I disappear, and he no longer sees, hears, or feels me. He spontaneously supplements the hiatus in the visual field by positive hallucinations concerning objects. At my suggestion he can also hear and feel me without seeing me, etc.

Reflexes.—I say "You must yawn." The hypnotized yawns. "Your nose itches, and you must sneeze three times in succession." He at once sneezes three times in a natural manner. Vomiting, diarrhœa, constipation, etc., may be produced in the same way.

Vaso-motor effects are among the most wonderful phenomena. We can produce or discontinue the menstruation of women by simple prophecy during hypnosis; can regulate its duration and intensity. In two individuals I have effected the accuracy of their obedience to the very minute indicated, both as regards the beginning as well as the end of menstruation. Blushing and pallor may be produced; also redness of definite parts of the body or parts of the skin; epistaxis, even vesication and bleeding stigmata. These, however, are rare results. Furthermore, the pulse and respiration may be accelerated or slowed.

Feelings — Emotional Effects.— Appetite is easily suggested by affirmation. The effect of the suggestion may be increased by touching the stomach, or by permitting the ingestion of suggested food. Anxiety, joy, hate, anger, jealousy, love of any one or anything, etc., are easily produced by suggestion; also laughing and crying. Onanism, nocturnal enuresis were often cured in a similar manner.

Mental processes, memory, consciousness, will, are influenced in the same way. I say: "Everything that I have told you in your sleep will be forgotten by you, and you will only remember that you had a cat upon your lap and stroked it." After awaking the hypnotized has forgotten everything with the exception of the cat episode. To a young lady who spoke French well, my colleague Frank said: "You no longer know a single word of French until I restore it to you," and the poor girl could not speak French until the suggestion had been removed. This individual could be made temporarily mute and deprived of all her psychical faculties by simple suggestion. Since then, similar experiments have often succeeded in my hands. To a somnambulist I allowed long dead relatives to appear post-hypnotically, and with these she conversed for a long time. Others I allowed, like Peter, to walk upon the sea or upon a river. Others I transformed into hungry wolves or lions, so that they attacked me barking and wanting to bite me. One man I converted into a girl who

remembered her menstruation. On the other hand, I converted a girl into an officer. By the suggestion of childhood in good somnambulists speech and writing are changed in a corresponding manner. Such things often leave behind a profound emotional impression unless amnesia is suggested.

I may convey to the hypnotized any desired thought or notion. I may convey to him any conviction; for example, that he no longer desires wine; that he shall join this or that society; that he wants this or that which he formerly could not tolerate. In an alcoholic subject who had become false to her vows of abstinence I produced by suggestion, and without saying a word to her in her waking condition, pangs of conscience and remorse, open (spontaneous) confession to the president of the temperance society, and renewal of her pledge of abstinence. The success was very striking and followed immediately upon a single hypnosis, while nothing of the sort had been previously observed. The action upon the will is especially important and the volitions of the hypnotized may be influenced at pleasure. It has often been maintained that the latter thereby grows weak and infirm in will. This is a mistake which is due in part to the false assumption of an essentially free human will. Indeed, a weak will can often be strengthened by hypnosis.

Resistance of the Hypnotized—Auto-Suggestion.— Like Liébault, Bernheim, and others, I have produceed all of the above-mentioned phenomena and many others in my hypnotized individuals, but, as Bernheim justly emphasizes, we should not be blinded by the power of these almost terrifying facts, and therefore overlook the other side of the phenomenon, namely, the resistance of the cerebral activity of the hypnotized to these foreign attacks. The blind, automatic obedience or belief of the hypnotized is never complete. The suggestion always has its limits, which are sometimes broader, sometimes narrower, and may also vary greatly in the same individual.

The hypnotized resists in two ways: consciously, by his rational logic; unconsciously by auto-suggestions. I lift the arm of the hypnotized and say it is stiff. He attempts with spasmodic rage to bring it down, and finally succeeds, but the feeling of strain which he experiences brings him so much more certainly into my power, because it shows him my predominance. I say a second time: "I draw your arm forcibly

upward by magnetism." This suffices to prevent its falling. I keep my hand in front of his, and without touching it force him, by the power of his suggestibility, to lift it over his head.

But the resistance was there. If it is not overcome very rapidly, the hypnotized believes in his power of resistance and overcomes a number of suggestions. Indeed, some lose their suggestibility by vigorous reflections of reason and strength of will. This results more often from the fact that they are dissuaded by other individuals. As a rule, the hypnotizer retains what he has already gained, but if, in an unskilful manner, he has several times failed in a number of suggestions he can with difficulty regain them, inasmuch as the auto-suggestion that this or that cannot be effected is formed in the hypnotized. For example, I say to a hypnotized while touching his hand, that I make it dead and insensible; but he still feels and does not believe me, and when I ask, "Have you felt anything?" answers in the affirmative. In such cases it will be difficult to produce gradual anæsthesia. This is probably due in part to the insufficient profoundity of the sleep, but by no means always. I have produced total anæsthesia in simple hypotaxia in the following manner: I did not touch the fingers whose anæsthesia I had uselessly suggested, and allowed the hypnotized to believe that I had touched them but that he had not felt it. Then, in the next following hypnosis I succeeded by very slight touches in gradually producing a partial anæsthesia. The same thing holds good with regard to amnesia. If amnesia cannot be produced after two or three sittings it will prove extremely difficult. Finally, however, it sometimes succeeds through certain manipulations. For example, we give to the individual a sip of water with the statement that it is a sleeping potion which will make him amnestic, etc. In short, as Bernheim says, the hypnotized is not a complete automaton. He often discusses the suggestions, especially in the beginning, and discards some of them. I might say that the chief factor consists in communicating to the suggestion the suggestive character of a dream or something past, of a perception or action before the hypnotized becomes conscious of it. If he first becomes conscious of it as a simple conception it succeeds with much more difficulty or not at all.

Typical auto-suggestions are the product of the brain itself

and abound in hysterical, but also in healthy individuals. For example, an otherwise healthy woman was sleepless, but had a good appetite. I hypnotized her and successfully put her to sleep. In its stead the appetite has now disappeared. The loss of appetite depends upon auto-suggestion. This illustration suffices to characterize the entire series of phenomena. For example, every night when we fall asleep in the accustomed position after we have retired to bed, this happens from auto-suggestion.

A cultured and very intelligent lady, Miss X., had seen me hypnotize and had been very much interested thereby. The power of her imagination and her understanding of hypnosis are illustrated by the following: During the next night she was awakened by a violent toothache. She now attempted to suggest this away herself by imitating aloud my voice and the monotonous tone and contents of my suggestion. She succeeded completely in driving away the toothache and falling asleep. In the morning on waking it had remained away.

This lady then told me the means employed by her lady friends to delay menstruation at pleasure, if it threatened to appear on the evening of a ball. They simply placed a somewhat narrow red thread around the little finger of the left hand. This measure did not act with equal certainty in all persons, but in some who had menstruated with extreme punctuality it acted with absolute certainty and could delay the menstruation even for three days. This woman is entirely trustworthy and the case is a striking illustration of unconscious suggestion, as became evident to her after she had seen me operate. In the mildest form of hypnotic influence, in the somnolence of Liébault and Bernheim, the hypnotized can resist with some exertion every suggestion, and only becomes somewhat suggestible when he allows himself to be passive.

Post-Hypnotic Phenomena.—Among the most important phenomena of hypnotism, especially from a forensic point of view, are the post-hypnotic effects of suggestion. Everything that is produced during hypnosis can often be produced in the waking condition by giving the suggestion to the hypnotized during hypnosis that it will happen after his awaking. Not all hypnotized are suggestible post-hypnotic, but with some practice and repetition we can produce post-hypnotic effects

in almost all profound sleepers, and even in many cases of simple hypotaxia without amnesia.

Illustrations.—I say to a hypnotized: "After waking you will have the idea of placing this chair upon a table, and then clap me upon the left shoulder with your right hand." I then make other remarks, and finally say: "Count up to six, and you will awake." He counts, and when he reaches six opens his eyes. He looks around for a moment as if drowsy, sees the chair, and stares fixedly at it. A battle often develops between reason and the powerful impulse of the suggestion; according to the degree of unnaturalness or naturalness of the suggestion on the one hand, and of the suggestibility of the hypnotized on the other, the former or the latter gain the victory. But, like other experimenters, I have repeatedly observed that when the suggestibility is marked, the attempt to resist the impulse of suggestion may have evil consequences. The hypnotized becomes anxious, excited, and annoyed by the thought that he must do it nevertheless. In two cases, indeed, a hypnotized individual was about to take subsequently a walk of three quarters of an hour, at one time, in order to clap me on the shoulder, and at another time, to hand Miss Y. a handkerchief. This desire may continue for hours and days; at other times it is feeble, may even be a mere thought like the memory of a dream but which does not lead to a conclusion, so that the suggestion is not carried out. The hypnotized merely looks at the object, or does not even look it. In such cases, however, by vigorous repetition of the suggestions during hypnosis the desire, and finally its execution, may be produced. Our hypnotized has now gazed at the chair. Suddenly he arises, takes the chair and places it on the table. I say: "Why did you do this?" The answer varies according to his culture and temperament, and the quality of the hypnosis. One says frankly: "I felt the desire. I was compelled to. I do not know why." The other says, "The idea just came to me." Another advances an *a posteriori* motive. "The chair was in his way, annoyed him" (or, after the suggestion that he would obtain a handkerchief and wipe his face with it, said "that he perspired so freely"). The fourth, after performing the action, has lost all memory of it; believes that he has just awakened. It is particularly in the latter event that the hypnotized has the appearance of a somnambulist. His gaze

is more or less fixed. His movements have an automatic character which is lost after the completion of the act. If the experiment is made for the first time upon one who knows nothing of hypnotism, and who is really entirely amnestic concerning the period of hypnosis, in my experience he will never suspect that the hypnotizer is the producer of his actions. Many suspect it either because they retain a dreamlike memory of the suggestion in hypnosis or because the experiment has already been made upon them, or because they had seen or heard or read of it in others.

I say further to a hypnotized: "After awaking you will see me dressed in scarlet red and provided with two goat's horns upon my head. At the same time my wife sitting next to me will have disappeared, and also the doors of the room, which will be entirely replaced by carpets and portières, so that you will be forced to go through the other doors." I now speak of other matters, allow the hypnotized to yawn three times by suggestion, and thereupon to awaken. He opens his eyes, rubs them several times, as if he wished to remove a cloud, looks at me, begins to laugh and again rubs his eyes. "Why do you laugh?" "You are all red and have two goat's horns upon your head. Your wife is gone." "Where did she sit?" "Upon this chair." "Do you see the chair?" "Yes." I ask him to touch the chair. He does not do it willingly; feels around my wife; thinks that he sometimes feels the chair, sometimes an invisible resistance (according to the manner in which he has supplemented the suggestion by auto-suggestion). Then he wants to leave, but is unable to; sees only the carpets and portières, and also maintains this after touching the doors. When I now open the doors the hallucination may disappear or continue. In the latter event he sees the empty space filled by carpets and portières, but does not see the open doors. According to the suggestion and the individual these post-hypnotic hallucinations may continue from a few seconds to hours, or even days. As a general thing they only last a few minutes. I have attempted to make the hypnotized trace pictures, which I had suggested, upon white paper. The drawings, as a rule, were poor; the individual stated that they saw the pictures indistinctly. A few were not poor. Bernheim relates that a lady was unable to say whether a rose suggested in this manner was real or suggested. I have often

made the following experiment: I told Miss Z., during hypnosis, that after awaking she would find two violets upon her lap, both natural and beautiful; she would give me the more beautiful one. I, however, placed a real violet upon her lap. After awaking she saw two violets; the one was brighter and more beautiful, she said, and gave me the corner of her white handkerchief, retaining the real violet for herself. I now asked whether she thought that both violets were real, or whether one was one of my temporary gifts which she knew from previous experience. She said that the brighter violet was not real because it looked so flat upon the handkerchief. I repeated the experiment with the suggestion of three real equally dark, by no means flattened violets, and provided with palpable stem and leaves, but gave her only one real violet. This time Miss Z. was entirely deceived and was unable to say whether one of the violets, or two, or even all three, were real or suggested. She thought that this time all three were real, holding in the one hand air, in the other, the real violet. If the deception is suggested for all the senses, it becomes more complete. For example, I give another hypnotized individual a real knife and tell her there are three of them. She is perfectly awake and cannot distinguish the supposed three knives from one another, neither in cutting nor on feeling them, nor tapping upon the window with them, etc. When other persons afterward laughed at her, she became angry and firmly maintained that there had been three knives, but that I had abstracted two. She had seen, felt, and heard all three knives and could not be convinced to the contrary.

Feelings, thoughts, convictions, etc., may be suggested post-hypnotically as well as hypnotically. The results obtained in the above-mentioned inebriate and in the menstruation of women were post-hypnotic. In only two cases did I succeed in producing or checking menstruation at once during the hypnosis.

Amnesia or Loss of Memory.—We must here warn against the old mistake of confounding this notion with that of unconciousness. That we no longer know anything concerning a definite period of our life, or certain events through which we have passed, by no means proves that we were unconscious at the time, even if the amnesia at once follows this period. Yet, as a general thing, we have no other proof but this very am-

nesia that an individual was unconscious. This is almost tantamount to the declaration that it is impossible to demonstrate unconsciousness. We can only speak of a probable obscuration of consciousness. As a general thing, though not always, an individual is amnestic concerning the period of profound obscuration of consciousness. On the other hand, by suggestion we can at will make the individual amnestic concerning facts and periods of life in which he had been perfectly conscious, Amnesia of a certain period, therefore, does not entail absolute unaccountability during this period.

Amnesia plays a very important part in hypnosis. I will illustrate its significance by a single illustration: In a man suffering from toothache I attempted to produce anæsthesia by suggestion. This only succeeded partially, but the extraction of the tooth was nevertheless performed. He awoke at the time, cried out, grasped the hand of the physician, and struggled. I continued to suggest, as the tooth was removed, that he was sleeping very well; that he felt nothing at all; that on waking he would have forgotten everything, and that he had no pain. He then slept and on waking was amnestic concerning everything. He therefore imagined that he had felt nothing and was very glad and thankful for the painless extraction of the tooth. At a later period I had him questioned concerning this by a third person from whom he had absolutely no reason for concealing the truth. He said, however, that he felt nothing at all. On the other hand, I have had teeth extracted in waking individuals who had been made anæsthetic by suggestion. During the extraction these individuals, who otherwise dreaded pain extremely, laughed and experienced not the slightest sensation. In the former case we had merely inhibited by suggestion the consciousness of the memory image; in the second event, we had inhibited the consciousness of the peripheral irritation itself.

Suggestion à Échéance (*Suggestion for a Definite Period*). —This phenomenon, so ably described by the Nancy school, is only a variety, but a practically very important variety, of post-hypnotic suggestion. I say to a hypnotized: "Tomorrow at 12 o'clock while you are on your way to your dinner the thought will suddenly occur to you that you must write me at once how you are feeling. You will return to your room and write; will then suffer from cold feet and put

on your slippers." After awaking, and until the following day at 12 o'clock, the individual has no suspicion of the entire matter. At the moment he is going to dinner the suggested thought rises in his consciousness and is punctually carried out. To another I say, on Monday: "Next Sunday morning, at 7:15 o'clock sharp, your menstruation will begin. You will at once go to the supervising nurse, tell her the fact, and then come to me and report. You will see me in a sky-blue coat and with two long horns upon my head, and will then ask when I was born." On the following Sunday I was seated in my study and had entirely forgotten the matter. The hypnotized individual knocks at my door at 7:35, enters, and almost bursts with laughter. I at once remembered my suggestion, which was now carried out word for word. The menses had appeared punctually at 7:15 o'clock. This had been reported to the supervising nurse, etc. In the waking condition the individual had previously had no suspicion of the entire matter, not even of the period at which the menstruation was to occur. The enormous importance of suggestion à échéance is evident. We can determine the thoughts and resolutions of the hypnotized for a certain period in advance, when the hypnotizer is no longer present. In addition, we can supply the suggestion of a free effort of the will. We may also suggest that the hypnotized will have no suspicion that the desire is due to the hypnotizer. Indeed, in very suggestible individuals we may even successfully suggest total amnesia of the hypnotization: "You have never been hypnotized; if you are asked, you will swear that no one has ever put you to sleep. I have never put you to sleep." It is very evident to me that herein lies the most dreaded forensic danger of hypnosis. Of the above-mentioned nineteen healthy nurses who sleep soundly during hypnosis, no less than thirteen carried out suggestions à échéance. Hence the symptom is by no means rare. In one nurse, as already mentioned, it succeeded even at the first hypnosis.

The opinions of the hypnotized concerning the source of the successful suggestion à échéance are extremely remarkable. When asked how they happened to do so and so, they usually state that it was an idea which occurred to them at the suggested time and which they were compelled to execute. They constantly give the time at which the idea occurred to

them, while, as a general thing, one does not look at the clock with every passing thought; hence this is to be regarded as a co-effect of suggestion. Because the period of time has been suggested, they pay attention to it. Furthermore, the suggested idea develops suddenly at the suggested time, at least usually. In a few cases, however, it appears for quite a time before. It seems to the hypnotized "as if in a time to come he must think or do this or that." In rare cases the idea does not develop with the subjective character of spontaneity, but as a suddenly appearing memory of the hypnosis. Then the hypnotized individual says, for example, "Suddenly, at 12 o'clock, I remembered that you told me yesterday during the sleep, that I should come to you at 12 o'clock to-day." As a general thing, the suggestion of a definite period possesses the character of a force, an irresistible desire, until it is completed. The intensity of the desire, however, varies greatly. Practised somnambulists generally recognize by these qualities that they have to deal with suggestions and not with their own idea or volition; but it is usually not difficult to deceive them, if we in advance suggest away the character of unnatural force, and replace this by a free, spontaneous volition, skilfully and logically associating the suggested thought with actual occurrences. In this way it is not difficult to deceive the somnambulist so that he remains perfectly convinced that he has acted spontaneously and with uninfluenced will.

It is a most astonishing fact that the contents of the suggestion rarely become conscious in their waking condition during the period from hypnosis to the period suggested. But if, on the other hand, the individual is hypnotized during this interval and he is asked during the hypnosis what he must do at such and such a time as a rule he knows it accurately. Bernheim therefore concludes, in my opinion without justification, that the hypnotized individual thinks of the matter during the entire period, but is not conscious of it. I do not believe that we may express ourselves in such a manner because it disturbs our psychological conceptions. We have to deal with unconscious thinking or knowing, with a cerebral dynamism which slumbers below the level of consciousness and which is repeated by means of a sign of time that is associated with it and with the suggested period. In this way alone can we explain the suggestions à échéance

which Liébault, Bernheim, and Liégeois successfully carried out even at the end of a year. In suggestions at a brief period, the feeling of time without a sign of time may suffice to produce the suggestion at the correct period. The phenomena of suggestions à échéance are identical with those of other post-hypnotic suggestions.

Waking Suggestions.—In very suggestible individuals in the full waking state we can employ suggestion successfully, without producing hypnotic sleep, and can thus produce all the symptoms of hypnosis or of post-hypnotic suggestion. We lift the arm and say: "You can no longer move it," and the arm remains in cataleptic rigidity. We may suggest anæsthesia, hallucinations (also negative ones), amnesia, mutism, hallucinations of memory, in short, whatever we wish in this way, with as certain results as in hypnosis. And the waking suggestion can be effected not alone in hysterical individuals, but also in perfectly healthy ones.

As a general thing, waking suggestibility can be effected only in individuals who have been placed in hypnotic sleep one or more times. Yet pronounced suggestive effects may be produced in waking individuals who have never been hypnotized. In a very intelligent lady of my acquaintance, of firm character, the arm was fixed in a cataleptic condition by the suggestion of the magnetizer while she was completely awake; she had never known anything concerning hypnosis. I succeeded in two out of four non-hysterical women in whom I made the experiment.

By suggesting waking suggestibility during hypnotic sleep we can also produce the waking suggestibility in those cases in which previously it has not appeared to be present. It is then suggested spontaneously. I am firmly convinced that only the necessary practice and boldness are requisite in order to produce waking suggestibility in a large percentage of normal individuals, inasmuch as I have succeeded in all of the above-mentioned nineteen deep-sleeping nurses.

Condition of the Mind during the Performance of Post-Hypnotic Suggestions, Suggestions à Échéance and Waking Suggestions.—Whoever has often observed all of these phenomena will soon be convinced that the condition of the mind of the hypnotized in the three cases just mentioned must be and is the same; the soul is awake and yet changed. How

changed? Liégeois ("De la suggestion hypnotique dans ses rapports avec la droit civil et le droit criminel," Paris, 1884), Beaunis ("Recherches expérimentelles sur les conditions de l'activité cérébrale," etc., "Somnambulisme provoqué," p. 67), and then Delbœuf ("Revue de l'hypnotisme," 1ère année, 1887, p. 166), were the first to attempt to answer these questions. Liégeois applies the term "Condition Prime" to this condition, in which the hypnotized individual is perfectly awake and normal with the exception of that point which the hypnotizer has prohibited or commanded. This condition is the analogue of the "Condition Seconde" which Azam applied to the second condition of consciousness of his Felida (case of double consciousness in the waking condition). Subsequently, however, Liégeois came to the conclusion that the "Condition prime" is only a variety of the "condition seconde." Beaunis calls the "condition prime" "veille somnambulique." Delbœuf, on the other hand, thinks that he has proven that in all of these cases the hypnotized individual is simply re-hypnotized, and that we have to deal, therefore, with ordinary somnambulism, except that it occurs with open eyes. The suggestion simply produces unconsciously a new hypnosis by association. [After I had finished this work I found an article by Delbœuf in the "Rev. de l'hypnot." (April 1st, 1888) in which he expresses a different opinion concerning the phenomena in question and concedes the gradual transition of the waking condition through suggestive conditions while waking into hypnosis proper. As I arrived at the above-mentioned conviction independently of him, I leave the text unchanged.]

In my opinion none of these views hold good, because all are too dogmatic. Delbœuf is undoubtedly right with regard to many cases. In post-hypnotic suggestions, suggestions à échéance, and in the waking condition, the actual occurrence of the suggested phenomenon may produce the auto-suggestion of hypnosis; the gaze becomes fixed and the hypnotized subsequently may be amnestic concerning everything. But if we attempt to generalize these cases we deceive ourselves as much as if we were to generalize the undoubted cases in which the suggestion is effected in a perfectly clear waking condition. We can eliminate by means of suggestion, everything hypnotic from these conditions, until they grow constantly more identical with the complete waking condition. There are all

stages, from the fixed to the perfectly clear gaze; from the uncritical automatism, in which the grossest nonsense appears as natural and self-evident as in a dream, to the finest and most severe self-criticism of the hypnotized individual and the most furious struggle against the force of the suggestion. Indeed, we may restrict the suggestion to such natural and trifling details, which we weave repeatedly into the chain of thought, that there can no longer be even a question of a "condition prime" in Liégeois' sense. Apart from the individual variations I have observed that the conditions under discussion approximate hypnosis so much the more when we suggest a more coherent, and at the same time, fantastic complex; but that, on the other hand, we approximate the normal waking condition when the suggestion is more natural, probable, restricted, and briefer. The subject will be explained most clearly by illustrations.

Lifting the arm of a woman who is thoroughly awake, I tell her she can no longer move it. She is astonished, attempts without success to lower the arm, is abashed, etc. I then add the following suggestions in rapid succession: "Here comes a lion; do you see him? He will eat us. Now he goes away. It is growing dark. The moon shines. Look at the large river there with the thousands of fish. Their bodies are stiff and they are unable to move, etc." In a few seconds all these impressions course through the consciousness of the woman as sensual perceptions with the corresponding feelings, and the condition of her mind approximates more and more to ordinary hypnosis. Things appear to her as if in a dream.

On the other hand, I say to the same hypnotized nurse: "Every time when the assistant physician passes through the ward and you report to him the condition of the excited patient, Louise C., you will make a mistake and call her Caroline C. You will notice it, attempt to correct yourself, but will be unable to do so, and will always say Caroline instead of Louise. Every time that you wish to address the assistant physician by calling him Doctor, you will at the same time, without noticing it, scratch the right side of your forehead with your right hand." The suggestion is carried out. In the middle of ordinary conversation the nurse constantly makes a slip of the tongue and says Caroline instead of Louise C. It is like the suggested paraphasia of a word. She notices it,

attempts to correct herself, but again makes the same mistake and is astonished thereat. Almost every time that she addresses the assistant physician by name she scratches herself as was suggested to her. It is astonishing to see how the unsuspecting nurse almost daily wonders at her mistake in pronouncing C.'s name, excuses herself, and says she cannot understand what is the matter that she always makes a mistake in this name; that such a thing had never happened to her before. At the same time, the scratching occurs instinctively with outher noticing it. Now, at the end of several weeks, she begins gradually to help herself, as she leaves the surname off, and says only "C." A single suggestion sufficed for this disturbance which occurred during such a long period. It would here be necessary to assume that the "condition prime" continued only during the articulation of the surname and during the scratching, while the remainder of the conversation occurred in the normal waking condition; but while she is scratching she says things that were not suggested to her and are thoroughly rational. Hence the "condition prime" exists only for a part of her mental activity.

To a cultivated young man (a student) I gave the suggestion during hypnosis that after awaking he would tap my right shoulder with his left hand. He resisted the desire, because he is very obstinate and was unwilling to yield an iota of the freedom of his will at any price. He went home. I had asked him to return at the end of a week, and on his return he confessed to me that my suggestion had bothered him for a week, so that several times he was on the point of coming to me (a distance of three-quarters of a mile) in order to tap me on the shoulder. Was a "condition prime" present during this whole week in which the individual worked as usual, heard lectures, slept, etc.?

In an intelligent, very suggestible nurse, the suggestions à échéance acted so powerfully that she complained to me she was absolutely overcome and would be compelled even to commit murder should I suggest it to her, so fearful was the desire to carry out even the grossest nonsense. Her repeated vigorous attempts at resistance only increased the desire so much more violently. At one time she spoke to me in the presence of two persons concerning hypnotism, and said: "But, Mr. Superintendent, although I must do everything

that you suggest to me during sleep, yet I know that it comes from you, although I knew nothing of it beforehand; it is a very peculiar desire and has a strange character." I said to her: "Fall asleep." She slept at once. I then said: "Half a minute after waking the idea will occur to you spontaneously to ask me the following question: Ah, Mr. Superintendent, I have often wanted to ask you how it comes that we fall asleep so suddenly in hypnosis. In ordinary sleep this is not so. We fall asleep more slowly. How does this come; it is so very astonishing. You will then have no suspicion that I told you this during the sleep; will think that the idea comes entirely from you, and that you long wanted to ask me. Now, count to six, and you will be awake." She counts, awakens, and assures me that she has slept very well. Then, at the end of about half a minute, she blurts out the suggested phrase, word for word, her questioning tone expressing the greatest interest. I listen quietly, answer her, and then ask how she happened to ask me this question. "Well, I wanted to ask you that for a long time." "Is it not a suggestion which I just gave you during sleep?" "By no means; I cannot be mistaken; this is my own idea." "And yet you are mistaken; here are two witnesses who have heard me suggest this to you word for word two minutes ago." The poor hypnotized woman was greatly confounded and was compelled to acknowledge that not every suggestion could be recognized by her as such, but only those which were sufficiently grotesque not to be the product of her own brain.

I could adduce many other illustrations, because I have devoted special attention to this part of the subject. The above mentioned post-hypnotic hallucination of a woman who could not distinguish two suggested violets from a real one, belongs, for example, to this category. But it will suffice to show that a suggestion can be smuggled into the normal activity of the waking normal mind in such a way that every hypnosis-like phenomenon is excluded. In these cases the hypnotized individual is completely deceived; believes that he thinks or wills spontaneously, and does not suspect the will of the hypnotizer.

Spinoza's dictum that the illusion of a free will is nothing more than the ignorance of the motive of our volitions, has never been proven more conclusively than by this hypnotic

experiment. It is a perfect ocular demonstration that our subjective free will is conditioned objectively. The only difference is that in the hypnotized it is influenced by the suggestions of another and in the non-hypnotized by auto-suggestions.

The interesting and frequent transition form between hypnosis and the waking condition consists in the fact that the hypnotized, while his eyes are open and he acts like a normal individual and forgets nothing, has a slightly fixed gaze, finds unnatural and nonsensical suggestions natural, that is, he is not astonished at them and carries them out without discussion. If he is questioned afterward, he not infrequently states that he felt somewhat dizzy or as if in a dream; he was not thoroughly awake and clear in his mind.

Permanent Results of Suggestion.—Can we change permanently by suggestion the soul or any nervous function, even in a single special point? This question has often been put, but has hardly been answered satisfactorily. Suggestions à échéance have been given for the duration of a year. Sleep lasting days has been produced by suggestion and particularly a series of permanent therapeutic results have been brought about. On the other side, however, every one who concerns himself with hypnosis must admit that with time the action of an hypnosis is enfeebled and that the hypnotized individual, after the prolonged absence of the hypnotizer, gradually ceases to remain under his influence.

The therapeutic results of hypnosis, carefully considered, appear to me to give the best explanation of this apparent contradiction. I believe that a permanent result can be effected only: 1, either when the desired change itself contains the power to be converted into auto-suggestion or habit by a single or repeated suggestion, and to maintain itself in the struggle for life between the individual dynamisms of the central nervous system; 2, or when this power which it lacks is secured by auxiliary remedies which, it is true, can be secured in great part by suggestion. We must then always furnish the suggestion that the result will be permanent, but as a matter of experience this rarely takes place completely.

Illustrations.—1. A child has retained the bad habit of wetting the bed. By suggestion he is forced to arise at night and to urinate into the vessel, finally, even to retain the urine. The bad habit was replaced by a good one which at

the same time acts normally and easily becomes fixed. The child had accustomed himself to sleep in the wet. Now he becomes accustomed to remain dry. Even the dream of wetting will awake him. Here we can produce permanent recovery. 2. Some one suffers from migraine, insomnia, anorexia, a tired feeling, and, as a result, has become anæmic and emaciated. I succeed in securing sleep and appetite by suggestion. Hence the anæmia soon disappears; the hypnotized individual improves in nutrition and bodily weight. The sleep cures the nervous exhaustion, and thus the migraine (which can also be suggested away directly for the moment). Thus the equilibrium of the organism is restored and recovery becomes permanent when the cause which has produced the disease does not return or is not a permanent one.

For this reason I believe that suggestion is able not infrequently to remove acquired vices and bad habits; also certain acquired diseases, especially by the aid of auxiliary remedies, but that it will never change permanently hereditary or constitutional peculiarities of character. In such cases it will only have a temporary effect, as so often happens in severe and especially in deeply rooted maladies.

Every prolonged result of a suggestion, when it refers to acts in the waking condition, is *eo ipso* post-hypnotic. It thus belongs logically to Liégeois' "condition prime," for example, suggested menstruation, suggested cheerfulness, suggested disappearance of home-sickness, etc. If we carry this out to its logical conclusion, an individual permanently cured by suggestion would remain for his lifetime in the "condition prime." By this statement I only wish to show distinctly that there is no boundary between the changed condition of the mind in hypnosis and its perfectly normal activity in the waking state. All shades and transitions can be produced experimentally. Even without suggestion, in some individuals quite delicate transitions can be observed between spontaneous sleep and the waking condition. These are produced by the accident of auto-suggestion and therefore are by no means so finely shaded and not so systematically graded as can be effected by suggestion.

Retroactive Hallucination or Suggested Deception of Memory.—Bernheim applies the term retroactive hallucination to the suggested remembrance of things that have never

been experienced. I do not consider this expression appropriate because we do not have to deal with an effective actual perception and not necessarily with the memory of perceptions (it may also be the memory of a thought, a feeling, an act). Nor is it exactly the same as the phenomenon termed deception of memory in psycho-pathology, because this is always the projection into the past of an actual complex of perceptions. In my opinion, however, the suggested process is psychologically co-ordinate with the deception of memory. The notion is merely somewhat enlarged.

Illustration.—I suddenly say to Miss X. (she was awake), at the moment that an entirely unknown young man enters the room: "You are acquainted with this gentleman. A month ago at the railroad depot he stole your purse and ran away with it, etc." She looked at him at first with astonishment, but was at once convinced, remembered accurately, and even said that her purse contained twenty francs. Finally she demanded the punishment of the individual. If I can suggest amnesia successfully concerning any previous period of time, or concerning certain cerebral dynamisms of an individual (for example, an acquired language), I can suggest equally well a fictitious plus of memories, in so far as I can bring the corresponding conceptions into his brain. If I tell the hypnotized individual: "You can speak Sanskrit," he will not be able to do so (if he has not learned the language). But if I say: "You have experienced, done, said, thought this or that, etc.," he believes that he has said, done, and thought so, and completely assimilates the suggestion with the memories of his previous life, and even supplements them where the hypnotizer has left gaps (for example, in the above-mentioned case, the contents of the purse). A boy of eight years, whom I demonstrated to the Legal Society of Zurich, swears, at my suggestion, that one of the lawyers standing before him robbed him of his handkerchief a week before. He adds, when asked concerning it, the exact place and hour. Five minutes later I suggested to him that this had not occurred and that he never maintained it. With equal boldness he denies the testimony which he had given a moment before.

It is Bernheim's great merit that he explained this important fact clearly by means of numerous illustrations. He has even given such retroactive suggestions collectively and thus

produced a series of false witnesses who gave their testimony with the most profound conviction. He has pointed out that particularly in children, who have an instinctive tendency to accept everything, to a greater or less degree, that is told them in a certain tone by adults, such deceptions of memory are produced very easily in the complete waking condition. But inasmuch as in many cases, when strong impressions act upon the fancy, suggestion may be successful even without a previous hypnotic sleep, particularly in children and weak individuals, we see how great is the danger of suggesting false testimony, especially false confessions, at the hands of judges. Bernheim has also pointed out that, in fact, such cases have undoubtedly occurred in criminal procedures. Lawyers will certainly be able to find many such in the history of notorious cases.

In psychiatry we have long been acquainted with cases of false self-accusations, in which the insane accuse themselves with the most accurate details, of a crime which had not been committed, and presented themselves to the court for punishment. We are also acquainted in them with the occurrence of false accusations against others. These facts have been regarded hitherto as simple delusions depending upon the basis of the delusion of committing crime, or of persecution, or upon hysteria or mania, etc., and this is generally true. The patients are convinced of the fact; indeed delusions in general are imperative auto-suggestions dependent upon insanity. But there are cases in which these self-accusations possess a typical suggestive character and are associated with very slight mental abnormality. I have observed a man who accused himself of a murder which had been committed in reality by another; this individual presented only a slight melancholic depression. In a few days he became rational and stated that the murder in question had created a profound impression upon him. Shortly before, he had spoken with a woman who had harbored the murderer and it then seemed suddenly to him as if he had committed the murder. All the individual circumstances which his fancy depicted appeared to him as if they had been experienced by him. He was convinced, and could do no other than place himself at the disposal of the police and to confess everything. Now it was clear to him that this was all a deception, like a dream.

A similar condition is found in many hysterical individuals and fanciful liars. These individuals lie constantly to others and to themselves, but are, in fact, unable to distinguish what has actually occurred from what they have invented. They feign half consciously or unconsciously. Psychologically they are misconceived entirely if the importance of conscious lies is attributed to their false statements. They are instinctive liars; they can do no other than lie, even when they are reproached, chastised, or despised, when all possible strict and stern measures are employed to make their lies hateful to them, they continue automatically, unconsciously, to feign the most stupid and useless stories. In my youth I observed carefully such an unlucky comrade and attempted to cure him in the most different ways without any result. He had inherited this auto-suggestive character from his mother, whom he had never known, as she abandoned him a few weeks after his birth. We have to deal here with a constitutional, cerebral, or mental error which is probably not unrelated to an habitual morbid auto-suggestibility.

Simulation and Dissimulation of Hypnosis.—From all this it must be evident to every rational man: 1, that the statement of those sceptical "esprits forts" who briefly dispose of hypnosis as a swindle, is based upon prejudice without personal examination of the facts; 2, but that, on the other hand, as all the better experimenters have shown, careful criticism and self-criticism are necessary in hypnotic experiments. There are people who, from a morbid thirst for swindling or lying, simulate the symptoms of hypnosis half unconsciously. These are usually hysterical individuals, or the above-mentioned liars, but as these people, as we have seen, believe their own lies, their hypnosis is neither entirely real nor entirely simulated. They sport with it, add auto-suggestions, often obey those suggestions which suit their fancy, and the like. The more fantastic and theatrical the suggestion, the better it usually succeeds in them. These are extremely unreliable witnesses. Certain schools, particularly that of the Salpêtrière, have fallen unfortunately into the great mistake of utilizing such individuals as the basis of their experiments. But there are others who believe that we only wish them to appear asleep, and who simulate "as a favor to the experimenter." Bernheim calls attention to this class. It is very

easy, however, to discover the source of this deception, by self-control and by carefully put questions. On the other hand, there are vain individuals who are afterward ashamed to have been hypnotized, and who maintain that they have simulated, although, in reality, they had been well hypnotized. Bernheim has called special attention to these cases, which I have also observed a few times. If this is suspected, a few properly applied suggestions usually suffice to compel them to make a spontaneous confession of their false statements. Others, again, have a real belief that they have not been hypnotized because they are not amnestic. "They did not attempt" to draw the arm down. A bold challenge here suffices to convince them: "Now try with your whole force; I will allow you; I beg of you; but you are unable to do so." If we manifest distrust of a hypnotized individual, we may in this manner, without noticing it, give him the suggestion that he has simulated, and thus induce him to make a false confession of simulation (auto-suggestive deception). I have seen a classical case of this kind caused by a distrustful physician.

The hypnotized individual, a man, came to me in tears, and confessed that he had not slept; that everything had been a fraud. He had felt all the pricks of the needle; had only performed the post-hypnotic phenomena in order to favor me, etc Next to him stood, with a grave countenance, the physician, who had obtained from him this confession (as a matter of course by suggestive questions, put with the best motive). I believed him apparently, gave the hypnotized individual a lecture, told him he should be ashamed of himself to manifest such want of character, and made him promise that henceforward he would tell the whole truth. This he did with the most profound emotion. Touching as the scene was, I knew very well that he had not simulated, because he had been deeply hypnotized, entirely somnambulistic. His facial expression during hypnosis and on awaking, could not have been simulated. Immediately after the promise and the reconciliation I again hypnotized him in the presence of the physician. I then suggested anæsthesia of the hand. He still felt the first two pricks of the needle, and stated this during the hypnosis. The subsequent ones he did not feel and at once denied that he felt them, and the other suggestions succeeded as before. After awaking, he stated that he had felt

two pricks of the needle, of all the rest he knew nothing, although the many later pricks of the needle were much deeper than the first. The hypnotized man was thus quieted and the physician received a lesson.

It is evident that the last two categories of deceptions offer no great difficulty, while the first can often not be cleared up on account of its inextricable confusion with true hypnosis. There remains only a clearly purposed simulation for a definite reason. This may occur, and may deceive at the start, because we must be cautious in the first hypnosis of an individual, but the simulant runs the risk of being caught, that is, of being hypnotized, if he plays his part too well. If he does not, he will not long deceive an expert experimenter. Moreover, the matter merely possesses the value of a tolerably stupid joke which few will perpetrate, especially not the sick who desire relief.

Significance of Suggestion.—In view of what has already been said, we may discuss this briefly. The chief importance of suggestion is psychological and psycho-physiological. It gives to the psychologist the experimental method of natural science, which he has hitherto lacked. What a wonderfully fine and manifold reagent it is with which all the qualities of the soul, in the finest shades of ethics, logic, and æsthetics can be influenced and modified.

On careful consideration, suggestion also appears as an inroad upon the associated dynamics of our mind. It dissociates what was associated, and associates what was dissociated; but its main action is inhibitory, a dissociation of the associated (unconscious) mental (cerebral) automatisms. The dissociated dynamism of the cerebrum of the hypnotized is in a condition of weakness and hypotaxia as opposed to the well concentrated and associated dynamic of the hypnotizer, which is forced upon it by means of the organs of sense. Its activity becomes plastically pliable and must yield irresistibly to the suggestion. The cause of his subordination does not reside in an especial strength on the part of the hypnotizer, but in the subjective feeling of subordination on the part of the hypnotized. In normal sleep we are all in a condition of hypotaxia or weakness, of dissociation, and confuse all our thoughts (dreams) with actual occurrences. Hence sleep is so useful for suggestion. In sleep, the more vigorous brain must yield to

the suggestions of the otherwise feebler, but now waking and therefore more vigorously associated one; but if the mind A (a brain) has been vigorously influenced in this way by another, B, the memory, which produces the conviction that the soul, B, has the power of acting upon the soul, A, produces the disposition to a subsequent constantly increasing influence by B. In reality, however, it is the activity of the mind, A, which carries out the powerful suggestive actions. It is only guided, dissociated, associated, inhibited or stimulated to the most vigorous activity by the mind B. The taming of lions and elephants depends upon similar processes.

B employs only the dynamisms present in A which act in the manner peculiar to the mind, A, and which follows the suggestions of B only because they are no longer capable of a conscious concentration against B, and are unconscious of their own strength. The dynamisms of A are therefore constantly overcome, more and more, by B's suggestions and follow them more and more automatically.

We find entirely similar phenomena in the influence of men upon one another in political and social life. We find them in the leaders among children, as among animals; in certain prophets and chiefs; in whites as opposed to the negroes; in Napoleon I. and Bismarck as against Europe; in the victor against the vanquished in general, both in men as well as in animals. Indeed, similar nervous phenomena can be observed even in insects (ants) (Forel, "Fourmis de la Suisse," 1874, p. 314), when after a bold attack by a few small ants innumerable larger and stronger ones run away without resistance and courage, forsaking in a cowardly manner their otherwise most carefully nurtured larvæ and pupæ. This is also a striking suggestive action. Too much importance, however, must not be attached to these analogies, plausible as they are. They are merely analogous processes.

As we have seen, suggestion is practically important in medical therapeutics. It acts most successfully in little matters, headache, loss of appetite, etc., and in all sorts of habits. Habits themselves are auto-suggestions. Here we see the intimate psychological relationship of suggestive action with the automatism of dynamic associations, with the formation of habits, with automatic unconscious cerebral activity in general.

This brings us to the pedagogic importance of suggestion, which has been discussed considerably in recent times. Whoever fails to understand suggestion is terrified by this thought. But the one who understands it thorougly will desire it to be employed pedagogically in two ways: in the first, symptomatically, as it were, medically, in order to combat bad habits, perverse peculiarities of character. Here it must be employed as in therapeutic hypnosis, and, as in the latter, we must attempt, not to utilize it ad infinitum, but only so long as necessary. We must endeavor in every way to convert the result into a permanent one, which aids itself by properly conducted auto-suggestions.

In the second place, suggestion, considered from another standpoint, becomes one of the most interesting problems of future pedagogics, of the developmental history of psychology. Every one knows that there are teachers, parents, instructors, who make of the children whatever they wish, while others attain the opposite and experience only insubordination and resistance. This depends solely upon the fact that children are subject to the unconscious suggestive action of the former, but not of the latter. Repeated unwise reproaches, scoldings concerning their non-respected (for example, paternal) authority, the exhibition of temper, in short, the exposure of weaknesses, are, as is well known, those factors which produce in children insubordination, resistance, and thus, obstruction to education. But, on the other hand, whoever understands how to advocate his counsels as a matter of course, as unavoidable, as beyond discussion, does nothing less than to suggest instinctively. The exaggeration of these methods, especially their continuance to a later period than childhood, offers the danger of developing blind belief in authority and lack of independence. At the proper time and place, the spirit of rational discussion must be invoked. But if we have once understood that the key to these actions and reactions lies in the proper application of suggestion in children, pedagogics will learn consciously to employ what has been used unconsciously hitherto, and will thus derive enormous advantage.

Nature of Suggestive Action.—Concerning the nature of suggestive action, we can only express surmises. From the fact that we attempt to give to the suggested conception the subjective character of an occurring fact, that is, of the per-

ception or action, or the inhibition of both, and that this indeed succeeds; furthermore from the fact that a series of these results affect palpable or ponderable exercises of force (muscular activity, menstruation), the assumption seems to be justified that suggestion releases in the central nervous system, by means of the conceptions, dynamisms which qualitatively and quantitatively are more or less co-ordinate with those of the suggested nervous activity when based upon a real cause, or actually occurring; while, at the same time, they differ from the conception proper, being much stronger and qualitatively different.

Illustrations.—I suggest to the hypnotized individual that he sees a cat. He sees it. The complex of activity of his brain thereby produced is very similar to, if not identical with that produced in the brain by the reception of the rays of light from an actual cat, while it is entirely different from the simple conception of a cat. An entirely similar action holds good with the catalepsy, actions or menstruation produced by suggestion as compared with the mere conception of these conditions. It is the relation of the so-called profounder condition of consciousness as opposed to the correspondingly weaker ones (vide, for example, Herbert Spencer's Psychology).

It is extremely probable that this unconscious conversion of a conception into a perception, of the image of an action into the corresponding action, of the conception of a vasomotor effect into this effect itself, of the conception of anæsthesia into the anæsthesia itself, is only rendered possible by the abolition, *i.e.*, the inhibition or dissociation of the organically associated contra-conceptions which maintain the equilibrium. By the abolition of the associated dynamic complex of forces, which effect the internal, in part, unconscious logical discussion, the suggested conception becomes all powerful and gains the highest development of nerve force, which is propagated into the peripheral nervous system. Similar peripheral irradiations also occur evidently in a simple conception, as physiologists (Stricker) have already shown, but they are too feeble to produce material results. We may here use the term reflexes, but this is merely another word.

Free Will.—After all that has been said, it need hardly be mentioned that with the assumption of free will, the suggest-

ive effects must remain inexplicable, because they constitute its negation, and that our apparently spontaneous activity of thought and will must depend upon auto-suggestion, that is, upon the actions and reactions of our cerebral forces (nerve forces in general), the crests of whose waves we are alone conscious of. We will, however, remind the reader that for centuries the best philosophers have denied the existence of an essentially free will.

Suggestion and Mental Disturbances.—Lunatics are the least suggestible of all people. Severe cases of insanity are not at all suggestible. Herein all experienced hypnotizers are agreed. This is owing simply to the fact that the morbid inhibitions or conditions of irritation in the brains of lunatics attain such an intensity that they can no longer be dissociated by suggestion. Even if we succeed in hypnotizing a lunatic, the majority of curative suggestions act not at all or only temporarily, and least of all, those which are directed against delusions. A lunatic, Mrs. X., considered herself to be Mrs. Y. I was able to hypnotize her, and succeeded in suggesting sleep, appetite, and even post-hypnotic hallucinations, but when I explained to her during hypnosis, with the greatest energy, that she now is perfectly well aware that she is Mrs. X., and not Mrs. Y., that the latter opinion had been a nonsensical delusion, concerning which she now could only laugh, she shook her head in the negative constantly during the hypnotic sleep (that is, so long as I made this statement) in order to show me that she did not accept the suggestion.

In suggestion we work with the cerebrum of the hypnotized as an instrument. In lunatics the function of this instrument is disturbed, therefore it works poorly. The failures in insanity are one of the best proofs that the power of hypnosis resides in the brain of the hypnotized, not in that of the hypnotizer.

This sketch of hypnotism cannot be concluded better than by the expression of Bernheim: "Suggestion is everything." The sleep itself is merely the effect of suggestion, although it forms one of the most powerful aids on account of the dissociated condition of the mind inherent in it and which notably increases suggestibility.

CHAPTER III.

CONSCIOUSNESS AND SUGGESTION.

THE old problem which constantly meets us in psychology is that of our consciousness. Clear and simple as the word consciousness sounds, the fact or corresponding notion is equally conplicated. Indeed, this notion is undefinable, because it coincides at every moment with that of our subjectivity, our conscious ego. Consciousness is the internal subjective mirror which illuminates a part of our cerebral activity and thus stamps that part as ours, as our cognizing mental ego.

In a lecture on memory, I expressed myself, therefore, incorrectly when I said that consciousness is an activity. We may apply the word consciousness only to this internal illumination of a part of the cerebral activity, not to the activity itself. It is, nevertheless, true that we cannot conceive of consciousness without activity.

In order to understand the relation of our mind to our brain, it is, in my opinion, of prime importance, that we limit the conception of the word consciousness in the above-mentioned manner. For example, if we confound it with the notion of the Ego, of our personality, we mix a large number of not always conscious cerebral activities with the purely subjective notion of consciousness, and thus do not escape from the confusion. Every one has, more or less, the subjective feeling that his entire feeling, will, cognition, and knowledge occur with consciousness, because at every moment of life he can only account to himself for that part which, as we say, "he can call into consciousness," that is, which he can illuminate by consciousness.

This feeling, however, is the source of most marked subjective deceptions whose climax is found in the dictum of the old pure spiritualists (in the philosophical sense) so drastically

satirized by Molière in his "Le Mariage Forcé." "The whole world appears to me only as a condition of my consciousness. Outside of my consciousness I know nothing. Hence the world does not exist; only my consciousness exists."

Sganarelle answers the philosopher Marfurius, who expressed similar ideas, by saying: "I only appear to be beating you."

Induction, that is, experience, constantly shows us that there is really an external world, although our perceptions often deceive us, often do not bring actualities to our knowledge; on the other hand, often simulate what is not real, and, in general, only furnish us with a relative and symbolic knowledge of the world, that is, with a knowledge only of those forces of the external world, which, when transformed, can be propagated through our senses into the brain and there, remodelled, appear in the mirror of our consciousness.

If we hold fast to the monistic idea that there is only one cerebral activity which forms but a part of the nervous activity whose greater point of concentration constitutes attention, and which produces only in résumé (not in its individual dynamisms) the internal illumination that we call consciousness, and usually only at these points of concentration, then hypnotism loses in great part its problematic character. Even in our normal waking condition we can observe this introduction and removal of the mirror of consciousness and particularly of the conscious illumination of our memory images. Often it is impossible to become conscious of a known name, and perhaps so much the less the more we seek it. Somebody relates a tedious story; suddenly we no longer hear him (some are still conscious of a murmur; others hear nothing at all), etc. In hypnosis such introductions and removals of consciousness are produced purposely by suggestion and the conscious part of the cerebral activity is constantly astonished by the results of the suggestions which are produced in an unconscious manner. The suggestion given is usually heard and understood consciously, and its actual success also is usually perceived consciously, but the mechanical connection between the two, between cause and effect, remains unconscious, and it is this separation of such unconscious dynamic associations, attended with the coincident development of others, which acts so astonishingly. When, by suggestion, I make an

arm motionless, this is only the inhibition of an old automatically associated complex of motor images. When, by suggestion, I allow water to be drunk as chocolate, I have simply, without the adequate sense stimulus, called forth to an hallucinatory height the old conception of the taste of chocolate which rested unconsciously in the brain and associated it with the perception of the cup and the fluid. When, in some individuals I can easily introduce and remove the mirror of consciousness and can inhibit or guide the attention, I will soon be able to do so for large complexes, and in this way everything is explained. We should note, at the same time, what is too often forgotten—that our human consciousness is solely a reflection of the concentrated activity of the cerebrum. After division of the cervical spinal cord, we are clearly and completely conscious only of the activity of that part of the nervous system which is situated on the cerebral side of the injury. If the spinal cord possesses consciousness (Pflueger), the nour cerebral consciousness is not conscious of this spinal consciousness. An amputated individual experiences pain in the fingers which are no longer present, when the former paths of conduction to the fingers, the nerves of the arm, are irritated at the stump. Disturbances of the cerebrum alone impair consciousness. Hence activities of other parts of the nervous system only become conscious when they are propagated to the cerebrum, and there produce an adequate stimulus. If this point of stimulus of the cerebrum is afterward, by an unusual path, brought into a condition of renewed or similar movement, it appears in the mirror of consciousness as on the first occasion, and the original cause is often erroneously attributed to it (consciously or unconsciously) by the judgment, that is, by other cerebral dynamisms. In this way the hallucinations of lunatics and all other possible deceptions of our mirror of consciousness, develop. On the other hand, cerebral activities, whether subjectively conscious or not, are propagated centrifugally into other (sub-cortical) parts of the brain, into the spinal cord and the peripheral nerves, and may there exercise considerable effects.

How the use of words, such, for example, as "seeing," "willing," is abused. Does a pigeon which has been deprived of its brain see or not? There are many degrees of vision: 1, the elementary amœba-like vision of the retinal elements.

2. The vision of the anterior corpora quadrigemina and the corpus geniculatum externum (secondary or sub-cortical optical centres), which have already received a co-ordinated transmission of the retinal image through the optic nerve. This is the vision of the pigeon deprived of its brain.

3. The vision of the so-called visual sphere of the cerebral cortex (cuneus), which exists despite the physiologist Golz, inasmuch as the system of fibres from the sub-cortical centres there terminate (Monakow). The visual sphere receives the retinal image from a second hand, if we may use this expression, and combined with much more complicated associations.

4. There is still another vision—a more mental vision—namely, the repercussion of these optical stimuli of the visual sphere in other associated cortical tracts of the cerebrum. There are even individuals who see tones in colors (Nussbaumer, Bleuler, Lehmann).

This also holds good with regard to centrifugal or volitional activity from the conscious wish through resolution and action down to reflex twitching. The study of disturbances of speech shows very clearly that there are no boundaries between somatic and psychical motor disturbances.

If we group together all these facts, the apparent contradictions and puzzles in hypnotism will no longer astonish us to such an extent. We will understand more easily that a hypnotized individual sees and does not see, believes, and yet often simulates, apparently with a certain degree of pleasure. His consciousness may believe, and yet not see or hear, for example, in a negative hallucination, while outside of the mirror of consciousness which is only excluded to a slight extent (as if covered by a veil) his entire remaining cerebral activity (his second consciousness, as it has been called) sees and hears accurately and escapes the obstruction. In another case, however, a concentrated, profound suggestive action may extend much more deeply into the unconscious cerebral activity, and, radiating into the peripheral nerves, may even react strongly upon them, as we see, for example, in the inhibition and production of menstruation, in the production of diarrhœa, and epidermic vesicles. We must interpret in a similar manner the wonderful cases of so-called double consciousness (case of Macnish; Félida of Azam). This holds good also with regard to the double consciousness possessed by every individ-

ual, namely, dream consciousness, and consciousness in the waking state.

If we bear all these facts in mind, the dispute concerning the value of negative hallucinations, recently entered into between Delbœuf and Bernheim in the "Revue de l'Hypnotisme" will be deprived of a subject.

CHAPTER IV.

HINTS FOR HYPNOTIC PRACTICE.

If we wish to hypnotize, and particularly to secure therapeutic results thereby, we must first arm ourselves with great patience, enthusiasm, persistency, an assured demeanor, and with ingenuity in devising stratagems and manipulations.

This indicates that not every physician is a suitable hypnotizer. It is true that the personal magnetic fluid, formerly regarded as necessary, is a superfluous myth, but not every one possesses the characteristics above mentioned. By far the greatest foe to success is the vis inertiæ which inheres so strongly in the majority of mankind, which constantly falls asleep unless again made to flicker as a result of the unavoidble frictions of life. Whoever hypnotizes in a routine manner, after a certain scheme, will soon have very little result when the stimulus of novelty is past. He will constantly fall more and more asleep, his patients less and less.

The second foe is mistrust, the anxiety, the dread of the laughter of others, of the simulation of the hypnotized, the doubts and fears of all kinds. This second foe, which is originally the greatest, soon disappears after some practice, and the first mentioned then acquires its full importance and must always be combated. Indeed, we can often observe that when we are in a depressed mood or tired, we effect less successes, because this weakness on the part of the physician is perceived unconsciously by the cerebral dynamism of the hypnotized.

As Bernheim advises, we should approach the individual to be hypnotized in a perfectly natural manner, explain to him that it is nothing unnatural or magical, but a simple property of the nervous system inherent in every individual and that he will easily fall asleep or slumber. We should avoid many words and explanations and place the patient or non-patient upon a comfortable chair. It is preferable that the chair have

no arms, and, at one side, should be placed close to the wall, in order that we can aid a still uncertain suggested catalepsy of the arm by leaning it against this wall.

We must enjoy, or attempt to obtain as far as possible, the confidence and favor of the one to be hypnotized.

The individual should not be mentally excited, anxious or in a condition of intense expectancy. The latter spoils the first hypnosis in very many intelligent people who imagine and expect the most wonderful phenomena. Many are afraid that they cannot be hypnotized and therefore make this auto-suggestion, which is often destroyed with very great difficulty. We must here rely upon patience and all sorts of devices. The first attempt then usually fails. We should explain to the patient that he is too excited for the moment, has interested himself too much, but that he is already influenced—that sleep is by no means necessary in order to produce success, and that later it will occur spontaneously. We speak of light slumber, etc. On one occasion (I had exhausted in vain all my manipulations in this manner) I asked the lady to call again the following day; permitted her to rise, put on her gloves and cloak, and then I arose and said to her in an apparently indifferent manner: "Please be seated for a moment," and with a few rapid and assured suggestions she was hypnotized in a few seconds.

In such cases it is in general very advisable to make several successive attempts with intermissions. We must also attempt to deflect the attention of the patient from his idea "it will not succeed in me." In many of these cases the hypnotization of another person in the presence of the one to be hypnotized is very advantageous.

The patient is placed upon the chair, looks into our eyes from a few seconds to one or two minutes, and, at the same time, we explain to him in a loud and positive but monotonous tone that everything is going excellently, that his eyes are already moist, his lids heavy, that he feels an agreeable warmth in his legs and arms; then we allow him to look at two fingers (thumb and index finger) of the left hand of the hypnotizer which are slowly lowered in order that the lids may follow. If the lids close of themselves, the game is won. If not, we say, "Close your eyes." We then lift an arm and lean it against the wall, stating that it is stiff. It is best to

explain, at the same time, that the hand of the corresponding arm is drawn hypnotically and irresistibly against the head. If this does not happen, we help a little, grow very decided and intense in the suggestions, finally suggest disappearance of thought, obedience of the nerves, rest, slumber. As soon as it is noticed that one or the other suggestion begins to act we utilize and emphasize this, and allow the patient to give us information concerning it by nodding the head. Every affirmed suggestion is at the start a considerable gain, which we must utilize for further suggestions: "Do you see? Everything is going well. You are sleeping better and better; the arm is growing stiffer; you can no longer bring it down" (the patient attempts this with some success). "On the contrary, if you wish to bring it down, it will go up toward your head. You see, I draw it constantly more and more toward the head," etc.

It is best to touch the painful part (head, abdomen, etc.) with the left hand, and to explain at the same time that the pains disappear. We ask the patient during the hypnosis concerning the result, and if possible, do not stop until the success is complete (momentarily). For this purpose we often require several different suggestions and must exercise ingenuity. In very suggestible people everything succeeds at once; in others great difficulty is experienced.

We must attempt to secure anæsthesia as rapidly as possible and also amnesia after waking. It has occurred that many curative suggestions succeed without these two results, but as a general thing, we succeed better and more rapidly with them. By means of the amnesia we prevent the patient from conveying the threads of his conscious logic from the hypnosis to the waking condition, and vice versa.

It is furthermore a serious duty of the hypnotizer to prevent the injurious results of auto-suggestions. Hysterical, but also other anxious, nervous individuals are apt during the first hypnosis to develop auto-suggestions of its injurious effects, especially when they are full of the subject from reading newspapers or the remarks of others. They are dizzy after the hypnosis, or they feel dull, or have feelings of anxiety or headache, even tremor, or twitchings which may increase to spasms. When this occurs we must be on our guard to avoid exhibiting anxiety, or the auto-suggestion will be thereby in-

creased and cultivated. On the contrary, we must declare with great positiveness and confidence that these are small matters which occur occasionally in the first hypnosis, but are relieved at once and never occur again, and in saying this we suggest the disappearance of the phenomena *in toto* by an immediate renewed hypnotization. We may allow none of them to remain and must bear in mind that everything produced by suggestion can also be relieved by suggestion if it is suggested away at a sufficiently early period and not allowed to become fixed. In such individuals, in hysterical subjects in general, hypnosis should be made brief, and used only a few times, and only therapeutic suggestions should be given.

To this procedure I attach great importance. To its ignorance or neglect, according to my firm conviction, all the unintentional injuries by hypnotism which are reported in literature, owe their origin. I have seen a case of trembling and pains in an arm produced by such unskilful hypnotization on the part of an inexperienced young man and which continued for months.

It is self-evident that we can injure by suggestion, if we so desire, as this is only the reverse of its curative action. We can suggest headache, menstrual disturbances, etc., as well as suggest them away, but if we desire to achieve only good, we must never speak to the hypnotized of the possibility of injury. On the contrary, we must always maintain firmly and unconditionally that the suggestion can only produce good. We thus prevent in the best manner injurious auto-suggestion.

By the same method of contra-suggestion, we must prevent the falling into self-hypnosis, the weakening of the will power, and other things of this kind, whose danger is constantly maintained against therapeutic hypnotism. On a single occasion one of my 250 hypnotized persons fell spontaneously into a hypnotic sleep, but received such a vigorous suggestive lecture that the case was never repeated. If we recognize the justification of the occurrence of such phenomena, they will soon be repeated not alone in the same individual (as, for example, in the hypnotized, hysterical patients of Von Krafft-Ebing) but also in others. On the other hand, self-hypnosis suggested by an amulet is free from danger, but we must restrict its duration by suggestion to a few minutes, and permit its occurrence only by the amulet in question.

In addition, we must always suggest complete euphoria, cheerful mood, good sleep, and appetite, and strengthening of the will. Furthermore, the rules of Bernheim and Liébault should always be carried out:

1. In all hypnotizations, at least one witness should be required—as a protection for the hypnotizer as well as for the hypnotized.

2. In all very suggestible persons (somnambulists) the suggestion should be given that no one else can hypnotize them.

3. No one should be hypnotized against his previously expressed wish.

4. Suggestions should be given only for therapeutic purposes in so far as juridical, scientific or didactic purposes do not come into question.

We must always observe carefully the individual suggestibility of the patient, act accordingly, and not according to rigid routine.

If suggestive anæsthesia is to be employed for surgical purposes, the patient must first be prepared by a few hypnotizations. If he no longer feels the pricks of a pin in the palm of the hand, or contact with the cornea, he is ready for the operation. But we must avoid exciting his mind by great preparations for the operation, or we will run the risk of dissuggesting him (I have observed this in a number of cases). We must hypnotize him beforehand, declare that the operation is a mere nothing, and thus surprise him with it as far as possible. During the operation, the anæsthesia of the corresponding part of the body must be continually suggested.

If suggestion fails in any one, the method is to be interrupted for four or five sittings. It then often succeeds at a later period by another hypnotization.

No one may be hypnotized mechanically *ad infinitum.* We only lose thereby and gain nothing. We must attempt to gain as much ground as possible rapidly in a few sittings. Then we must gradually reduce the originally daily hypnotizations and then discontinue them, the result which has been obtained being declared definitive, permanent. There are obstinate individuals of less suggestibility, however, who, nevertheless, recover after a longer time and greater persistence. But everything has its limit. If the patient no longer sees

any result, he is thereby often dissociated, and we thus lose influence instead of increasing. We must constantly endeavor to obtain something new until the end is reached, and then must gradually cease.

The hypnotizers are often dissuggested by auto-suggestions as well as by the remarks of other individuals, readings which criticise hypnotism, etc.; often by the fact that the hypnotizer himself loses courage and confidence. As a general thing, however, the lost ground may be regained by energy and trouble. Things often go better after a prolonged interval.

As Bernheim correctly says, hypnotism may be employed therapeutically, not only alone, but also in combination with other measures. Many curative measures may be employed as auxiliaries of suggestion or directly as suggestion, and there is no doubt that a large number of medicines have acted solely in this way. Homœopathy offers a striking proof of this fact.

Many pains which will not yield to simple suggestion yield to colored water or bread pills. Bernheim and Wetterstrand have shown the suggestive action of metallotherapy, and, in part, of electricity.

I have repeatedly emphasized that suggestion is not a panacea which cures everything. If we expect everything from it, we will be deceived. Much can be attained, however, especially if we act with persistence, judgment, and medical knowledge, and understand to unite suggestion with other remedies. For example, if stuttering cannot be cured entirely by suggestion, it is combined with a systematic treatment (exercises in breathing, vowels and consonants). If verbal suggestion alone does not succeed in relieving a woman of sea-sickness, she may be rocked thoroughly during hypnosis while the suggestion of feeling well is given. It will then probably succeed.

I will here mention those morbid conditions which seem to me to yield best to suggestion, although the indications are by no means determined with sufficient certainty, and undoubtedly much will yet be added:

Spontaneous somnambulism.

Pains of all kinds, especially headache, neuralgia, sciatica, toothache, which does not depend upon abscess, etc.

Insomnia.

Functional paralyses and contractures.

Chlorosis (very favorable).

Menstrual disturbances, metrorrhagia, amenorrhœa, anorexia.

Constipation and diarrhœa (if the latter does not depend on catarrh).

Alcoholism and morphinism.

Rheumatism.

Surgical anæsthesia for slight operations.

Neurasthenic disturbances.

Stuttering, nervous visual disturbances.

Night fears of children.

Nausea and sea-sickness; vomiting of pregnancy.

Nocturnal enuresis (often very difficult on account of the profound normal sleep).

Nervous attacks of cough (also in emphysema).

Hysterical disturbances of all kinds, including hysteroepileptic attacks, anæsthesia, etc.

Bad habits of all kinds.

Many other affections are mentioned in literature, concerning which Liébault, Bernheim, and other works may be consulted. The above list will suffice at the beginning for every one, and later we may form our own indications.

CHAPTER V.

ILLUSTRATIONS OF CURES BY SUGGESTION.

It would exceed the limits and purpose of this little work were I to give statistics. These have already been published, and I refer particularly to Bernheim's classical work. I will merely mention in brief two illustrations:

1. A thoroughly honest servant was attacked in the summer of 1888 by profuse menstruation, which increased in the autumn to such an extent, despite all medication, that the menses occurred every two weeks and lasted a week. The originally anæmic girl thus became extremely anæmic, almost as pale as a corpse. She had lost her appetite and sleep, dozed at night with heavy dreams. The gentleman in whose service she was, described her condition, and believed that she would be compelled to return to the country to her family. I asked him to bring the girl for consultation. This occurred on the evening of the fourth day of menstruation. I allowed her to sit upon the chair and look at me and hardly had she seen my finger when the lids closed. I now suggested catalepsy, anæsthesia, etc., with immediate success. This gave me the confidence to suggest the immediate cessation of menstruation. This suggestion, attended by touching the abdomen, and the explanation that the blood would pass from the abdomen into the legs and arms succeeded in a few minutes. At the close I suggested good sleep and appetite. I directed that she allow the menstruation to be carefully controlled by the mistress of the house. The menstruation ceased completely and the girl slept quite well on the following night. I again hypnotized her a few times and prescribed the next menstruation for four weeks later, slight and only lasting two and a half days. At the end of three or four days, I had secured by suggestion sound sleep, and, after a week, a fair appetite, also regular daily evacuations of the bowels, in the morning

after waking (previously the patient had been obstinately constipated). Thenceforward the girl improved daily. The next menstruation occurred in twenty-seven days (one day too early) at the suggested hour, was very slight and lasted only two days. Since then the girl has menstruated regularly every four weeks; the menstruation remained very moderate, and lasted at the most three days. After a few weeks color returned to the face, and since then the girl has worked regularly without disturbance, although she remained somewhat feeble and anæmic. She has not been hypnotized since, with the exception of one occasion, when she was again somewhat prostrated and had lost appetite (April, 1889).

2. An old alcoholic, æt. 70 years, who, ten years before, had twice cut his throat, during delirium tremens, was treated from 1879 to 1887 as an incorrigible drunkard and vagrant in the Burghœlzli Lunatic Asylum. All opportunities to get drunk in secret were utilized. During drunkenness he had hallucinations, and became dangerous to himself and others. At the same time he was the greatest foe to my attempts at securing abstinence in the alcoholics of the institution, and although otherwise good-humored, he stirred up the others against the temperance society. In later years he suffered considerably from rheumatism in the back, which interfered with him greatly in working. The slightest freedom could not be granted him without immediate resort to drink. I had long given him up, but in 1887 attempted to hypnotize him. He proved to be very suggestible, and in a few sittings I succeeded in making him very serious. His intrigues ceased like magic, and after a while he asked that the wines be discontinued, which I had still permitted him in small quantities because I had regarded him as lost. Soon afterward, the rheumatism disappeared entirely by suggestion (and never returned until the beginning of March, 1889). The patient improved and soon became one of the most enthusiastic abstinents of the institution. For a long time I hesitated to give him a free pass, but finally did this in the summer of 1888. This freedom, during which he always received some pocket money, was never abused. He remained absolutely abstinent, entered the temperance society at suggestion, and since then has been a very enthusiastic member. On his trips to the city he drank only water, or coffee and the like. On account of his

total inability to resist alcohol it would have been impossible for him to have taken a single glass without escaping notice. Recently he took cold and suffered a severe relapse of his rheumatism. In three hypnotizations (within twenty-four hours) this was entirely relieved, and although seventy-two years old, he has since worked more steadily than ever. In the course of the last nine months he was only hypnotized a few times for purposes of demonstration. He no longer requires anti-alcoholic suggestions.

These two cases, it is true, are among the best results, but similar ones are obtained in the majority of patients who are brought into a tolerable degree of somnambulism, and even in many cases of simple hypotaxia we can obtain very many improvements and also recoveries. Unfortunately, the Director of a Lunatic Asylum has the most unfavorable field conceivable for hypnotic therapeutics.

CHAPTER VI.

JUDICIAL SIGNIFICANCE OF SUGGESTION.

Two articles on hypnotism have appeared in the "Zeitschrift." Von Lilienthal, in "Der Hypnotismus and das Strafrecht," first gave an admirable table of the phenomena in the domain of hypnotism which are of interest to criminal law. As is well known, these are found particularly in French researches. This article was written from the standpoint of the jurist, and throws light on the question in the clearest manner. Von Lilienthal reaches the conclusion that our present criminal law furnishes sufficient protection against the dangers of hypnotism to the rights of society. As a jurist Von Lilienthal has not, like ourselves, entered into a criticism of the different views of physicians.

Rieger's article, "Einige irrenärztliche Bemerkungen u. die strafrechtliche Bedeutung d. sogenannten Hypnotismus," is the real inciting cause for the following lines, inasmuch as I oppose his views in many particulars. Rieger assumes that hypnotism is a morbid condition; that only nervous, hysterical individuals (excluding psychical epidemics) are hypnotizable. He believes that France possesses hypnotism as it does hydrophobia, and that both are due to their imperfect police measures, calls hypnotism an artificial insanity, and formulates the following dilemma: "Either the entire matter merits no consideration in criminal law, because, although long known, it has no significance criminally, or it has only recently become significant because a new important criminal factor has developed because of the passage of our criminal statutes."

In our opinion, such complicated questions cannot be settled by a simple "either-or." Rieger considers hypnotism as insignificant, thinks that, like the Salvation Army, it could be forbidden by the police. His article concludes in polemics against the method in which hypnotism is practised in France. But it is evident that the writer did not separate the admira-

ble investigations of the Nancy school from the public sensational performances of the laity and the comments of theatrical clowns. (Without further investigation of the subject, Rieger regards the results of the Nancy school as a psychical epidemic like that of the dancers and other possessed individuals.) His article is not free from a national, political spirit, which, in our opinion, does not belong to the treatment of a scientific question. In the following remarks, I will avoid as far as possible trespassing upon the judicial domain, and will refer only to those facts which appear to me, in my experience as well as in that of others, to possess criminal importance. I refer the reader to the recently published work of Liégeois.

This work contains everything that we will here mention in brief, although we agree with Von Lilienthal that the subject is in reality not so dangerous as Liégeois thinks. On the other hand, we agree with Liégeois in his opposition to Delbœuf, who fails to recognize the gravity and criminal significance of suggestion. In the first place, we will mention the interesting fact that the hitherto observed and well-known peculiarity of certain individuals of being influenced very easily, as if instinctively and unconsciously, and without hypnotic procedure, is based upon suggestion. In some individuals this peculiarity is developed in a marked degree, in men as well as in women. They simply are unable to resist the influence of those with whom they are associated, are therefore the football of others and are generally subject to abuse. They are often called "infirm of will," yet they may be very intelligent, and are by no means always weak against their own passions. They may even exhibit great energy and persistence, but are incapable of resisting the suggestions of certain other individuals. The grossest facts do not bring them to reason, nor are they able to free themselves of the influence of him who has once mastered them, and who is not necessarily their intellectual superior.

On the other hand, we find individuals who understand how to bring others irresistibly under their control. These are the great natural hypnotizers, who, without conscience often abuse their gifts. Napoleon I. was an historical type of this kind. It is often believed that this is due to success alone, but this view is incorrect. In society we can often observe such individuals, who meet with many failures because

they are lacking in clear judgment, and who, nevertheless, act "magnetically" upon many others, especially upon women, and drag an entire series of them to destruction. Their victims not infrequently declare that they had simply been unable to resist the influence; that they had experienced a sense-deluding mental pressure. These facts resemble suggestion in the waking condition as much as one egg does another. To what extent this psychological relationship with a volitional non-free condition may be utilized in the future in criminal practice, is a matter for the law.

If we now pass to hypnotism in the narrower sense we must emphasize, as Lilienthal has done, that the hypnotized may be the subject of crime or may commit crime. I intentionally avoid giving extracts from literature in order to prevent repetition of Lilienthal's article. We will here concern ourselves chiefly with the bearings of suggestion.

It is clear that all conceivable crimes may be carried out easily in the hypnotized as soon as a somewhat higher degree of hypnosis is obtained. We have seen that too much importance may not be attached to the non-willingness on the part of the hypnotized, because this includes all possible gradations. A general knowledge of hypnotism on the part of the public will, however, make it better acquainted with its dangers and thus more capable of resistance. Mention has already been made of the precautionary measures recommended by Bernheim and Beaunis of procuring an authorized witness during the hypnotization and of obtaining a previous permit for the suggestions to be given. In practice, however, this second rule can be carried out with difficulty, and it is especially French writers who have sinned most sorely against it.

The hypnotized, however, carries another protection within himself. Enticing and easy as the commission of a crime may be to the hypnotized, its results are, on the other hand, equally dangerous to the criminal, for the entire structure upon which he bases his safety is a very fleeting thing that may be easily destroyed. The hypnotized sometimes wakes at the moment when we are least expecting it. We sometimes believe him to be amnestic and suddenly the memory of everything that has occurred again becomes conscious by some auto-suggestion. As a general thing, the hypnotized can again be hypnotized by another, and thus the most complete memory of the phe-

nomena during a previous hypnotic sleep may be restored to him. All the impressions which his brain received during the hypnosis have remained there Only an inhibitory pressure prevents their consciousness, and this pressure can be removed. I believe that to the instinctive feeling of this fact on the part of the hypnotizer it is chiefly owing that hitherto such few crimes have been committed upon the hypnotized.

All these protective qualities, however, are lost in certain "better somnambulists" who are subject so completely and profoundly to suggestion that they can be used for all purposes with great security. It is very difficult to say what percentage of humanity belongs to this latter category, inasmuch as this cannot be decided in many individuals who have been hypnotized only once or twice. As we have seen, an individual who for some time could hardly be hypnotized at all, suddenly, when the proper point of attack for his individual suggestibility is found, is converted into a perfect somnambulist. The proportion of 15 to 20 somnambulists among 100 individuals and of about 50 among 100 children hitherto assumed by the Nancy school may therefore be increased considerably after sufficient practice and better understanding of suggestion. (This also appears to be true from the most recent publication of Bernheim.) On the other hand, the proportion is too high for the inexpert. Moreover the percentage of good somnambulists is increased quite considerably by so-called training, *i.e.*, with the number of hypnotizations performed upon each individual, although in many cases a certain slight grade of hypnosis can never be exceeded and certain somnambulists not infrequently lose their suggestibility by auto-suggestion.

I must here oppose a view entertained by Lilienthal, who thinks that a distinction from the criminal standpoint may be made between the lethargic and somnambulistic condition, inasmuch as he considers the lethargic individual alone as unconscious in the legal sense, probably as a result of the statements of Charcot. The speaking somnambulist with his open eyes is *de facto* as incapable of resistance as the only apparently unconscious lethargic individual. Taken all in all I believe that it is much easier to abuse suggestion in an indirect manner than to commit gross crimes by its aid.

At the head stand sexual crimes, which have hitherto been

reported almost alone in literature. These were cases of the abuse of deep hypnosis, of the performance of coitus by the hypnotizer, who is sure that his victim will not awake and will remain amnestic. This is undoubtedly true of all tolerably good somnambulists; indeed, of all deeply sleeping hypnotized individuals who can be made anæsthetic and remain amnestic. If we remember that of twenty-three nurses I could plunge nineteen into deep sleep, with amnesia and anæsthesia, the danger can be easily seen (apart from the danger of subsequent discovery). As a matter of course, murder and robbery can be easily committed upon such helpless individuals.

The misuse of the post-hypnotic actions of suggestion appears to be more complicated and important. We must wait, however, until such cases have come up for criminal consideration, but I believe that it is well to obtain a clear insight of the matter at the present time.

We have seen how these phenomena vary according to the individuality. A very interesting fact is the different individual ethical or æsthetical reaction of the normal personality to an unethical or unæsthetic post-hypnotic suggestion.

If I say to a hypnotized: "After awaking you will drink water from this glass," the suggestion succeeds without delay. If I add: "You will also place this chair upon the table," many will hesitate, will look at the chair, be abashed, laugh, and finally, some will not carry out this second suggestion, because they find the matter too stupid and nonsensical. If we ask what they thought, they answer: "I had a stupid idea of placing this chair upon the table." This thought, in the form of an imperative conception, may now plague the hypnotized for a long time, for he has not carried out the suggestion; but this is by no means always true. It often passes, and everything is over. If I say to a still more suggestible hypnotized, who has placed the chair upon the table: "After awaking, you will give Mr. X., who is now present, a kiss," or, "You will pour the contents of the inkstand upon your hand," or, "You will abstract the knife which lies upon the table and belongs to me; I will not see it; it is true, it is a small theft, but that does not matter"—the matter will run a different course. A violent struggle will arise between the force of the suggestion on the one hand, and the associated æsthetic or ethical contra-conceptions of the normal individuality, *i.e.*,

of the inherited and acquired cerebral dynamisms, on the other hand. This struggle will be so much more violent the more powerfully these contra-conceptions and the suggestibility are developed. The stronger the antagonistic forces, the more violent will the struggle be. Its termination will depend upon the intensity and the endurance of each of these forces. We must take into consideration here the individual components of which each of these antagonistic power consists. They are:

1. The height of the individual suggestibility.

2. The duration of the action of a suggestion in the brain of the hypnotized.

3. The strength of the hypnotic education or training.

4. The profoundness of the sleep (which, by dissociation, enfeebles the resisting power of the normal mind and comes into question particularly in activities during the hypnosis itself).

5. The adequate, *i.e.*, the desired action of a skilful and vigorously adapted suggestion, *i.e.*, of the psychical action of the hypnotizer.

6. The normal individuality of the hypnotized, *i.e.*, the development of his ethical and æsthetical disposition, his power of will, his education, etc.

7. The momentary psychical condition of the hypnotized, etc.

Point 6 is extremely important. Whoever possesses but little conscience will, other things being equal, follow out a criminal suggestion much more readily than one in the possession of a strongly developed conscience.

Point 4 also holds good, according to our former statements, for post-hypnotic conditions, in so far as these bear more or less the character of a renewed hypnosis. The more completely awake the hypnotized is the better he can struggle against a suggestion.

We see how complicated the problem is, and we have to deal chiefly witth the question: "How far can it go?"

It has been seen that even during profound hypnotic sleep a struggle may occur between the suggestion and the individuality of the hypnotized. Not every suggestion is accepted, as Bernheim has clearly shown. Even when a criminal suggestion is accepted, it usually leaves behind the traces of a profoundly associated emotional disturbance.

To a man of 70 years whom I had placed in profound sleep, and who sat in a room in the presence of the Zurich Legal Society, I said: "Directly in front of us is a bad fellow, a scalawag; we will murder him; there is a knife (I place a piece of chalk in his hand). He is standing directly in front of you; stab him in the abdomen." Manifesting great internal excitement, trembling, with distorted features, he grasps the chalk spasmodically in his right hand, suddenly rises, and stabs violently with great force into the air twice in succession. Afterward, he remained very excited during the hypnosis; does not return the chalk, but puts it in his pocket. Several minutes elapse before I can quiet him by suggestion. When I then awake him, he is still perspiring and excited; he can no longer remember what has happened, but says that something must have gone wrong.

Bernheim, Liégeois, and other French writers mention extremely interesting cases of, in part, quietly executed criminal suggestions of (apparent) murders, of suggested actual robbery, etc. By suggestion I have also induced a man to shoot (with a revolver loaded with blank cartridges) a student. I also induced a nurse to slap vigorously the same individual, and another honest girl to steal a 50-centime coin.

The fact remains patent that a somnambulist may commit grave crimes by suggestion during hypnotic sleep, and at times may know nothing of this subsequently. The best proof that somnambulists consider the actions which they perform post-hypnotically as willed freely by themselves, resides in the manner in which they are ashamed, and often attempt to conceal them. I allowed a hypnotized who was quite feebly developed ethically, to steal a knife from the table post-hypnotically. After she had left the room, she went in a shamefaced way to my cook, told her that by mistake she had taken a knife, she did not know how, and begged of her to restore it without telling me, "because she was ashamed of herself." One of the most cunning knaveries of suggestion consists in the utilization of the suggestions à échéance together with the suggestion of amnesia and of a free effort of the will, in order to induce an individual to perform an action for a selfish purpose or to commit a crime. He then finds himself in a condition which must simulate spontaneity to him and all others, and yet he acts only at the command of another. We

can imagine how, in this way, love and marriage may be suggested to a rich heiress, how a will in favor of this one, or, on the other hand, disinheritance, may be suggested. We can understand that an individual may carry out plans of revenge, robbery, etc., by the abuse of a hypnotized by means of suggestion à échéance. It is true, that this requires individuals of feeble character with somewhat blunted feelings and easily suggestible natures, in whom the criminal originator would first suggest the necessary preliminary feelings by means of which they would soon obtain the conviction that they had acted spontaneously. This holds true concerning the suggestion of suicide.

Formerly it was often observed that the hypnotized were afraid of their hypnotizer; concealed themselves from him as if from an evil spirit. This was due to the fact that the "magnetizers" of that time did not understand the psychological sense of their own art and effected the hypnosis by means of all sorts of mystical hocus pocus. In Liébault's method hypnosis is effected by the aid of quiet, natural, friendly words. The hypnotizer no longer appears as a Mephistopheles, but as the healing physician, or, at least, as the trust-awakening man of science, who employs only natural means. Moreover, he is able by suggestion to make hypnosis desirable to the hypnotized. He can suggest to him a feeling of well-being, cheerfulness, sleep, appetite. This explains the fact that such hypnotized individuals in great part return very readily, and regard the hypnotizer as a friend; but herein resides one of the greatest criminal dangers of suggestion. Flies are caught with honey, not with vinegar. It is not only a matter of yesterday, as we have seen, that certain siren-like individuals possess the gift of converting others into the blind tools of their egoistic purposes. In future, however, much more will undoubtedly be obtained in this direction by well-directed suggestion.

On the other hand, a new knowledge always brings the antidote with it. Men are thus warned against the danger of suggestion by bad people. The judge will learn to consider the psychological significance of the entire series of facts. Finally, a very suggestible person who is suggested in a favorable manner by a trustworthy physician before witnesses can thus acquire a considerable, although not absolute protec-

tion against bad suggestions. This protection will be won by suggestions of strength of will, of struggles against bad influences, etc. But above all, we will tell the hypnotized: "I alone and no one else can hypnotize you." To an extremely suggestible, very profoundly sleeping nurse who wished to journey to her bridegroom in America I gave this suggestion because she feared to be abused upon the journey. It was easy here to note the result, because other expert persons have since vainly endeavored to hypnotize her. In order to effect this, I gave her my photograph as an amulet, with the suggestion that if she should again suffer in America from her rheumatic and other complaints, she need only regard the picture for a moment; she would then fall asleep for a quarter of an hour, and on awaking the symptoms would be lost. Even before the trip I allowed her to make a test of the fifteen minutes' hypnosis with the photograph.

Unfortunately, a criminal may employ similar measures, and suggest to the hypnotized, "I alone can make you fall asleep, and then you will no longer know that you were hypnotized." By means of experiments which Liégeois carried out in common with Bernheim and Liébault, he showed that a hypnotized to whom a criminal had shrewdly suggested amnesia and spontaneous resolution, etc., of the suggested crime, could yet be brought to reveal the evil-doer in an indirect way by the suggestion of apparent protection against the criminal, etc. However, he seems to assume that we can rehypnotize the somnambulist; that the evil-doer cannot successfully offer the suggestion that no one else can again hypnotize him.

I believe, however, with Liégeois, that the final discovery of the true criminal by persistent hypnosis of the somnambulist by an expert hypnotizer will succeed almost always, if not always.

This does not exclude, however, the possibility of crime. Criminals often commit their deeds without sufficient precautions, and hypnotism will exercise its attraction for the criminal because it offers him certainty of protection in the immediate future. In addition, we will not always think of hypnotism after a suggested, but apparently spontaneous act.

The further danger of hypnosis consists perhaps in the production of diseases. For evident reasons no experimental

proofs of this suspicion have been offered; yet this is undoubtedly possible, perhaps even easy. Hysterical attacks have been produced accidentally by poor methods of hypnotization. Even the Nancy method in the inexpert hands of a tyro may produce disagreeable accidents. If the hypnotizer does not understand how to check a morbid symptom which develops perhaps during the first hypnosis (tremor, headache, and the like) by vigorous contra-suggestion, according to my experience, this is always possible. Such accidents can almost always be made good by an expert operator. But Liébault, and later, Bernheim, have pointed out that many peculiar phenomena, certain diseases, and even cases of death, which the individual in question had prophesied for a definite period, or which had been prophesied by fortune-tellers, depended perhaps upon auto-suggestion or suggestion. By means of auto-suggestion, a man who exhibits a tendency to hypochondria, may acquire anorexia, considerable emaciation, etc. Formerly, I have seen real diseases followed by cachexia lasting months, and even years, whose development I now think may be attributed to auto-suggestion of the patient, and its further development by suggestion on the part of the equally deceived physician. In one case (doubtful ulcer of the stomach without hemorrhage) my view was confirmed by the extreme suggestibility of the patient as observed by me at a later period. If we remember that by means of suggestion we can produce or prevent a process such as menstruation (recently I have experimentally delayed menstruation for two full weeks), there can be no doubt that disease, and, indirectly, death, may be produced by suggestion in a criminal way. If it were possible to suggest paralysis of the heart, for example, or œdema of the glottis, the possibility of a direct suggestion of death would be given. As we have seen, suggestion *per se*, when cautiously employed in the proper manner, according to the Nancy method, is attended with no disadvantages, neither hysteria nor nervousness. And if it produces a disagreeable symptom, such, for example, as spontaneous somnambulism, a contra-suggestion suffices to relieve this. In the 250 individuals whom I have hitherto hypnotized I have never observed an injurious effect (apart from the auto-suggestion of headache, etc., which ordinarily occurs only after the first hypnosis and which is at once suggested away); but if suggestion is em-

ployed frivolously and excessively, if from carelessness or ignorance we fail to relieve at once the above-mentioned auto-suggestions of nervous symptoms, neuroses may develop without any intention on the part of the hypnotizer. Herein lies the main danger of hypnotization by the laity.

One of the most peculiar, and at the same time, most important, if not the most important, criminal meanings of suggestion resides in the deception of memory, the retroactive hallucination, called forth unconsciously, *i.e.*, suggested to an accused person by a criminal judge. We have already discussed this phenomenon. In attempting to obtain from a child, a woman, or a weak man, by the use of great persuasive power, a confession of a deed of which he is suspected, we can suddenly produce the suggestion that the innocent victim is the criminal. If this is the case, there follows not alone a complete confession of the deed which was not performed, but, in addition, every possible detail of the most complete character is hallucinated retroactively. These very details will best inform us that we have to deal with a suggested deception of memory, that is, when they do not coincide with what has been ascertained positively concerning the deed. An easy and very valuable control experiment, when this suspicion is entertained, consists in suggesting to the accused details concerning which we are certain that they could not possibly have occurred. If he admits all these we can be tolerably certain that the entire confession is worthless or dependent upon suggestion by the judge. In this way we can prevent the murders of justice. We have also seen that certain well-known instinctive liars are nothing more than individuals of such a susceptible character that they constantly mistake their own conceptions and those conveyed to them by others for reality. We find this peculiarity existing quite generally in many savage races, especially in the Orient, and in negroes, not to speak of the countryman of Alphonse Daudet's notorious "Tartarin de Tarascon."

Not alone false confessions, but even false witnesses can be prepared in this manner, and by the terrifying procedures to which witnesses are often subjected. In the method in which they are plagued by lawyers they will certainly often be led to make statements which depend upon suggestion. Contradictions which are held up against them are not always con-

scious lies, but not infrequently suggested actions. Children are especially dangerous in this respect, the more so the younger they are.

We must here distinguish two cases: 1st, the case in which suggestion is produced by the special action of the examining advocate in an individual who otherwise is tolerably truthful; second, the case in which the witness always confuses truth and the products of his fancy, because he can do no other.

The second case has long been known under another name, and is less important. We soon recognize the character of such witnesses by their conduct and by other testimony, or we learn it by their reputation. We regard them as habitual liars and attribute no importance to their statements. The first case, on the other hand, must be a source of much thought to the criminalist, because it may occur in really good people who in all other respects depose to the truth and have received a false memory only by suggestion.

Is a hypnotized individual to be considered unconditionally as irresponsible? This question must be considered in the concrete case as extremely difficult. It is true that, as almost all authors, including Lilienthal, believe, every individual who acts under the effect of a suggestion must be regarded as irresponsible. The hypnotizer who utilizes him is responsible for his action. But how can we carry this out in practice when we remember the frequency of unconscious suggestions, not recognized as such, which occur everywhere in the world without palpable hypnosis, and how will we define the boundary of responsibility in the fine shades of waking suggestion which we described above? *Natura non facit saltum.* Here this old truth also holds good, and stamps as wrong our artificial categories as it does in mental diseases.

From a practical standpoint, I must again warn expressly against the distinction between lethargy and somnambulism made by Von Lilienthal. These conditions are only apparently different. The somnambulist can be abused sexually with as great certainty as an apparently lethargic individual. There are merely differences in the variety of the suggestions. In my opinion both must be placed on the same plane in a forensic sense.

As the writers have shown, the great danger of suggestion consists in its utilization on the part of the hypnotized for

blackmail of every kind. This danger is so great that the presence of witnesses for the protection of the hypnotizer is still more necessary for him than for the hypnotized. I refer the reader to Von Lilienthal's work and also to the case in which an individual allows himself to be hypnotized intentionally in order that by suggestion he may receive courage for a crime. (How many screw up their courage by drink?) It is hardly necessary to add that I agree entirely with Lilienthal and Rieger that public exhibitions of hypnotized somnambulists should be strictly prohibited as gross breaches against public morals and health. Such exhibitions can be compared only with those of lunatics or physiological experiments. It seems to me that the mercenary use of hypnosis in general should be prohibited.

Finally, it seems to me that the results of careless or frivolous use of suggestion, especially its use for egoistic or criminal purposes, should not be left out of consideration by jurisprudence. Must I, in conclusion, reply to Rieger, that he is entirely wrong when he believes that hypnotism can be repressed by the police and will soon disappear, like Mesmer's frauds? I hardly believe that this is necessary. Every one will see that the discredit into which Mesmerism fell was due to the vague theories of Mesmer, its charlatanry, and its times.

The child was poured out with the water, but did not die. It now appears in its true shape, free from nonsense, mysticism and mercenary motives in the light of the newer psychology. It is now too late to hide our head in the sand, like the ostrich, or to call upon the police for help. We must to-day look at these phenomena, which are so terrifying to many people, with clear consciousness and without dread. We must subject it to the strictest scientific investigation and utilize it for the benefit of humanity, instead of its injury. We will assimilate this, like all other natural phenomena.

The discovery of the psychological significance of suggestion by Braid (imperfectly) and Liébault, is, in my opinion, so great that it may be compared with the greatest discoveries of the human mind. With the majority of them it possesses this feature in common: that the facts are not new, but have hitherto been misunderstood in their real significance. I mention, for the sake of comparison, Darwin's theory of

evolution, the discovery of antisepsis, and the utilization of electricity. To Braid and Liébault we owe the discovery of the nature of hypnotism and suggestion. To Bernheim belongs undoubtedly the merit of making this truth recognized generally by the clearness of his presentation, and by the addition of many facts. Among others who have rendered services in certain points of detail, we may mention principally Faria, Puysegur, and Durand Degros. But no one has advanced our knowledge of the nature of the phenomena to such an extent as the first-mentioned writers.

THE FORMS OF

NASAL OBSTRUCTION

IN RELATION TO

THROAT AND EAR DISEASES.

BY

GREVILLE MACDONALD, M.D. LOND.,

Physician to the Throat Hospital, Golden Square, London, W.

THE FORMS OF

NASAL OBSTRUCTION.

CHAPTER I.

In these days of multiplication of text-books, the lecturer finds it unprofitable to himself, and wasted time to his hearers, if he treat of the subjects systematically arranged therein. I do not, therefore, propose to take you over the whole field indicated by the title I have chosen for these three Lectures, seeing that you can traverse it for yourselves better than I could lead you. Indeed, I intend discussing only the commonest forms of nasal obstruction, and solely as they appear to myself. But, nevertheless, although I say the diseases of which I shall treat are of common enough occurrence, yet some at least of them have been but inadequately described, while their pathology is often totally misunderstood. And it is my hope that I shall, if not elucidate these myself, at any rate help you to investigate them for yourselves.

Briefly, the affections to which I wish to draw your attention are these:

I. Obstructions the obvious consequence of chronic rhinitis; which may be further divided into—
   - (*a*) Chronic catarrhal rhinitis with vascular tumefaction;
   - (*b*) Chronic catarrhal rhinitis with hyperplasia; this leading in its turn to—
     - (i.) Dry hyperplastic rhinitis;
     - (ii.) Dry atrophic rhinitis.

II. Obstructions arising from mucous polypi and cysts.

III. Obstructions arising from post-nasal neoplasms.

These divisions are purely artificial, seeing that no line of demarcation can be drawn between them. Nevertheless, as you will see, they are rational, and will be of assistance pathologically as well as therapeutically.

The consideration of these various forms of obstruction will involve the investigation of symptoms referred to more remote structures than the nasal mucous membrane; and we shall inevitably find ourselves discussing diseases which at present you may suppose to be apart from the domain of nasal pathology—and this, although I do not consider it necessary to do more than refer in the briefest manner to the various so-called nasal neuroses.

Before I describe seriatim the diseases I have mentioned, I propose enumerating the various symptoms, subjective and objective, of nasal obstruction, from whatever cause arising. And before these can be satisfactorily discussed, I must give you an idea of the normal appearances of the nasal fossæ, as revealed by anterior and posterior rhinoscopy. This I must trouble you with, seeing that some otherwise trustworthy text-books give but imperfect, and sometimes actually erroneous, descriptions of the normal appearances; and believing as I do that no class of speculum-observation needs so much practice and patience before it can be employed with satisfaction to ourselves and benefit to our patients.

The apparatus needed for anterior rhinoscopy is simple enough. A good light from a laryngeal reflector, a nasal speculum, and one or two probes, are all that is needed. The form of speculum is not of much consequence, provided the blades are not fenestrated; otherwise small hairs in the vestibule obstruct the view. Thudichum's instrument in two or three sizes serves all purposes, and is self-retaining.

Well, then, having dilated the nostril and illuminated the interior, what do we see? Presuming the head to be sufficiently tilted back to afford the best view, and the projecting hairs held on one side by the blades of the speculum, we see for a greater or less distance into the nasal fossa. In the lower part we observe standing out from the external wall the inferior turbinate body, a fold of mucous membrane covering the inferior spongy bone, and including a plexiform arrangement of veins to which the erectile property of the structure is due. This erectile tissue extends along the free border

of the turbinate body, being specially developed at the anterior and posterior extremities. According to the degree of turgidity we can see to a greater or less distance along the inferior meatus; but frequently the swelling is so great, although scarcely to be considered pathological, that this channel is completely blocked. After exposure to cold air, and during an attack of acute coryza, the inferior turbinate body becomes greatly swollen and darker in color than the usually bright red of its mucous membrane, a point to which I shall have to refer again in speaking of the functions of the nose in relation to pathology. Yet although I say this turgidity is scarcely to be considered pathological, at any rate anatomically it frequently comes under observation as a form of nasal obstruction and as such requires definite treatment.

If we investigate with a probe the nature of the inferior turbinate body when in a state of turgidity, we observe that with this instrument it gives the sensation of a bag loosely filled with fluid. Indeed it fluctuates; and on a first examination one can hardly be persuaded that it does not contain fluid other than the blood of its erectile structure. But the application of a twenty-per-cent solution of cocaine quickly restores it to its normal condition, and the true nature of the swollen tissue is revealed.

Besides that on the inferior spongy bone, there is another mass of erectile tissue on the septum, a little lower than opposite the anterior third of the middle turbinated bone. This is as liable as the inferior turbinate body to erection, although its smaller bulk renders it less conspicuous. But the appreciation of its real character is quite as important, seeing that not infrequently its turgidity leads to the supposition that the middle turbinate body is in contact with the septum, whereas the erectile tissue merely conceals the slit which normally exists between the convex margin of the middle turbinate and the inner wall. The middle spongy bone has also a small amount of erectile tissue along its free margin. (Bigelow, "Boston Medical and Surgical Journal," April, 1885.)

And now we must consider more particularly this middle turbinated bone and its coverings, which are so much misunderstood in their physiology and pathology, not to speak of their normal appearances; and the diseases of which are so numerous, and have given rise to so much dispute, that I trust

you will pardon me if I seem to trespass unduly on your time with minutiæ on this head.

According to the degree of prominence in the inferior turbinate body, we see more or less of the free convex border of the middle spongy bone, between which and the septum there is a clear passage, although this may be obscured by the erectile tissue on the septum which I have mentioned. This, I say, is important to bear in mind, seeing that in certain inflammatory conditions the middle turbinated tissue is found in contact with the septum, and the physiological may be mistaken for the pathological, or vice versâ. Then, between the free margin of the bone, curved inward on itself, and the outer wall of the fossa, is a deep sulcus, corresponding with the concavity of the spongy bone, so that the free margin appears to hang downward from the roof. Once more we must observe and remember that between the anterior extremity of the middle spongy bones and the nasal bones there is a free passage, which, nevertheless, is considerably narrower in the living subject than in the skeleton. This passage also in pathological conditions is occasionally obstructed.

That portion of the fossa which lies below the upper margin of inferior spongy bone is commonly spoken of as the inferior meatus; into it opens the naso-lachrymal canal. Between the concavity of the middle turbinated bone and the convexity of the inferior extends the middle meatus, into which, an inch and a half or two inches from the orifice of the nose, opens the antrum, as well as higher and more anteriorly the frontal sinus with the anterior ethmoidal cells, through the semilunar opening into the infundibulum.

In by far the majority of individuals we find more or less deflection of the septum, with the greater or less development of bony ridges running from before backward. The most frequent site of the deviation is at the junction of the vomer with the perpendicular plate of the ethmoid and the triangular septum. This malformation, which it must be considered, although the rule rather than the exception, chiefly interests us at this stage of examination because of the difficulty it causes in examining properly the side encroached upon. Deviation of the septum may be readily diagnosed from ecchondrosis of the septum by the corresponding depression in the opposite fossa.

So much for what is seen by anterior rhinoscopy.

Posterior rhinoscopy is at once more difficult and easier than anterior. As you well know, it is effected by manipulation of the laryngeal mirror directed upward behind the soft palate. The difficulty consists in placing the mirror in position; but that effected, the structures brought into view are easy enough of comprehension.

In ordinary cases all that is needed is a small laryngeal mirror, with stem strong enough for the angle to serve as a tongue-depressor. For examining the septum, turbinated bodies, and vault, the angle of the mirror should be about a right angle, while for investigating the posterior naso-pharyngeal wall, the angle should be even wider than that of the laryngeal mirror. Sometimes it may be necessary to use a tongue-depressor as well. In either case we must insinuate the mirror behind the uvula without touching either it or the posterior wall of the pharynx. The advantage of dispensing with the tongue-depressor is that, with our left hand, we can steady the patient by holding his chin. But in every case we must persuade him that we are master of the situation. A patient knows who is examining him as well as a horse knows its rider, and he permits us to get a view or not, according to our manipulative dexterity. We must be at once firm and gentle for this sometimes exceedingly difficult operation. Often we may cause relaxation of the soft palate by requesting the patient to breathe through his nose; more often, however, he only closes his mouth. But the longer we take to get a view, the more difficult does it become. Sometimes we may need the application of a ten-per-cent solution of cocaine, in conditions of great irritability; and sometimes we may be helped by one of the many forms of palate-hook, in conjunction with cocaine. Of course we should aim in this, as in all surgical manipulations, at effecting our end with the simplest means and least display of apparatus.

Well, then, having the angle of the mirror pressing on the back of the tongue, and the upper edge of the reflecting surface just below and well behind the margin of the pendulous palate, we have our view of the post-nasal structures, a view, however, pieced together by a series of altered inclinations of the mirror. We see the two choanæ separated by the vertical, in this situation very rarely deflected, septum. Into these

project almost horizontally the three, sometimes four, turbinated bodies with their intervening channels. The inferior meatus, owing to the prominence of what is called the uvula-cushion, frequently appears as no more than a narrow slit, while often the whole of the inferior turbinated body is hidden. The middle meatus is not infrequently contracted by congestion of the erectile tissue covering the bone below, while the superior meatus may appear to possess an unwarranted importance, seeing that it is only posteriorly that it is fully developed.

Externally to the choanæ we see the yellowish opening of the Eustachian tube, with the salpingo-palatine fold in front and the salpingo-pharyngeal fold behind. Externally to this again is Rosenmüller's fossa, into which the Eustachian catheter sometimes inadvertently passes. In the another part of the vault we may, or may not, distinguish the raised collection of lymphoid tissue described as Luschka's tonsil, in the centre of which a depression may sometimes be seen. Running downward from this are not infrequently seen two adjacent folds of mucous membrane, one on each side of the central raphe. These and Luschka's tonsil, otherwise called the pharyngeal bursa, have lately given rise to much speculation and divergence of opinion, of no practical and not much other value. ("Journal of Laryngology," February, 1887. Art. "The Pharyngeal Bursa.")

Now I have only a word to say concerning the normal color of the nasal and naso-pharyngeal mucous membrane—a matter not very easy to determine in a city like this, where nearly every one suffers from more or less nasal catarrh. In front the darkest portion is the inferior turbinated body, the middle being much paler. Posteriorly the structures projecting into the choanæ are usually a pale gray in color, while the mucous membrane of the naso-pharynx in general may be described as pinkish gray.

And now before considering the various affections which give rise to nasal obstruction, I will recount to you some of the more obvious symptoms, principally subjective, for which the patient seeks relief; and I shall refer to certain points in the physiology of nasal respiration explanatory of one or two objective signs which nevertheless are not dependent for their discovery upon instrumental examination.

As one would naturally suppose, the patient frequently complains of his nose being stopped up; yet he frequently denies any obstruction to his breathing, although he may actually be suffering from very grave interference with that function. In the former case the obstruction is more or less complete, while in the latter it is only partial. When complete obstruction exists, the sufferer complains of difficulty in breathing; this he experiences especially on taking any extra exertion and during sleep, when he not only snores, from the air impinging through the mouth on the lax velum and causing it to vibrate, but he may tell you he often wakes up struggling for his breath. Sometimes, indeed, the patient is supposed to be asthmatic, as indeed he may be, though the cause is nasal obstruction. Then he may complain of dizziness, loss of memory, inaptitude for mental exertion, symptoms to be referred more properly to the anæmia resulting from interference with respiration than from direct or reflex irritation as some imaginative observers maintain. Occasionally the sufferer will assert that there is something flapping in his throat or nose, and the noise may even be audible to a bystander, a phenomenon which obtains when a pedunculated polypus or flap of hypertrophied mucous membrane is the cause of obstruction. Another symptom observable to the patient's friends, and for which he is peculiarly hated by his enemies, is a perpetual sniffling varied by frequent and unsatisfactory attempts at blowing his nose. The nasal flux may be so excessive that the patient's pillow is stained with it; he may have frequent attacks of sneezing specially aggravated the first thing in the morning. Then his speech is thick and guttural from a semi-paretic condition of the palate—his voice is nasal, in common parlance—and he experiences difficulty in chewing and swallowing. In the case of polypi the symptoms are all more or less aggravated during damp weather. A patient of mine lately came one morning drenched by a passing shower. He inconsiderately attributed the blame to me, saying I had utterly ruined his aneroid on which he had learned to be so dependent, in that I had removed some enormous nasal polypi! But since he had been told two or three months previously by a physician of repute that he had incipient softening of the brain, his symptoms being headache, anxiety, impaired memory, difficulty in speech, and general

feebleness; and since all of these had been greatly relieved by restoring nasal respiration, his complaints were in the nature of gratitude rather than of aught else!

The sense of smell is of course interfered with, and this although taste may be almost or quite normal. The reason of this is obvious when we consider the course of the inspired and the expired air-currents. Even in the healthy condition, probably very little of the inspired air reaches so far back as the bulk of the superior turbinated bone, while when the upper channels of the nose are obstructed none can pass in that direction. But on the contrary, the expired air, which during mastication is laden with odoriferous particles, is driven by the thorax directly upward to the vault of the naso-pharynx and readily comes in contact with the projecting superior turbinated bone, over which is distributed the posterior division of the olfactory nerve. Indeed it would not be incorrect to style the superior spongy bone the organ of taste, the middle turbinated and plain surface of the ethmoid that of smell. Of course, we may have polypi obstructing access to the superior spongy bone; but it is not common, obstructions for the most part being confined to the region of the middle and inferior turbinated bone.

Among the far-reaching consequences of nasal obstruction we must mention the occurrence of pigeon-breast, which in its relation to enlarged tonsils was first pointed out by Dupuytren. Indeed, when we meet such grave disturbances as this simply as the consequence of nasal obstruction, we cannot fail to be impressed with the importance of the nasal functions, although it is difficult to perceive from mechanical considerations why respiration should not be even easier through the mouth. Probably enough, contraction of the bronchial tubes is a physiological regulating valve for the prevention of improperly prepared air gaining access to the pulmonary vesicles, and for enforcing nasal respiration. Indeed we never see asthmatics with open mouth, but with dilated nostrils. Breathing, they will tell you, is so much easier through the nose. I had a patient lately in the hospital suffering from laryngeal stenosis. He could breathe fairly comfortably when his nasal passages were free; but when from any cause they became obstructed he was seized with stridor. He could even produce the stridor at will by holding his nose and adopting buccal

respiration. The case suggests very strongly the importance of a healthy nose for the mechanical welfare of the larynx.

Then, again, we must accept as fact the not infrequent cure of spasmodic asthma by the removal of nasal polypi. One reads of even more serious diseases, such as epilepsy, being caused by such interference with respiration; and I imagine they can all be explained on the mechanical theory, without having recourse to the elaborate and conflicting observations and opinions of our Continental confrères. But I do not in this place intend referring to the so-called nasal neuroses, seeing that they have not fallen under my observation.

Continuing the enumeration of possible consequences of nasal obstruction, we must not forget epiphora or lachrymal abscess as the result of pressure on the nasal duct.

Whether Eustachian deafness can be considered as a direct consequence of nasal stenosis, or rather as an extension of a catarrhal process, may be open to discussion. Some authorities hold that the tube is permanently open, and that fresh air enters the tympanum by diffusion at every inspiration. This is too large a physiological question to discuss at present, and I will be content with mentioning two practical points: first, that there is no manner of doubt that post-nasal growths cause direct occlusion of the Eustachian tube; and second, that whatever the physiological facts, restoration of the nasal functions frequently leads the way to the permanent cure of Eustachian and middle-ear catarrh.

I have now mentioned the principal subjective and functional symptoms arising from any form of nasal obstruction. But with those objective symptoms of which I intend speaking now, I must be more precise as to the exact seat of the obstructions which give rise to them.

And first I will inform you of certain appearances to which sufficient attention has not hitherto been paid, and which are due to obstructions arising in connection with the middle turbinated bone. The first is a physiognomical and the second a pharyngeal sign, often, though not necessarily, associated with one another.

Whenever the middle and upper channels of the nose have been obstructed for some time (I cannot speak more definitely), whether from the presence of polypi, or from hyperplasia of the mucous membrane covering the middle spongy bones, in

which state the whole of the middle meatus may be blocked up and the passage into the superior channel obstructed, we find a peculiar expression of the face which to me is almost, if not quite, pathognomonic of the obstruction in question. Briefly it consists in this—elevation and eversion of the upper lip, the alæ of the nose being at the same time raised at their junction with the cheeks, this point being also apparently more sunken than usual; as well as a deep furrow running downward from the nostrils toward the angle of the mouth.

Now this appearance is not to be confounded with the type of face so commonly met with in strumous children, which it somewhat resembles, which moreover is frequently found in connection with the various forms of chronic rhinitis; for we find the appearance of which I speak developing in after-life, when it is clearly the result of an adventitious nasal obstruction. The thick everted lips of the strumous child, the sunken alæ, the prominent cheeks, with the depressed point of origin of the nose from the forehead, and the nostrils directed slightly forward, form a distinct and unmistakable picture.

But to what is due this expression which I have said is almost pathognomonic of obstruction in the middle meatus? To answer this question we must for a minute consider, step by step, the course normally pursued by the indrawn current of air.

The expansion of the lungs, causes a rush of air vertically downward from the naso-pharynx. This current, from the direction of the epiglottis backward and upward, must necessarily sweep over the posterior and lateral walls of the naso-pharynx, and leave a vacuum in the highest as well as the lowest regions. The vacuum is immediately filled by air passing along the various channels of the nose, which incline slightly downward as they pass backward. This movement in its turn causes the external atmosphere to pass more or less directly upward, as well as backward, through the nostrils.

Now the external orifice of the nostrils is placed at a lower level by a quarter of an inch or more than the entrance to the floor of the nose, an arrangement which obviously gives the indrawn current of air an upward direction, instead of allowing it to pass directly backward through the inferior meatus. This is a fact which may be submitted to the simplest experiment. If with an odoriferous substance beneath the nose we

inhale its particles, first with the nostrils merely distended, and secondly with the alæ raised, we find that the olfactory region is far more stimulated in the former than in the latter case; the explanation obviously being that when the orifice of the nostril is raised on to a level with the entrance to the inferior meatus, the air passes more readily backward than upward, and so avoids the olfactory mucous membrane. Indeed, in the presence, say, of bisulphide of carbon, we instinctively raise the alæ, or, as we say, turn up the nose; while on the contrary, when anxious to abstract the full quotum of sweetness from a rose, we merely dilate our nostrils, and perhaps even depress them. Or again, when we wish to express our contempt for a professional brother, we raise one ala only, by which, in a very delicate manner, we tacitly insinuate that we do not like the smell of him!

The pathological expression under discussion, indeed, we may call briefly a slight turning up of the nose. Respiration being obstructed through the middle and superior channels, an effort is made to facilitate the passage of air through the inferior meatus by bringing the external opening of the nostrils more on a level with the osseous entrance to the floor of the nose. The muscle by which this compensatory movement is effected is the levator labii superioris alæque nasi, which not only raises the ala and upper lip, but also slightly everts the latter, seeing that its fasciculi are inserted in the lip superficially to the bulk of the orbicularis oris. And it is by a persistent shortening of this muscle that the permanent expression indicative of obstruction in the middle meatus is produced. But remark, that although in this peculiar physiognomy the mouth is kept slightly open by elevation of the upper lip, yet the respiration is obviously nasal and not buccal. The latter I shall consider presently, when I have discussed the other objective sign in connection with obstruction of the middle meatus, for which, as I said, we must look in the pharynx.

Briefly, it is a dried, shrivelled appearance of the mucous membrane on the posterior wall of the pharynx. I emphasize "appearance" advisedly. For if we pass a piece of cotton wool over the surface in question, a thin film of inspissated mucus will peel off, leaving the membrane beneath in its naturally moist condition, granular, atrophied, or hypertrophied, as the case may be.

Now this so-called pharyngitis sicca is, in a large number of cases, symptomatic of swelling of the middle turbinated tissue and obstruction of the middle meatus; and it results, as appears to me, from the inspired air, instead of circulating among all the moistening interstices of the nose, passing directly backward along the inferior meatus and impinging on the posterior naso-pharyngeal wall in an abnormally dry condition. And not only this: but just as a river flows more rapidly as its channel narrows, so does the inspiratory current pass along the inferior meatus with an abnormal velocity, and striking on the first moist surface it meets with unusual momentum, it rapidly dries it, and leaves the film of dried mucus. This dry appearance, in fact, is confined to the posterior wall: we never find the pillars or palate sharing in the trouble; and when swelling of the middle turbinated tissue is the cause of it, we see that the dryness, on examining with the post-rhinal mirror, affects the mucous membrane opposite the opening of the inferior meatus, extending from that point downward.

In passing, I must remind you that pharyngitis sicca is in the text-books, if not described as an affection sui generis, then spoken of as an accompaniment of atrophic or granular pharyngitis. But when you have met with it a few times you will be convinced, seeing it even in the so-called hypertrophic conditions, that it is, in a large proportion of cases, really a symptom of nasal obstruction.

But we do not find pharyngitis sicca invariably when we have the peculiar physiognomy which I have just described. The latter may be due to the presence of polypi, which always excite a watery catarrh, in itself more than sufficient to prevent any drying of mucus; or we may find the obstruction as part of a catarrhal rhinitis, which also prevents the development of the symptom. On the other hand, the pharyngitis sicca may be but part of a rhinitis sicca, in the early stages of which we have indeed great inflammatory thickening of the middle turbinated tissue, although in the later stages the spongy bones themselves have all but disappeared. But although in the latter case we may succeed in restoring the normal secretion—a result, by the way, very seldom attained—yet the inspired air still reaches the pharyngeal wall unmoistened from the abolition of the turbinated bodies and their functions, and we still have our pharyngitis sicca. However,

in spite of all these qualifications, I advise you, whenever you see a pharynx with a tendency to dryness, to examine the condition of the middle turbinated bodies; for their swelling is often the sole cause of the dryness in the throat of which the patient complains; and by the knowledge of the pathology we may cure our patient. Nor is this all. We never find this pharyngitis sicca without more or less chronic laryngitis; and I think I may even say that we never find obstruction of the middle meatus, whether the posterior pharyngeal wall be dry or not, without a certain amount of laryngeal irritation, and that sometimes of a very serious nature. Those of you who have attended my clinic have seen several cases where the aphonia for which the patient sought relief has succumbed only by treating the nasal abnormalities. And when we remember that the nose has the definite function of moistening, warming, and filtering the inspired air, it only stands to reason that, if this function be abolished, the first structures which the inspired air meets will suffer. So that in every case of chronic laryngitis, we must examine the condition of the nasal mucous membrane.

And now for a word on the true buccal respiration and allied symptoms. They are met with chiefly in children the subjects of post-nasal neoplasms and enlarged tonsils, who are brought to us generally with the complaint of deafness. Sometimes the buccal respiration is caused by persistent tumefaction of the inferior turbinated bodies, a condition also occasionally met with in children; while less frequently polypi and deflection of the septum produce complete nasal obstruction.

In true buccal respiration the patients, chiefly from dropping of the lower jaw, but partly also from the frequently concurring deafness, have a singularly stupid appearance, and often actually lack the average intelligence of their age. This must be due in great measure to the general condition of faulty nutrition induced by imperfect preparation of the inspired oxygen for combustion to which I have already referred. For we must not forget that oxygen is as necessary to combustion as carbon; and that nasal obstruction is to the former element what œsophageal stricture would be to the latter.

But in connection with the nasal obstruction met with as the cause of buccal respiration there is another objective symptom which invariably accompanies the latter, and is

sometimes of great value in suggesting the course of investigation to be pursued even when the mouth is not habitually held open. I refer to a semi-collapsed condition of the alæ, and more especially to a dimple on each side of the median line, nearer the point of the nose than the osseous framework. It is situated, I imagine, at the angle between the superior and inferior lateral cartilages, and is the point which, from lack of adequate support, most conspicuously reveals the abolition of function on the part of the special muscles of the nose. Briefly, the nose has in cases of buccal respiration a pinched appearance, almost pathognomonic. Yet occasionally an onlooker will remark rather the apparently great breadth of the bridge of the nose. But this is obviously only relative to the narrowness of the alar portion.

While still on the subject of the physiological consequences, if I may so style them, of nasal obstruction, I may advantageously refer to the function performed by the erectile tissue on the inferior turbinated bone. For the swelling of it, as I have already remarked, is a physiological phenomenon, and as such, we must suppose, performs a definite office. The passage from a warm to a cold atmosphere is sufficient to fill the plexuses with venous blood. If, as we have seen, swelling of the middle spongy body forces the air to traverse in preference the inferior meatus, correspondingly we must assume that obstruction in the latter will cause the indrawn current of air to circulate more freely among the higher tortuosities of the cavity, the purpose served obviously being a more thorough warming of the cold air.

There is one more objective symptom of nasal obstruction to which I will refer, namely an apparently paretic condition of the soft palate, which hangs down, away from the posterior wall, and responds but feebly to tactile stimulation. It is not by any means pathognomonic of obstruction, being frequently observed as an accompaniment of post-nasal catarrh. In fact it should be regarded as a concomitant of catarrh, rather than aught else; and its occurrence is sufficiently explained by the supposition that the muscular tissue is in a state of congestion no less than the overlying palatine glands. In dry rhinitis it is seldom observed, a fact which almost substantiates the hypothesis.

I will briefly recapitulate the symptoms of nasal obstruc-

tion, and then pass on to consider the different diseases which cause it.

The patient complains of stuffiness in his nose, chronic cold, and inability to clear his head, although he use his pocket-handkerchief constantly. He snores at night, his voice is unresonant and his gutturals blunted; he has difficulty in mastication and swallowing; he is possibly anæmic and otherwise badly nourished. Olfaction is more or less interfered with. He has a peculiar physiognomy, varying from a slight elevation of the upper lip and alæ nasi to complete buccal respiration with the pinched nose. He may suffer from a dry throat, and hoarseness or aphonia. He may have headache, asthma, and other nerve phenomena, or signs of obstruction of the naso-lachrymal canal. Finally, there may be no direct symptoms at all.

The forms of obstruction met with under the comprehensive term chronic rhinitis are numerous enough, although we may class them in two divisions—(1) obstruction arising from swelling of the inferior turbinated body, and (2) that originating in swelling or hyperplasia involving the middle spongy tissue. This division, although broad and rough, has yet a pathological value which we shall appreciate as we proceed. More accuately we may subdivide chronic rhinitis in the method I have already mentioned. Let me remind you:

(*a*) Chronic catarrhal rhinitis with vascular tumefaction, which involves chiefly the inferior turbinated tissue;

(*b*) Chronic catarrhal rhinitis with hyperplasia, involving chiefly the middle turbinated tissue.

This in its turn leads to—

(i.) Dry hyperplastic rhinitis;

(ii.) Dry atrophic rhinitis.

(*a*) Chronic catarrhal rhinitis with vascular tumefaction is one of the commonest affections met with. We see it passing through various stages, from the simplest persistent hyperæmia of the mucous membrane, to that form where all the special structures and functions of the nose are completely destroyed.

We find the affection for the most part in children of a strumous habit, perhaps less frequently in adults, and even more rarely in individuals past middle life. When I say it belongs to the strumous diathesis, that is almost equivalent to

ascribing it to the persistence of an acute catarrh, seeing that by struma we signify little more than an inability to recover from acute inflammation. We have all the signs of nasal obstruction in varying degree, buccal respiration, snoring, etc., being frequent in children, who are not uncommonly deaf from implication of the Eustachian tubes. In older patients, as well as deafness, there may be hissing tinnitus from the catarrhal condition having extended to the tympanum. There is usually, especially when obstruction is most marked, no great amount of secretion, although the child may be constantly picking his nose to remove the dried plugs which collect in the vestibule. Sometimes, however, there is a thin watery discharge. Occasionally the pocket-handkerchief will reveal streaks of blood, but there is seldom real epistaxis. So far the subjective symptoms are not different from those of post-nasal neoplasms and enlarged tonsils.

On examining the anterior nares with the speculum, we frequently see, especially in children, the inferior turbinated body altogether preventing any further view. Investigating the swelling with a probe it gives the sensation, as I have already remarked, of a bag loosely filled with fluid; while if we spray it with ten-per-cent solution of cocaine, the tumefaction subsides to a greater or less degree, and we may thus make a further examination. The swelling is moist, generally homogeneous, but sometimes granular, of a pale or dark-red color. In old-standing cases we may find hard papilloma-like bodies clustered anteriorly about the free margin of the body. The latter, also in old cases, may present a grayish-pink, translucent aspect, when it may readily be mistaken for a mucous polypus; but this is a variety not often met with. The tactile sensation is generally, but not always, diminished.

Examining the posterior extremity of the same body with the rhinal mirror, we discover swelling to as great or even a greater degree; but the appearance is more varied. The color is usually lighter than that of the anterior extremity, while it is sometimes a pale gray and quite polypoid in appearance. More rarely the color is dark-red or bluish. In any case the surface may be granular or lobular to a marked degree, and in the darker variety the appearance is generally and very correctly styled mulberry-like. Occasionally these dark-red lobulated bodies are of such size that they project into the

post-nasal space, conceal the septum by their approximation, and even obscure the choanæ altogether. If we make a digital examination by passing the forefinger behind the soft palate, so unsubstantial do we find them to be that we can feel nothing but the hard posterior extremity of the bone; while if, by the help of the finger in the naso-pharynx, we endeavor to slip over them a stiff wire snare passed through the anterior nares, we make the astonishing discovery that there is nothing to bring away. Nevertheless, the operation is frequently effectual in removing the obstruction although there is nothing to show for it; and we are driven to the conclusion that the tumefaction is possibly due to a high degree of œdema, or less likely to the development of cysts. I know of no microscopical researches into its true nature, the difficulties being sufficiently obvious.

The middle turbinated bodies also take part in the general tumefaction of the mucous membrane. But the development of erectile tissue in these situations being quite insignificant as compared with that on the inferior turbinate bones, we find their swelling under simple irritation much less pronounced, although sufficient to make them abut on the septum. The venous plexuses lying on the latter are also swollen, and the mucous membrane inflamed. The floor of the nose seldom presents any noteworthy changes, although small red elevations are sometimes observed anteriorly.

The course of chronic rhinitis is slow, and varies much with the state of the barometer, being troublesome when it falls, and vice versâ. The affection has little tendency to spontaneous cure, except in so far as improved general health will expedite recovery. The probable tendency of all cases is in the direction of hyperplastic rhinitis, with, ultimately, either the abolition of function as the natural consequence of excessive stimulation—that is to say, dry rhinitis, ozæna and atrophic changes; or with the development of polypi. Of the anatomical connection of these different states I shall speak when I discuss their pathology.

The only possibility of a mistake in diagnosis is to confound the simple vascular tumefaction with polypus, a blunder almost incredible to any one accustomed to a good light, a good speculum, and a handy probe; yet the error is frequently made. It is more excusable when the anterior swelling of the

inferior turbinate body is of long standing and has the grayish appearance I have mentioned. But even here the probe will remove all doubt; for the tumefaction, though perhaps seemingly movable and pitting under pressure, is fixed and not pedunculated. It may also help us to remember that polypi are rare in children, although the persistent and extreme tumefaction of the inferior turbinated tissue is fairly common. Lastly, this erectile tissue will subside under cocaine, which of course has no influence over polypi.

The prognosis is favorable, although the treatment is sometimes difficult. If the disease is left to itself it leads to such complications as deafness, from implication of the Eustachian tubes, probably post-nasal vegetations, arrest of development in the thorax, with the formation of pigeon-breast, as in the case of enlarged tonsils, and a general condition of malnutrition.

The treatment of simple chronic rhinitis consists in the first place in keeping the nose clean with alkaline or saline douches and placing the patient under the special hygienic conditions suitable to his constitution. He must avoid cold and damp, close rooms, and dust, sedentary occupations and over-fatigue.

While I attach a great importance to cleansing the nasal cavities, I must warn you against prescribing douches and sprays indiscriminately for every case. Where the flux is profuse there is no fear of accumulation of inspissated mucus; and we must remember that most chemical substances, even pure water, destroy the action of the cilia, and ultimately these bodies themselves; while the olfactory mucous membrane, though devoid of cilia, is even more susceptible to the action of such destructive agencies. The lotio alkalina of our Pharmacopœia is always very acceptable to patients; but I have failed to see any permanent benefits accruing from its use, although the out-patient assures us that it "clears his head beautiful." So I think it a pity to apply anything to the nasal mucous membrane that will not offer some hope of altering the morbid condition.

Very weak astringents, such as half a grain of alum to the ounce, applied through an atomizer, will sometimes effect a cure; or, on the contrary, in children we may paint the anterior extremity of the swollen inferior turbinate body with a strong astringent, such as glycerin of tannin, once in the

twenty-four hours with great benefit. The nasal bougies of our Pharmacopœia are sometimes serviceable. They dissolve slowly in the nasal cavities and bathe the mucous membrane with the drugs contained in them. Those of acetate of lead (gr. $\frac{1}{2}$), morphia (gr. $\frac{1}{10}$), or of bismuth, are particularly serviceable.

Where the vascular tumefaction persists in spite of such applications, we may sometimes gain much by the daily passage along the inferior meatus of a soft rubber bougie. The patient may even be taught to apply it himself. The largest size that can be comfortably passed should be retained for five minutes or so, and then a larger one substituted; and so on until the passage is comfortably free. We must remember that the normal width of the fossa varies much in different individuals.

Finally, in the way of radical cure, we may have recourse to linear cauterization of the swollen tissues with the galvano-cautery, or to a limited use of such caustics as chromic acid or London paste. If we desire to contract the tissue filling the concavity of the inferior turbinated bone, we may pass along it a probe covered with absorbent wool, saturated with a small quantity of chromic, glacial acetic, or nitric acids. The first of these appears to cause the least pain. Superfluous acid should be immediately removed by a piece of wool wrapped round a second probe. None of these operations give rise to much inconvenience, least of all the galvano-cautery. They will probably require repetition at intervals of a week or so for a few sittings, when a cure will be effected. If the catarrh is the most urgent symptom, it may be relieved by inhalations of creasote or cubebs, weak astringent sprays or morphia buginaria. In some cases I have found chromic acid ($\frac{1}{4}$ or $\frac{1}{8}$ grain to the ounce) very serviceable as a spray.

(*b*) And now we arrive at our next division of chronic rhinitis, namely, that which I have styled chronic catarrhal rhinitis with hyperplasia. It is a condition beyond that of simple chronic rhinitis with vascular tumefaction, intermediate between it and dry rhinitis on the one hand and polypus on the other. It is swelling especially over the middle turbinated bone, in which not only the erectile tissue is involved, although to a less degree, but real hyperplasia of the mucous membrane exists with cell-proliferation and its consequences. It is a

form of chronic rhinitis, while exceedingly common, yet frequently overlooked, and may exist almost per se, without much alteration in the other structures of the nose. Its symptoms are indefinite, and sometimes wanting, and seldom referred to the nose. But as I have already informed you, the patient may suffer (as, I may say, a physiological consequence) from laryngitis sicca and chronic laryngitis in every stage. The sense of smell is not as frequently vitiated as in the case of polypi, although it is always and inevitably more or less impaired. In minor cases there is but a slight swelling of the anterior third of the free margin of the middle spongy body, but sufficient to bring it in contact with the septum. In extreme cases we see in place of the middle turbinated body—the free margin of which should appear to hang down from the vault—the whole of the middle channel obstructed by a soft, scarcely movable swelling abutting on the septum, and reaching almost down to the inferior turbinated body; and between these two extremes there is every degree of variation. The affection is generally bilateral, although it is frequently more pronounced on one side than the other. Indeed, one may see the two sides of the nose in such different stages of what we must suppose to be essentially the same affection, that while one choana is crowded with polypi, the other is in a stage of incipient dry rhinitis, the channel in the middle turbinated bone being completely occluded with the tissue to which I refer. The posterior extremity may or may not be involved in the tumefaction.

While this hyperplastic rhinitis may undoubtedly be a further condition of simple chronic catarrh, yet the etiology of the two is not always the same; and we frequently find swelling of the middle turbinated tissue without any great amount of catarrh, and without any vascular tumefaction of the inferior turbinated body. While the latter is, I believe, always due to a persistent cold, so to speak, this hyperplastic swelling of the middle turbinated tissue is due frequently to contact of irritant particles contained in the inspired air. Granted that the current of the latter takes an upward as well as backward course, the first obstacle on which it impinges is the anterior third of the middle spongy bone, especially if the inferior turbinated body is in a condition of tumefaction. In fact, this anterior third is the very portion first

attacked; and sometimes, indeed, we find it covered with the dust to which the patient is exposed. The most extreme degree of this affection I have ever seen occurs in a case I will presently show you, although the patient's condition is now very different from what it was when she first came under treatment. Her work consists in preparing furs, and it exposes her to great quantities of dust. She came complaining of aphonia. She had chronic laryngitis sicca, and great swelling of the middle turbinated body, so great, indeed, that the middle meatus was completely obstructed, as well as, necessarily, the passage into the superior meatus. The case is rather one of dry rhinitis, which conducts me to the next subdivision of chronic rhinitis. But as the consideration of this involves the discussion of certain pathological points, I will postpone it to my next lecture.

CHAPTER II.

At the close of my last lecture I referred to a case which I trust you were all able to see for yourselves, but which I regret was not as it appeared before submitting it to treatment. As I then told you, when the patient first came under observation, complaining of loss of voice and dryness in the throat, the whole of the middle meatus on both sides was blocked with a red, slightly lobulated, semi-transparent swelling. On examining it with a probe one found it to be soft and yielding, although immovable from the structure to which it was attached. But there was a further objective sign, not often found indeed, but of great importance from its diagnostic, prognostic, pathological, and therapeutic value. When I gently pushed the probe, which for such a purpose must not have a bulbous extremity, through the epithelium covering the inflammatory neoplasm and deeply into its substance, my instrument was arrested by what even a hospital dresser would unequivocally pronounce to be carious bone. We have in fact what Virchow calls an ostitis granulosa of the middle turbinated bone. In some cases the caries may have totally destroyed the thin bone in spots, so that the probe will pasright through it without encountering its substance; and in other cases the probe will as unmistakably reveal the presence of minute osteophytes, rather, however, on the free border than the under surface of the bone. In this case we must assume the existence of osteoplastic periostitis, which, as we know, may be met with in other situations in combination with superficial caries without suppuration.

I think it will not be amiss in this place, before proceeding further with my systematic description, to say a few words concerning the pathological anatomy of the various degrees of inflammation as it attacks the middle spongy bone and its coverings.

Let me remind you of certain anatomical facts concerning

the normal structures. The pituitary mucous membrane is highly vascular, especially toward the free margin and anterior and posterior extremities of the middle turbinated bone. It is covered with columnar ciliated epithelium, and contains, visible to the naked eye, the openings of numerous racemose glands. Its connective tissue, little elastic, is inseparably united with the subjacent periosteum, which scarcely forms a distinct structure. The bone itself is minutely and closely perforated with foramina for the passage of arterial twigs from one surface to the other, a fact of considerable importance, as will be presently seen.

Well, then, you will readily understand, when this intimate connection of mucous membrane with periosteum is considered, that there can hardly exist any degree of inflammation in the one without the other suffering. And when we also call to mind the exposed position of the structure under consideration, arresting as it does cold air laden with mechanical, biological, and chemical irritant particles, and not perpetually swept clean as is the conjunctiva by the eyelid, we can only suppose, à priori, that it must frequently succumb to inflammatory attacks. But however intense the inflammation of the periosteum may be, and sometimes it is sufficient to give rise to excruciating pain, it is never in ordinary attacks followed by necrosis. And why? Probably because both surfaces of this delicate bone are seldom attacked together, and the blood-vessels of the one side have as much to do with the nutrition of the other as of itself, the arterioles anastomosing through the foramina of which I have just spoken. I am speaking now of simple inflammation; in syphilitic and perhaps scrofulous inflammation both sides may be simultaneously so gravely attacked that greater or smaller portions of the bones necrose. Such pieces are either shown us by the patient, or are found lying loose in the fossæ. But as the consequence of simple persistent rhinitis, which so far as my observations go is nearly always confined to the free border and under surface, necrosis never results.

But what, then, has happened in those cases of dry atrophic rhinitis to which I shall presently refer—where there is no trace left of any middle turbinated bone at all, and which we have no reason to refer to syphilitic agency?

Simply what we should expect to happen in a case of ostitis

granulosa or caries without suppuration. The pathology, indeed, is identical with that of certain cases met with in connection with the long bones, where we find a mass of granular tissue through which a probe may be pushed until it impinges on the carious bone; and this condition may obtain without the development of any pus. I believe the granulation tissue in the case of the middle turbinated bone may absorb all the bone salts and dissolve the connective tissue without producing any breach in the epithelium covering the fungous mass, or any purulent discharge or ozæna.

The minute pathological processes of chronic hyperplastic rhinitis are simple enough. The submucous connective tissue, in consequence of prolonged irritation and arterial stagnation, becomes infiltrated with small round cells, while the fibrous elements swell and lose their toughness, or are dissolved and converted into a gelatinous intercellular substance. The blood-vessels increase in size and number, while the mucous glands also probably enlarge and even become more numerous. If the inflammatory process partially subside, the inflammatory infiltration assumes a higher form of structure, and the serous infiltration disappears. The new cells partly form fibres and partly are absorbed, and we have a condition left of hyperplasia in which new tissue, similar to the old, is superadded between the layers of the latter.

But if the inflammatory process continue, the connective tissue becomes wholly replaced by this new granulation tissue. The periosteum also breaks down under its assaults, and even the connective tissue of the Haversian canals succumbs. Then follows gradually the absorption of bone salts, bone tissue, and osteoclasts by this new growth, and we have a superficial ostitis granulosa; while if we thrust a probe through the mass of granulations, it impinges on carious bone. This conversion of the connective tissue of periosteum and Haversian canals into granulation tissue will ultimately cause the entire absorption of the delicate middle spongy bone, a condition which, as I have said, we do occasionally meet with; and we correctly name it atrophic rhinitis, without having any very clear idea as to its significance, or the importance of preventing its development when we discover the earlier stages of bone inflammation which lead to it.

I trust, gentlemen, you will not accuse me of being thoreti-

cal as distinguished from being practical. It has always appeared to me in the first place necessary to discover what a pathological process is before we can hope to apply remedial measures for its arrest. And the lack of accurate knowledge on this head has led some who profess to be authorities in nose disease to perpetrate the most ridiculous and unintelligible blunders in nasal therapeutics. To such errors I shall be compelled to refer in a more suitable place.

And, on the other hand, I trust you will not quarrel with me for reminding you of pathological processes with which you must be as familiar as, at least, I am. But I thought it advisable to recall to you certain inflammatory changes that occur in bone everywhere, and then to put it whether the same process must not perforce attack the middle turbinated bones of the nose, exposed as they are to sources of inflammation experienced by like tissue in no other part of the body, and whose very nature renders them highly susceptible to attack. The pathology of simple chronic inflammation in this situation is also of considerable importance as bearing on the development of mucous polypi, as we shall subsequently discover.

But now let us return to the clinical features of hypertrophic rhinitis. I have referred to the use of the probe, where we have a mass of inflammatory growth blocking the channel of the middle meatus; if it discover carious bone we must be on our guard lest the atrophic form appear; if the bone is not attacked we may be less anxious, although scarcely less energetic in our treatment.

(i.) Gradually, from a state of hyperplasia with excessive or only normal secretion, we pass into one where the mucous glands, partly from destruction by the inflammatory action, partly from former over-stimulation, strike work, and the mucous membrane is affected with dry rhinitis. Then the patient's trouble begins. For if there was a tendency to pharyngitis sicca and chronic laryngitis before, these become greatly intensified now, for the inspired air reaches the pharynx no whit the better for passing through the nose, which is dry and can neither give moisture nor abstract impurities. Then from the retention of the viscid secretion, which forms hard crusts hanging about the turbinate bodies and septum—a retention partly due to the scanty secretion, partly to the destruction of the cilia which should sweep it

backward, putrefaction sets in, and the resulting fœtor makes the patient's life wretched to himself and his presence hateful to his best friends. And it is solely for his "bad breath" very often that he seeks our aid. The ozæna must be distinguished from that due to syphilitic necrosis of the nasal bony structures. It is not possible to describe the difference in words, although it is real enough to those who are accustomed to experience both. The difference is quite intelligible when we remember that in dry rhinitis the smell is due to the crusts of muco-pus, which putrefy simply because they cannot be washed away; whereas in syphilitic affections it is due to putrefaction of pus contained in the interstices of necrosed bone. A point of some value in diagnosis is that thorough cleansing will remove the stench of dry rhinitis, while no amount of douching will destroy that of necrosed bone. So that we must not suppose every case of ozæna to be one of dry rhinitis; far from it. A certain amount is a very common termination to an ordinary attack of acute rhinitis, and is then, I believe, always more perceptible to the patient himself, during expiration, than to others. Similarly, we sometimes discover it in cases of chronic catarrhal rhinitis. In such cases it more readily succumbs to treatment than in cases of dry rhinitis. Then, again, in the matter of diagnosis we must not mistake the stench met with in a patient who has just been regaling himself with onions, cut Cavendish, and whiskey toddy! Indeed, this vicious and not uncommon combination of odors may readily be mistaken for ozæna, especially if there be at the same time any nose trouble.

If we examine the naso-pharynx, we find the same deficiency of secretion, and sometimes actual crusts. Tenacious muco-pus hangs about the vault and septum, while the lower part of the posterior wall is covered with a film of inspissated mucus, as I have already related.

To the possible causes of dry rhinitis I have already referred. Dry rhinitis is frequently accompanied by a most distressing condition of general health, though which is the primary trouble it may be difficult to say. At any rate, both the local trouble and the general symptoms react on one another; while if we can improve the one, the other is more or less benefited. Besides the symptoms referable to the throat trouble, such as aching, dysphonia, functional dysphagia, etc.,

the patient's digestion is variously disturbed. She complains of headache, drowsiness, and inability to sleep soundly. She is both nervous and hysterical; and often, though there is chronic laryngitis, the aphonia or dysphonia appears to be at least partly functional. Such symptoms are supposed by some to be of a reflex nature. If we inquire into her previous history, we find some more tangible cause than the nose trouble for her general condition. The cares of maternity, a series of unhappy love affairs, a hard task-mistress, or an inherited tendency to worry over petty annoyances; these, and such-like, will account for much of the nervous debility—an expression I employ for want of a more scientific—and anæmia from which the patients suffer. You will observe that I speak of the patient as she. We do not often see a man with dry rhinitis; but I believe most cases in males are the result very much of overwork.

Perhaps it is especially in strumous individuals that rhinitis reaches this stage. The tendency to it in some cases is apparently hereditary; at any rate we can say so of ozæna, without being more precise as to the pathological lesion. Nevertheless, there is good evidence to show that the affection is not contagious. Possibly, when several members of a family are attacked, there is a syphilitic element; or at any rate we may safely assume the existence of struma. But the whole question of etiology is a difficult one, and as I have no views of my own on the subject, I must refer you to the various text-books.

Although I have spoken more of the general condition of adult sufferers, dry rhinitis is said to be most frequently met with in young persons between the ages of ten and twenty; that is, roughly speaking, during the period of adolescence. But I am inclined to think it is more common in persons of riper years than is usually supposed; and I think it possible that in these it is frequently overlooked, seeing that ozæna is not so common an accompaniment, and that the patients seek relief for hoarseness or dryness in the throat. I showed you at our last meeting two such cases, one, a patient of five and thirty, and the other thirty, both of whom sought relief for their aphonia. Dry rhinitis is certainly more common in women than in men. In this hospital we notice that there are nearly twice the number of female as compared with male

sufferers. I may remark here that it is frequently noticeable in young girls who menstruate regularly that the ozæna becomes intensified at the approach of their periods. There may possibly be some connection between this fact and that which makes some people assert that they know when a woman is menstruating by the odor of her breath.

The duration of dry rhinitis can hardly be stated. It persists for many years, showing no tendency to spontaneous cure. Many cases probably pass, if untreated, into the atrophic form, when the symptoms become aggravated. Nevertheless, although ozæna, a term for the most part synonymous with dry rhinitis, is generally held to be an incurable malady, yet the fact that a large majority of cases occurs in young people, whom the disease does not kill, appears to assert that they recover. Or do they give up attending us because we do them no good? Such is not the general experience with regard to other affections.

(ii.) In the atrophic form of dry rhinitis the turbinated bones may be almost or quite destroyed, while the mucous membrane when freed from its crusts has a pale shiny appearance, its submucosa being actually in a state of cirrhosis. The atrophic process may occupy many years—how many it would be difficult to say; or, on the other hand, it may be comparatively acute, as in a case which has been proceeding under my observation, and which I showed you after my last lecture. But all cases of dry rhinitis do not necessarily become atrophic; nor does the dry stage always precede the wasting.

(Although in the text only that form of atrophic rhinitis is referred to which results from inflammatory absorption of the bone tissue, one must not forget a commoner form in which no hypertrophy precedes the atrophy. It affects chiefly the inferior turbinated tissue, and occurs for the most part in anæmic constitutions. The spongy bone actually bears so small a proportion to its erectile tissue, that the observer is too ready to imagine the bone itself is atrophied when only its coverings are affected; and it is presumably in such cases that we hear of the bone tissue being regenerated [Moure]).

No mistake can possibly be made in diagnosing dry atrophic rhinitis. The roomy fossæ enable us to see the posterior wall of the naso-pharynx, and even the Eustachian orifices.

The posterior wall of the pharynx is dry, as in obstruction of the middle meatus; for although there is room enough for the inspired air to circulate in the upper part of the nasal cavities, yet the moistening function is entirely in abeyance, and hence the desiccation of the mucous membrane. We invariably find more or less chronic laryngitis, often of the dry variety.

Great quantities of greenish or blackish plugs of dried stinking mucus collect at the upper part of the spacious fossæ and are discharged only at intervals measured by the number of days, or even weeks. There is little or no disposition to use the handkerchief. Though the atrophied membrane is so little vascular, it often bleeds readily and is fairly sensitive.

And now as to the treatment of those cases of true hyperplastic rhinitis. The tendency with many practitioners is to ignore the condition if it produce no very obvious symptoms. They forget the physiological importance of the nose, and its intimate relations with other organs. Yet the treatment is simple and satisfactory; and often a tendency to pharyngitis sicca with chronic laryngitis will not be ameliorated until the passage into the superior and middle channels is freed of obstruction. I think in nasal surgery we ought to remember Carlyle's definition of dirt—"matter out of place;" and any proliferation of tissue should be looked upon as dirt and removed.

This destruction of hypertrophied mucous membrane is most readily effected with the galvano-cautery, which, however, needs some skill and discrimination in its employment. If, for instance, it is the free border of the middle turbinate body that is enlarged, I prefer, where there is a choice, to destroy the inner, rather than the outer side. And for this reason: if we destroy the under surface of the body, mucus very readily dries and accumulates there in consequence of the destruction of the mucous glands; whereas if it is the inner side which is burnt, mucus still pours down from above and keeps the cicatrized surface moist. But in cauterizing the inner side we must be very careful to apply our burner only to the diseased spot, and avoid bringing it in contact with the septum; else we may find at the patient's next visit that the septum and middle turbinated body are inseparably united, and that we have done more harm than good.

To assist the operator in avoiding so great a blunder, I

have devised a shield which can be placed and held in any spot at will, leaving both the surgeon's hands at liberty. It consists of a small ivory plate measuring half an inch or so in diameter, and attached to a flexible probe. On each inner side of an ordinary Thudichum's speculum is fixed a spring, lying close to the blade and turned a little inward at the free border. Under this spring we can readily hook the probe after the shield has been placed on the spot we desire to protect, by bending it to the suitable angle.

When the hypertrophied mucous membrane depends in a flap, we may find a hard-wire snare the most serviceable instrument for its removal. This also does good work in destroying the polypoid, cystic degeneration of the posterior extremity of the inferior turbinate body. For this purpose we must use Mackenzie's cog-wheel instrument, which, passed along the floor of the nose, can be held and tightened with the right hand while a finger of the left, thrust behind the velum, holds the loop over the swollen tissue. As I remarked in my last lecture, such a procedure often brings away nothing, although the enlargement has completely collapsed. Hypertrophied mucous membrane is often highly vascular, especially when arising from the inferior spongy body. Bleeding consequently may be rather profuse after operating; and to obviate this we must tighten our snare very slowly. For very slow work Jarvis's instrument is preferable to Mackenzie's. But as a rule I remove with the latter every abnormal structure that can be snared; failing this I employ the galvano-cautery.

In those cases where the probe, thrust through the swollen tissue filling the concavity of the middle turbinated body, impinges on carious bone, I employ a different method. The whole of the surface which appears to be diseased I scrape with a sharp spoon down to the bone itself, and dress with iodoform insufflations. There is subsequently some pain, but not very much, and the mutilated surface granulates and heals with surprising rapidity. I will show you a case presently that I have dealt with in this manner with the happiest results; we can no longer thrust a probe on to carious bone; but although the condition of dry rhinitis is much ameliorated, a cure is by no means effected, and we must trust for further improvement to our pharmacopœia. And here I must refer to an alternative method of treatment, if only for the sake of

giving it unqualified condemnation. And in order to impress this upon you, allow me to say that I have had ample opportunity of observing its results in the hands of its chief, and, as far as I know, its only advocate. I refer to the ablation of the middle turbinated bones. The operator discovers evidence of what he calls necrosing ethmoiditis (a word on this hypothetical affection may help to clear up an erroneous impression. In a large proportion of nose-patients, those initiated into the mysteries of discovering diseased bone may be misled. For if we pass a probe gently into the concavity of the middle turbinated bone, it may impinge on what every skilled hand would indubitably pronounce to be necrosed bone. But we have no suppuration, no fetor, and the symptom remains unaltered through many years; hence the probe misleads us, and we must trust to common sense in making a diagnosis. We are driven to suppose that the mucous membrane and periosteum are so attenuated in this situation that they scarcely interfere with the impact of the probe on the bone. I may remark that an ivory exostosis of the external meatus of the ear may give a very similar impression to the probe) in almost every case of hyperplasia involving the middle turbinated tissue; accordingly, though the rationale is scarcely obvious, he slips a strong cold-wire snare over the anterior extremity of the bone in question, and removes as large a portion as he can include in the noose. For my own part I believe that there is an important function in every structure of the body, notwithstanding the fact that it can be removed easily and without the consequences being fatal; and I prefer to cure the disease in question with physiological facts kept well in view.

The galvano-cautery is, as I say, generally all that is needed for simple hypertrophy of the mucous membrane. Where, however, we have a dry, glazed condition, especially if accompanied by ozæna, energetic cleansing of the parts is the first essential to a cure. For this purpose nothing is so effective as the nasal spray, for by no other means can we reach all the interstices of the ethmoid. It is best applied with the compressed-air apparatus, and both through the anterior nares and post-nasal space. If there is much accumulation of dried mucus, it may be advisable to remove it with a syringe, sniffing the solution up the nose, or with the nasal douche. The latter, however, is for many reasons objectionable.

When the cavities are fairly cleansed in this manner, it will be advisable to use the fine pneumatic spray at a pressure of about forty pounds. Simply for the rough cleansing, it is not of much consequence what fluid we employ; preferably it should contain an alkali, and some ingredient, such as chloride of sodium, which will stimulate secretion, and so the loosening of the crusts. For the final spraying probably nothing is equal to a weak solution of perchloride of mercury, say 1 in 10,000. In the hands of my colleague, Mr. T. Mark Hovell, it has been productive of the most beneficial results. Very little appears to be absorbed, and I have never seen any constitutional effects from the treatment.

Beyond cleansing and antiseptic treatment, which I say is absolutely essential, and which should be carried out if possible by the medical practitioner himself, there are two lines of treatment. First, that of stimulating the mucous glands; and secondly, that of giving them rest by protecting them from external sources of irritation.

For the former purpose we may employ insufflations of various powders. Iodoform may do good service, being at once stimulating and a most powerful antiseptic; or we may give it as a spray dissolved in ether. But the results are not very satisfactory. More encouraging is eucalyptus--one part of the powdered red gum to two of starch, as recommended by my colleague, Dr. Morell Mackenzie. Other stimulants, such as sanguinaria canadensis or baptisia tinctoria, which latter is also reputed as an antiseptic, are sometimes used. But all stimulants to the nasal secretion appear to me as faulty in principle as are hepatic stimulants to a cirrhotic liver. For whatever purpose we use stimulants, in the long run they defeat their own end. Whether operating on nerve centres, gland structures, or other tissues, stimulants draw upon capital rather than induce honest living upon interest, the natural consequences being bankruptcy. Indeed, many cases of dry rhinitis are unequivocally due to the inhalation of stimulating particles; and at least in such cases the fallacy of further stimulation is obvious enough.

So that I consider the other alternative I mentioned as preferable—namely, that of rest to the diseased structures. To effect this we must prevent the nose from performing its normal functions. And although such a proceeding would

appear to react unfavorably upon any co-existing chronic laryngitis, yet, considering the actual state of the nose, this will hardly be noticeable.

The most efficient means of giving rest to the mucous membrane is to exclude the external atmosphere with its irritating particles. We may use either respirators, which are objectionable on many counts, or plugs of cotton wool, which shall effectually block the entrance of air to the nose.

Gottstein was the first to introduce this method of treatment. His theory of its action I must state, however, is diametrically opposed to that of rest. He looks upon the plug of cotton wool as a mechanical irritant which stimulates the secretion of mucus. If it fail to moisten the surface, he assures us it is because the plug is not large enough, and not sufficiently in contact with the mucous membrane. I believe, however, that the improvement which this treatment always effects is partly due to the rest from stimulation, but largely owing to the fact that the drying up of the mucus is prevented. If a mechanical irritant were all that is needed, why do not the plugs of inspissated mucus themselves act as such? Anyhow, the treatment gives the greatest relief to the sufferers. We may enhance the beneficial effect by using medicated wools, saturated, for instance, with iodoform, camphor, boric acid, etc., according to the directions of our Pharmacopœia. But I consider it important that we should use the ordinary as distinguished from absorbent cotton wool, so as not to deprive the palate and posterior wall of the pharynx of any moisture that might flow over them.

The easiest method of introducing the pledget is by means of Gottstein's screws; they assist the efficient introduction, and can easily be withdrawn by unscrewing them from their covering. Wearing the cotton wool even for so short time as two or three hours gives great relief; but they will often be tolerated for the whole night, when the benefit is of course more striking.

II. And now I must leave the diseases which may unquestionably be placed together under the heading of chronic rhinitis, and pass on to more debatable, but not therefore less interesting, ground. I refer to those obstructions which come under my second heading, namely mucous polypi and cysts. I specify these as mucous simply to exclude malignant neo-

plasms and fibrous polypi, though I do not intend allowing the term any narrower pathological significance.

Mucous polypi are usually divided into two classes, viz., those which from their structure more resemble adenomata, and those which may be classed as myxomata. In the former, histological examination reveals chiefly large mucous glands, which here and there may be expanded into cysts, lined with columnar epithelium and bedded in retiform connective tissue with more or less of the fibrous element; while, in the latter case, they consist chiefly of embryonic mucous tissue, pellucid jelly with nucleated corpuscles, some of which branch out to form a network, while their nuclei remain at the points from which the threads radiate. In both kinds there is a layer of ciliated columnar epithelium. Afferent and efferent vessels run through the pedicle or base, which is frequently, especially in old-standing cases, strengthened by the development of bands of white fibrous tissue dispersing in the substance of the neoplasm. There are no nerves, although Billroth claims to have detected them in some cases. But, as I shall show you presently, there is no real line of demarcation between these two kinds. We might as justly employ a more extended division according as granulation, mucous, spindle-celled, or fibrous tissue predominated. But we should only be the more persuaded that the distinction was artificial, seeing that every variety has the same cause as its primary factor.

A more practical division would be into those which contain cysts and those which do not. The cyst may attain a considerable size, but it has no limiting membrane, being merely lined with columnar epithelium. For the most part those polypi situated the furthest back are the most liable to contain cysts, and rarely a large polypus blocking the post-nasal region will collapse when grasped by the forceps or noosed in the snare. Some cases of swelling of the posterior extremity of the inferior turbinated bone may belong to this class, as I have already suggested.

The mucous membrane in the neighborhood of polypi, especially when these are of some years' duration, may be fairly or even quite healthy; an important fact which may mislead us if we attempt to investigate the etiology of these commonest of all morbid growths. Often, however, the most misleading facts are of the greatest value in ascertaining the

truth—a philosophical paradox, which I hope you will pardon me for expressing.

Mucous polypi are pinkish, bluish, or sometimes greenish, semi-transparent bodies, which readily lose their water after removal, especially when cut, and become wrinkled and shrivel. They have a singular capacity for absorbing moisture even from a damp atmosphere, and hence the patient's pretty general complaint that his nose is more obstructed when the mercury falls. If after removing a polypus we immediately weigh it, and then immerse it in water for an hour or two, we shall find it has increased its weight by half as much again or more. This statement applies also to those portions of hypertophied mucous membrane attached to the middle turbinated bone, which are so liable to degenerate into unmistakable polypi. I shall have to refer to this point again when discussing the etiology of these growths.

But to proceed with the objective characteristics of polypi.

They pit under pressure of the probe, although the depression is quickly effaced. They are more or less movable, according to the latitude of their attachment. This is sometimes a distinct fibrous pedicle, sometimes no more than a slightly constricted base, and occasionally, chiefly in recent cases, they are merely sessile. Their usual seat in those patients who seek relief is undoubtedly the free border and under surface of the middle turbinated bone, although according to Zuckerkandl's autopsies the superior meatus is the most frequent site. The growths occupy a varying space of the nasal fossæ, and may even be seen hanging into the pharynx, or protruding from the nostril. In the latter case their color may be a darker red, a shade also developed sometimes after application of the galvano-cautery, or other destructive agent. In the case of those smaller polypi which grow from the superior meatus and descend between the middle turbinated and septum, which, moreover, may easily be pushed out of sight by the probe, forcible blowing of the nose will usually bring them again within the field of vision.

When the polypi are young, and so small as to cause no subjective obstruction, we perceive on the under surface of the middle turbinated bone small reddish transparent smooth excrescences, which may readily escape notice; or if they are detected they may be considered merely as an hypertrophy of

the mucous membrane. But they are not sensitive to the probe, and, moreover, if left alone, will develop into unequivocal polypi. Nevertheless in this early stage, as far as my observations go, they are invariably associated with a chronic inflammation and hyperplasia of the mucous membrane in their immediate neighborhood.

And this brings me to the point where I must discuss the etiological pathology of the growths.

I believe that for the most part they may be regarded as œdematous inflammatory neoplasms. I have frequently watched a chronically inflamed mucous membrane slowly alter its appearance, becoming more congested and redder, then assuming a transparent œdematous aspect, gradually lose its color and rapidly develop into a typical polypus. And this in cases of both catarrhal and dry hypertrophic rhinitis, the resulting neoplasms being sitatued either on the free border or in the concavity of the middle turbinated bone. Indeed it appears to me that the variety of structure we find in mucous polypi depends largely upon the stage of the inflammatory process at which an œdematous change occurs. The œdema, moreover, is due in great measure or entirely to the fact that the inflammatory neoplasm is perpetually bathed in moisture, either from the contiguous mucous membrane, or from the hypertrophied mucous glands of its own structure. I will presently show you a section of hypertrophied mucous membrane, which after an hour's immersion in distilled water had increased its weight by one-half. To all appearance it consists of mucous tissue, with large spaces and fine interlacing trabeculæ extending in all directions. The fibres are more inclined to be wavy than in the generality of polypi, and in this respect incline to the fibrous tissue of chronic inflammation. But in other parts of the same specimen this characteristic disappears.

Then, again, if we examine a portion of mucous membrane, the nature of which, as frequently happens, we are unable from microscopical appearances to assign definitely either to hypertrophy or polypus, we frequently find unmistakable evidence of both conditions. In some parts the tissue may be infiltrated with small round cells bedded in a fine reticulum like that of lymphoid tissue; in other situations we find spindle-cells of various sizes, their extremities lengthening into

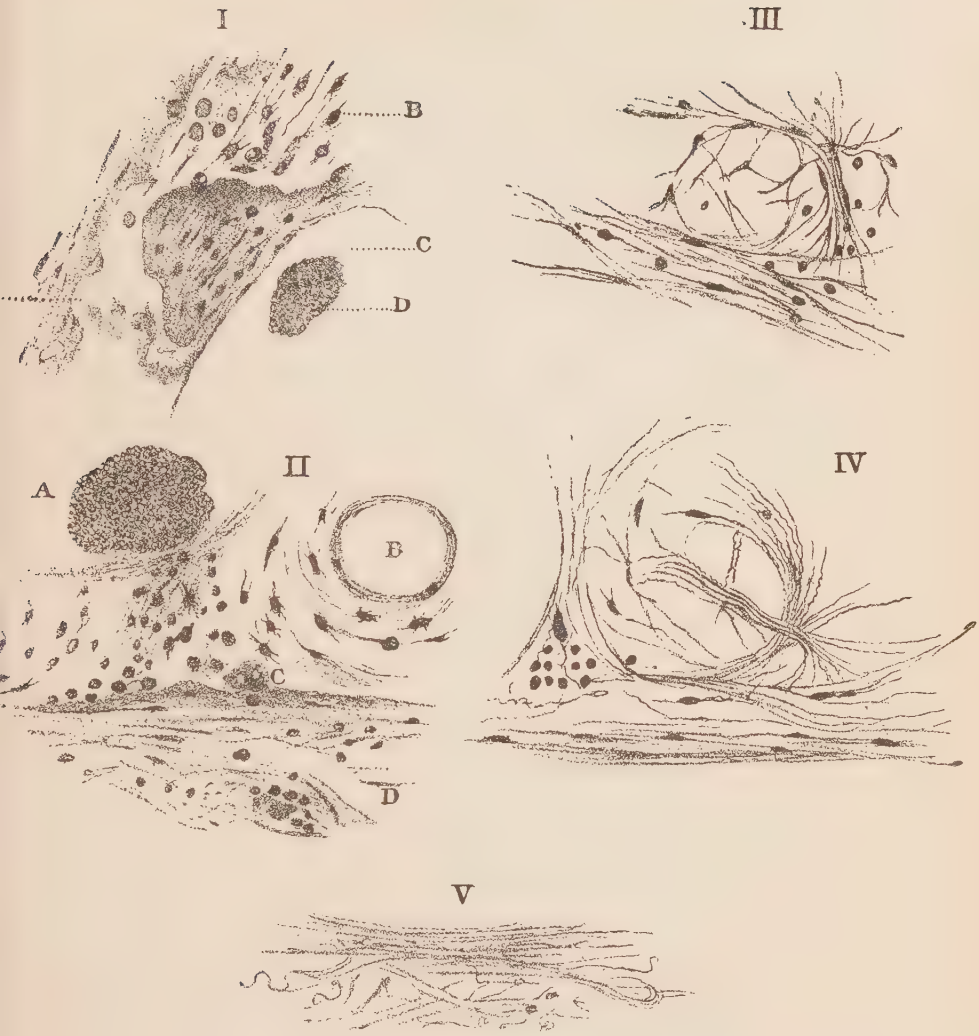


FIG. 1.—Section through hypertrophied mucous membrane, depending from middle turbinated bone. (*a*) osteophyte; (*b*) normal bone; (*c*) venous space; (*d*) blood clot.

FIG. 2.—Ditto. (*a*) blood clot in venous space; (*b*) Haversian canal; (*c*) giant cell and osteoblasts; (*d*) mucous tissue.

FIG. 3.—Ordinary mucous tissue from nasal polypus.

FIG. 4.—Œdematous fibrous tissue, obtained by immersing hypertrophied mucous membrane in water.

FIG. 5.—Œdematous connective tissue from elongated uvula.

Face page 274.

fibres, which intermingle with those of mucous spaces; while immediately beneath the epithelium we find the usual layers of hypertrophical connective tissue.

In some cases polypi may be the result of metaplasia (Virchow) occurring in the hyperplastic tissue, the products of chronic inflammation. In the neighborhood of the periosteum we may find osseous metaplasia alongside of a similar mucous change. I have, under the microscope here, a section which represents this process as it takes place in a portion of hypertrophied mucous membrane detached from the free border of the middle turbinated bone, together with a few projecting osteophytes. In another portion of the same specimen one may see the bony spicule intruding between the lobules of a racemose gland, the fibrous tissue of which has undergone interstitial increase. This would fairly establish the fact that the osseous structure was a new formation, even apart from the small number of offshoots in the bone corpuscles, the numerical profusion of the latter, and other minute characteristics.

So that it appears to me that we have conclusive evidence in favor of regarding nasal polypi as inflammatory products rather than belonging to the myxomata, or adenomata, or sarcomata. And, apart from the œdematous condition, which, as I have said, is largely due to the external conditions of the growth, the likeness between a young polypus projecting from a soft, friable mass of inflamed mucous membrane and the so-called fungous granulation is remarkable. That the latter contain a quantity of well-formed mucous tissue has been pointed out by Rindfleisch, although perhaps it is more correctly likened to lymphoid tissue. But it is an accepted fact that irritated connective and other tissues not uncommonly yield mucin, which thus becomes a product of inflammation (Virchow).

Therefore I do not mean to signify that all polypi are the result of metaplasia occurring in hypertrophied mucous membrane. When it springs from a friable inflammatory swelling which consists largely of cellular elements, its growth is not unlike that of a fungous granulation. This is what I believe occurs: a small point becomes more inflamed than the surrounding tissue, rises above its level, and in consequence perhaps of its diminished support, in consequence, that is to

say, of the dissolution of its fibrous elements, absorbs the moisture in which it is bathed, and becomes pale and œdematous. Its vessels increase in size; for, instead of breaking down on the surface as would an exposed granulation, the embryonic tissue grows and becomes feebly organized, thus increasing the demand for nutrition. The granulation elements branch out or become spindle shaped, developing around the base and the blood-vessels into fibres, exactly as happens in the process of healing by granulation. The inflammatory neoplasm advances, absorbing mucus externally, pushing the mucous membrane before it, or if not this, at any rate the ciliated epithelium, until it has become a structure wholly apart from, and independent of, its parentage. The surrounding membrane from which it sprung may, indeed, protected from external irritants by its swollen offspring, return to a healthy condition; but the polypus has its own pedicle and system of vessels, and there is nothing to stop its growth. Indeed it gains strength and independence by the conversion of its root into fibrous tissue from which bands ramify into its structure.

Polypi, as I say, tend to cicatrize at their base; but the cell-growth has, so to speak, got the start, and proceeds unchecked. Indeed microscopical examination will sometimes assist prognosis, for it is found that those polypi which consist for the most part of œdematous connective tissue are much less likely to recur than those which consist mainly of spindle-shaped elements, or mucous or granulation tissue (Billroth).

All mucous polypi contain a greater or less number of mucous glands, tubular for the most part, and greatly enlarged both in calibre and the size of their epithelial linings. The proportion they bear to the mucous tissue apparently varies according to the mode of preparing the specimens. If we harden them in alcohol the mucous spaces contract, the bulk of the growth diminishes, and the relative proportion of gland-tissue increases. So that, to form a just estimate, we must make sections without any hardening. These tubular glands can readily be accounted for on the hypothesis of hypertrophy and œdematous swelling. The lobules of the racemose glands become stretched and elongated from swelling of the tissue in which they are imbedded, and thus are converted into tubular glands. And this may also explain the apparent numerical increase.

But whatever the amount of gland structure, the connective tissue of these polypi consists of granulation, mucous, spindle-celled, or œdematous fibrous tissue; and I do not see how any distinction is to be drawn between these and the myxomatous polypus.

I shall have to refer again to the etiology of neoplasms, the result of inflammatory processes, when speaking of post-nasal vegetations. Perhaps it is not of much practical importance, except in the way of preventive surgery. But the history of nasal polypi is one of the most interesting problems in pathology, and they certainly form a connecting link between inflammation and neoplasms. Doubtless, all new growth, whether inflammatory or otherwise, is the result of irritation of pre-existing elements. And, at any rate in mucous polypi, inflammatory changes lead to the formation of tissue heterologous to that from which they primarily spring.

And now, gentlemen, after so much theory you will not be sorry if I give you some practical suggestions as to treatment. And do not imagine, because we see numbers of cases in the out-patient department of the general hospital treated more or less satisfactorily, and because every general practitioner is, to a certain extent, familiar with some methods of treatment, that the special hospital has nothing to teach you. On the contrary, while polypus-patients are frequently told they have an incurable malady, we come to regard them as among the most satisfactory and easy to cure. Yet the treatment has its difficulties, principally from the fact that the mere examination of the nose needs so much practice, more even than either the larynx or the ear.

As a rule our primary object is to destroy nasal neoplasms wherever occurring. But there is one case in which we may congratulate ourselves on their appearance, and not only leave them alone, but even encourage their growth. When in a case of hypertrophic dry rhinitis a polypus appears above the level of the mucous membrane, we may expect it to have the most beneficial effect. For, as a foreign body, it stimulates secretion, and so relieves some of the most distressing of the patient's symptoms. Yet, in spite of our care, it may become dry and shrivel up. When necessary to destroy nasal polypi, as is almost invariably the case, we aim at two objects. First, the restoration of respiration through the nose, or the removal

of more remote symptoms depending on the obstruction; and secondly, the prevention of recurrence. For the former our patient is immediately and profusely grateful, while the latter he will appreciate throughout his life, especially if he has formerly been assured, as is only too frequently the case, that his malady is incurable.

There are various antiquated methods of treatment seldom now adopted, such as injecting the growths with astringents, destroying them with chemical caustics, and so on. Of such I will not speak, since I have no experience of their application. The systems to which I shall draw your attention are those of mechanical avulsion and that of the electric cautery. Among the former the forceps and the snare are the principal, and the only ones of which I shall speak.

In discussing the use of the forceps, I shall treat separately of their application to large and small polyps. In the case of the former, their advantages and disadvantages are considerable. When the polypus is very large it is impossible to know precisely what we are including in the grasp of our instrument, and I have seen the whole of the middle turbinated bone inadvertently torn away, and that by experienced hands. On the contrary, it is in these very cases that the snare is so difficult of application. Yet so uncertain is the use of the forceps, that if we bring any bone away, we say, "so much the better," whereas if we do not we may justly pride ourselves on our skilful manipulation! Sometimes it may indubitably be desirable to remove the portion of bone from which the growth springs; but in that case we ought to see exactly what we are seizing, and not to do it hap-hazard. Then again, with large polypi the rapidity of work with the forceps may be of considerable importance. And I must tell you that some of the most practised and skilful hands prefer the forceps to any other instrument for ordinary cases. The hemorrhage, however, is always more formidable with the forceps than the snare.

For small polypi, the relations and extent of which one can easily discern, I think the forceps extremely seviceable. Wide-bladed and coarsely serrated instruments are generally the best, and the simplest mechanism is all that is necessary. Occasionally slenderer instruments like those known as sinus-forceps may be extremely useful, especially for polypi situated

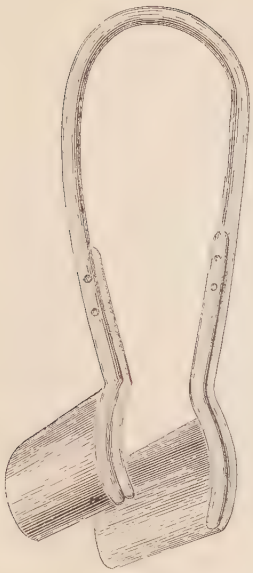


FIG. 1.

FIG. 3.

FIG. 2.

FIG. 4.

FIG. 1.—The Author's nasal speculum, with springs for holding the shields, Figs. 2 and 3.

FIG. 4.—The Author's forceps for seizing polypi in conjunction with the snare. The long diameter of the rings is transverse, in order to give the instrument a firmer grasp when held in the first and second fingers.

Face page 278.

far back or above the middle turbinated bone. Whenever possible we should, as all the text-books on surgery inform us, twist the polyp round and round until it breaks away. Sometimes, however, the only feasible plan is to tear them forcibly from their attachments.

There is great variety in these instruments, some of which I will hand you for examination. Perhaps no improvement has been so good as Mackenzie's punch-forceps. The handle is fixed at the proper nasal angle on the slender blades, which contain a longitudinal punch and groove respectively. Those which lock, such as Beverly Robinson's, save us a little trouble, and Mackenzie's axial forceps are useful for polypi situated high up.

Whenever possible, I prefer removing at any rate the great bulk of a polypus with the cold-wire snare, even if I finish it off with the forceps. If only for the small quantity of blood lost, the *écraseur* is far superior to the forceps. But it needs some dexterity, a good light, and a handy instrument; and, cæteris paribus, I consider the amount of blood lost to be in inverse ratio to the skill of the operator. There are almost as many snares as forceps; but the difference in utility is greater among the snares. The best for all practical purposes is Mackenzie's cog-wheel *écraseur*. We can wind up the wire, quickly or slowly, with the thumb, as we hold the instrument in the right hand, the left being free to alter the direction of speculum or the patient's head as required. Others, such as the many modifications of Hilton's, are available only with a soft noose, and are of no use in the denser varieties of polypus. But I find Mackenzie's instrument equally good for all kinds, while it is the only possible instrument for those cases where we must hold the noose over the polypus with our finger in the pharynx. With this instrument, too, I have removed a polypus depending from the hinder part of the superior turbinated bone, and not visible from the front, by reflecting it in the rhinoscopic mirror. Indeed, of all instruments, I consider this the most essential for nasal surgery. Jarvis's snare and its many modifications have the disadvantage of requiring two hands for their use. It was originally devised for removing hypertrophied mucous membrane from the middle turbinated bone; but even for this Mackenzie's is far superior.

Every polypus is more or less of an exceptional nature in the method of manipulation to be adopted. In the case of pedunculated polyps, our noose must be much larger necessarily than would grasp the pedicle. In tightening we must be careful not to pull upon the polyp, or we shall remove a much smaller portion than we had calculated for. Consequently, in every case, as we tighten the loop, we must advance the barrel of our instrument further into the nares. Also, as we tighten the snare, we should endeavor to lift it still higher on to the pedicle. For steadying the polypus as we do this we may grasp it, even before we throw the snare over it, with these very light forceps I have had constructed for the purpose, the forceps, of course, being thrust through the loop before seizing the tumor. In operating on growths situated very high up, or springing from the superior turbinated bone, we may find a small hook serviceable in holding the polyp in position.

Polypi which extend from the choanæ into the post-nasal space are sometimes too large to be included in any snare which can be passed through the anterior nares. In such a case the best plan is to convey a noose of soft copper-wire twist into the pharynx by means of Bellocq's canula. We then thread the ends in the barrel of a suitable instrument, such as Wilde's improved by Durham, hitch the noose over the growth with the left forefinger in the naso-pharynx, run the instrument into position in the nares, and instruct an assistant to fix the ends to the crossbar. Then we tighten the noose and bring the polypus away. Such growths, however, are frequently more fibrous than mucous. In this case, the best plan is to operate in a similar manner with a snare of platinum wire rendered incandescent, while tightening, by the electric current.

It is scarcely necessary to state that cocaine should be applied freely before any operative procedure on the nose. A ten-per-cent solution will generally suffice; but twenty per cent gives more certain results, and with actually less expenditure of the drug. A fine nasal spray producer is the most suitable method of application, as we can thrust the point behind and on all sides of the growth. Two or three minutes after applying the cocaine we may proceed to operate. In long-standing cases the mucous membrane has often become more or less anæsthetic. As a rule, no treatment is

called for after operation. Sometimes, however, after the removal of a large growth, an acute coryza supervenes in consequence of the unaccustomed exposure of the Schneiderian. Therefore in such cases, if the weather be inclement, I plug the nostril loosely with a pledget of cotton wool, which should be removed once for all when the patient reaches home.

For the prevention of recurrence there are two methods, both of which may be relied upon. The first is the galvano-cautery. If this is used so that the pedicle or base is thoroughly destroyed, our object is gained; but if used timidly and not properly localized, we may be surprised at a continual recurrence. A precautionary word in the interests of over-sensitive patients may not be amiss. There is never any occasion to let the sufferer see the incandescent point, in itself sufficiently alarming. One is frequently told of this or that operator putting red-hot wires and irons up the nose; and of this the patient naturally dreads a repetition. But, the mucous membrane well anæsthetized with cocaine, and the burner not ignited until *in situ*, the patient need not know the precise nature of the caustic we may tell him must be applied.

It is generally advisable to apply the cautery immediately after removal of the growth by the snare, else, before the patient pays a second visit, recurrence may have taken place. But we must obtain a clear view of the structure to be destroyed and all bleeding must have ceased before this is possible. Cold water is always a sufficient remedy, and we may wipe clean, with cotton wool wrapped round a probe, the region to be cauterized. One not inconsiderable advantage in using the galvano-cautery immediately is the diminished sensibility of the neighboring mucous membrane, which, however, soon regains its normal condition after removal of the obstruction.

Several operations will generally be required before the polypus is completely eradicated. And even when we think all fear of recurrence is over, we should not speak too confidently. The safest plan, both for our patient and our own reputation, is to request him to visit us again at intervals of one to six months, according to the nature of the case.

The second method of preventing recurrence is the removal of that portion of the bone from which the polypus springs. This may easily be effected if the free margin of the bone be

the site of the pedicle. Otherwise the galvano-cautery is less destructive of normal parts, and easier of application. For removing portions of bone, Mackenzie's punch-forceps, or nasal bone-forceps, may be used. Or for the anterior extremity we may employ Jarvis's or other form of *écraseur.*

When polypi are situated high up, springing from the upper surface of the middle turbinated bone, or from the superior spongy bone, it may not be possible to employ either of the methods I have advocated. But in such cases we may console ourselves by recollecting that few polypi recur more than three or four times after fairly thorough mechanical avulsion.

During the surgical treatment of nasal polypi it is preferable to suspend the use of all lotions or sprays, seeing that these but increase the tendency to rapid growth. Indeed we should aim at keeping the nasal cavities as dry as possible, and even forbid more blowing of the nose than is absolutely necessary. If much bleeding has occurred, we may keep the passages sweet with insufflations of iodoform, to which, if pain be present, a small quantity of morphia may be added.

CHAPTER III.

In discussing post-nasal growths you will find me trespassing beyond the narrower limits of the subject even more than in those other forms of nasal obstruction to which I have drawn your attention in my previous lectures. For no form, not even that of mucous polypi, is further reaching in its effects, or has more symptoms for one or other of which our aid is sought; and at the same time for no variety of nasal obstruction is the treatment more simple in its means and satisfactory in its results, notwithstanding the fact that there is much disagreement as to the most advisable methods to be adopted.

The patient generally comes under the observation of the aural surgeon, since the affection usually escapes notice until some such serious symptoms as deafness supervenes. Yet very frequently this causes no uneasiness to the child's parents; and more especially if the family medical attendant assures them, as is only too often the case, that he will grow out of it as he waxes older. The fact that children so easily contract a little Eustachian catarrh along with acute rhinitis, from which they speedily recover, is the only reason for supposing that deafness in children diminishes as they grow older; while it is perfectly certain that the subjects of post-nasal growths, although these may after many years disappear without any treatment whatever, nevertheless seldom outgrow the deafness which results from their presence, unless operative measure be adopted.

To begin with, I will relate the subjective and objective signs of the ear-trouble. The child is deaf, and occasionally (though generally it is in older subjects) he suffers from a humming, buzzing, or hissing tinnitus, seldom pulsating, unless there be some unusual complication. Often deafness is stoutly denied; but generally, on questioning the parents, they will admit that he has been very inattentive of late, and is fre-

quently punished at school for no fault of his own. Moreover, if we examine the membrana tympani on each side, we shall be sure to satisfy ourselves of a necessarily faulty conduction of auditory vibrations through the drum. The most typical appearance is a greater or less degree of depression of the drum-skin. The manubrium mallei is foreshortened and sometimes almost invisible. Posteriorly we may possibly get a view of the long process of the incus, and more rarely the processus gracilis is seen in the anterior segment. The surrounding membrane is variously affected, according to the duration of the post-nasal obstruction; it may not be thickened, and then sometimes appears more depressed than the manubrium, which stands out with an abnormal prominence. The membrane is not infrequently dull in lustre; the bright spot, which in a state of health radiates from the tip of the manubrium downward and forward, being more or less obscured, broken, or altered in situation. Beyond thickening and opacity, the drum-skin is sometimes congested, although seldom sufficiently to give rise to pain. The one distinctive feature, however, revealed by the ear-speculum is the extreme degree of depression. I say distinctive, seeing that if we find it in a child, by that alone we may be almost sure of the presence of post-nasal growths; and hence I have described the appearances somewhat minutely.

Occasionally, though not very often, the child's chief trouble is otorrhœa, and we diagnose a chronic suppurative otitis with perforation of the membrane. And even in these cases, as we shall see, the post-nasal growths are still the most important factor, if we may judge by the results of treatment.

Why Eustachian obstruction should produce depression of the drum-skin may not at first sight be quite apparent. Most probably the explanation is to be found in the fact that the oxygen in the tympanum is converted into carbonic acid; and when this latter undergoes solution in the mucus, we have one-fifth of the pressure removed, one in five being the proportion of oxygen in the ordinary atmosphere. But even if no oxidizing of carbon takes place, the greater solubility of oxygen over nitrogen would fully account for the diminished pressure. The latter, seeing that the Eustachian tubes are obstructed, necessarily causes depression of the yielding wall of the cavity, viz., the membrana tympani.

In order of the frequency with which patients complain of the different symptoms, next comes snoring at night. This is invariably present in greater or less degree, and the child may be never free from it during sleep. It depends, I need hardly add, upon the indrawn current of air impinging upon the velum, the muscular tissue of which, if not enfeebled during the waking hours, as is frequently the case in the disease I am discussing, yet is physiologically relaxed, along with other voluntary muscles during sleep. Then if we make further inquiry we find the patient is restless, tosses about a great deal, and throws off the bedclothes, occasionally waking with mild delirium. Older patients may complain of a dryness in the throat and mouth on waking, accompanied with a general feeling of malaise, which diminishes as the day advances. The remaining direct symptoms are indistinctness of speech and inability to blow the nose satisfactorily. The disturbance in articulation is characteristic, but is similar to that found in the other forms of complete nasal obstruction. We are told that the patient speaks as though he always had a cold in his head. He unavoidably substitutes B for P, one of the important differences between these labials being that P is assisted by resonance in the nasal cavities. If with the post-nasal space completely occluded we try ever so energetically to say P, we can emit nothing more than an intensified B. Correspondingly D takes the place of T, and DH of TH. Similarly, seeing that M and N depend entirely upon nasal expiration, when this is impossible, we are forced to say bay for may, and day for nay.

But besides these changes of labials and dentals dependent upon nasal obstruction, we may have other faults in articulation, due to inability of the palate to approximate itself to the posterior wall of the pharynx, either on account of the mechanical interference of the growths, or simply from paresis of the palate due to the co-existent congestion of the palatine glands and muscular tissue. The gutturals in these cases lose some of their value, and we find G substituted for K, and kick becomes gick. This weakness in the pronunciation of the gutturals is frequently found with a feebly-acting palate, where there is no obstruction, and the only other objective sign is a post-nasal catarrh, or vascular tumefaction of the inferior turbinate bodies; so that from a person's speech alone we may

often diagnose a catarrhal condition. In extreme degrees of enfeebled palate the impairment of speech closely resembles that occurring in cleft-palate patients. Thus D may approximate to N, and B to M. Even G, hard, may be almost an impossibility; while S and CH are also difficult. But beyond such palpable deficiencies, there is more that cannot be easily described. The speech is thick, difficult of comprehension, nearly all consonants being more or less blunted, and nasal. In such cases the patient really speaks through his nose. Possibly enough the so-called Yankee twang is due to an enfeebled palate; and it is by no means unlikely that this is more frequent across the Atlantic, when we remember the prevalence there of post-nasal catarrh.

There are a few other and less important subjective signs. There may be accumulation of mucus at the back of the throat, a reflex cough from the irritation produced, or frequent hawking from the sensation of a foreign body in the throat which cannot be dislodged. Then there may be a watery discharge from the nostrils, and even a little excoriation may be produced.

Finally, there is always more or less general disturbance of nutrition. The child is anæmic. He is stupid to a degree greater than can be accounted for by the often very slight deafness. He is peevish, reluctant to play, and disinclined for exercise. He takes cold in his chest, as we are told, on the slightest provocation; eats capriciously, and his bowels may be very irregular.

We now come to the objective signs, which, taken even separately and apart from special examination, are highly characteristic. To the physiognomical aspect of these patients I have referred in a former lecture. Let me recapitulate the points briefly. The buccal respiration attracts the attention of the most casual observer. It does not consist merely in elevation of the upper lip as occurs in swelling of the middle turbinated tissue; but the lower jaw hangs away from the upper to a greater or less degree, while the lips are prominent and expressionless from enfeeblement, through partial abolition of function of the orbicularis oris. Then when we have a good light thrown squarely on the child's face we see a little depression on each ala of the nose, situated at the angle between the superior and inferior lateral cartilages. This ap-

parently insignificant sign I consider quite pathognomonic either of the present or former existence of post-nasal growths. At any rate we do not see it in other cases of nasal obstruction. But besides this dimple, we observe with it that the nostrils are unusually narrow, while the bridge of the nose appears correspondingly widened. The collapse of the alæ and the dimples are due to the same cause. From inability to use the nose we must assume that the dilators of the nostrils are functionally in abeyance, and the alæ therefore collapsed; and this granted, we should assume that the portion least supported by the cartilages falls in the most. This portion is the angle between the superior and inferior lateral cartilages to which I have just referred, and which is filled in merely with a little connective tissue. Hence it collapses and forms a dimple.

I have heard this depression and the slit-like nostrils referred to as a symptom of enlarged tonsils; but, as I shall presently point out, enlarged tonsils frequently accompany post-nasal growths; and although tonsils may be so swollen as to interfere seriously with buccal respiration, I hardly think they can encroach upon the post-nasal space any more than upon the Eustachian tubes as they were formerly supposed to do. So until I see a case of enlarged tonsils without even the remains of post-nasal growths, I shall believe the signs I speak of to be pathognomonic of post-nasal obstruction. In confirmation of this hypothesis I may point out that even in double tonsillitis, where the tonsils may be more swollen than ever occurs from chronic enlargement, the patient almost invariably adopts nasal respiration.

While still on the topic of the nose I must inform you that in a large number of patients suffering from post-nasal growths, the cavities of the nasal fossæ appear small and ill-developed; while in fewer cases the hard palate is highly arched and narrowed anteriorly. And this condition we may find in adults where little else remains to tell of the former obstruction; for, as I shall presently point out, these growths generally undergo more or less atrophy at puberty, just as is the case with enlarged tonsils; but only after much mischief, now irremediable, has resulted from their presence. This ill-development of the nasal cavities may be to a certain extent aggravated in consequence of their posterior obstruction and

disuse, and is no more than one would expect on physiological grounds. The form of superior maxilla to which I refer is commonly described by dental surgeons as V-shaped. The contraction is sometimes excessively pronounced opposite the bicuspids; and the more pronounced it is, the higher is the vault and the greater the encroachment on the nasal cavities. Sir John Tomes says that this form of superior maxilla is frequently seen in children with enlarged tonsils, which, I repeat, frequently accompany post-nasal growths. He remarks, moreover, on the buccal respiration, but probably was not familiar with the vegetations. He attributes the narrowing of the superior maxilla to undue pressure of the buccinator on the alveoli consequent on the depressed lower jaw. But although the fact he notices is of extreme value, we need not accept his somewhat far-fetched explanation of it. Of these points I shall have more to say when I discuss the etiology and pathology. ("A System of Dental Surgery," by Sir J. Tomes; 2d edition. In the 3d edition, 1887, the author's remarks are considerably curtailed.)

The next step in the examination of the patient is to inspect the pharynx. Here again the appearances are often quite distinctive. Very frequently, but by no means in the majority of subjects, we find the tonsils enlarged. Sometimes they are enormous, and meet in the middle line; and then we may defer the verification of our diagnosis until they have been treated by operation or otherwise. But it is especially when the tonsils are normal, or nearly so, that the pharyngeal appearances are so characteristic. The mucous membrane of the palate and posterior wall is often almost œdematous in appearance, although the color is heightened rather than otherwise; that is to say, there is a semi-transparent and very watery aspect of the structures. This is seen especially well in the uvula, which may be rather elongated and twisted to one side—due, I imagine, to a commencing paresis of the mucular structure on the opposite side. The palate, as I have already remarked in discussing the abnormalities of articulation, may be thoroughly paretic, refusing altogether to respond to tactile stimulation. But when we have the paretic palate, the œdematous juicy appearance of it, if I may so speak, is not so marked and it is only in long-standing and generally anæmic cases that an extreme degree of muscular weakness is reached.

On the posterior wall of the pharynx we invariably see more or less mucus running down from the post-nasal region. Sometimes it is quite puriform, while at others it consists solely of thick tenacious mucus. Occasionally we have to wash it away with a douche or brush before we can inspect the subjacent mucous membrane. This is always more or less granular. But the granulations share the general surrounding aspect. They look watery and œdematous, as compared with the granulations seen in common granular pharyngitis, and frequently appear paler than the membrane from which they spring. In this case they may attain a considerable size, increasing in the upper parts until they disappear behind the velum and become continuous with the masses of lymphoid tissue which crowd the post-nasal space. When the connection is so obvious as this, and the granulations of the pharynx are almost in direct continuity with the post-nasal growths, we can hardly have any doubt as to their pathological anatomy, and we are inevitably led to suspect that the granulations of granular pharyngitis are pathologically if not etiologically identical with post-nasal growths. Sometimes we find sessile or pendulous growths attached to the uvula. I have seen them over an inch and a half long—long enough, in fact, to pass into the œsophagus during deglutition, and thus fortunately to avoid the laryngeal cavity.

Rarely one discovers a pharyngitis sicca, which in the absence of middle turbinated swelling or rhinitis sicca, is apt to confuse the diagnosis. But in such cases we find the growths confined to the upper part of the naso-pharynx, obstructing, in fact, the superior and middle meatus. Respiration is not buccal, but is carried on through the inferior meatus solely, which, as I pointed out in my first lecture, is generally the cause of the mucus-desiccation.

We now pass on to ocular and digital examination of the neoplasms themselves.

In recent cases in young children it is impossible to use the post-rhinal mirror, partly because of the insubordination of the patient, but even more on account of the irritability and contractility of the soft palate. But in cases of long duration, where the velum is anæmic, pendulous, and not responding to tactile stimulation, we may complete our diagnosis with the mirror. The image has a very varying appearance, but what-

ever the aspect of the individual growths, we see the post-nasal region more or less filled up. When the obstruction is not large, and especially when the surface is smooth and uniform, it may easily escape detection; but, by directing attention to the outline of the summit of the choanæ, if we see this hidden, we may conclude on the presence of neoplasms. Frequently, however, we see the posterior wall, the vault, and the parts surrounding the Eustachian tubes, studded with excrescences varying in size from that of the conventional hemp-seed to the magnitude of a cob-nut or more. Occasionally they hang down in clusters, currant-like, from the vault and posterior wall. And in old-standing cases we may see a uniform mass rising from the posterior wall, overlapping the Eustachian tubes, and extending almost into the posterior nares and downward so as to force the soft palate into a perpendicular situation. When the growth consists of individual lobules, we may see them readily through one of Zaufal's long nasal specula. But although this surgeon operates through his instrument, its employment is painful and quite unnecessary for diagnosis. The growths are generally pale in color, smooth, and moist on their surface. But the color is sometimes as dark as that of the pharyngeal mucous membrane.

Whenever, for the reasons I have stated, the post-rhinal mirror cannot be employed, we may confirm our diagnosis with the finger thrust upward behind the soft palate. This, like all manipulations in the throat, must be at once bold and gentle, while we must irritate the pharyngeal wall as little as possible. Digital examination is not always as easy a matter as it appears, so closely may the velum be applied to the posterior wall. When there is any difficulty it can always be surmounted by putting the fore-finger, the palm of the hand looking necessarily upward, behind the posterior pillar, a little above the tonsil, and sweeping it upward. The finger then discovers a soft, yielding mass, seldom distinguishable as separate growths, but giving a very similar sensation to that afforded by a varicocele. The obstruction is never so great but that we can reach the septum, which, by the way, is rarely or never involved in the morbid process. We can also feel the orifices of the Eustachian tubes. It is the posterior wall and vault especially that are covered with the soft, yielding neoplasms. Sometimes the finger can detect no more than

that the mucous membrane feels velvety and thickened; while in other cases, where, from other objective signs, we should expect to find a considerable mass of growths, we find only a few comparatively hard excrescences about the upper part of the posterior wall. This latter condition occurs for the most part in adults, in whom previously voluminous growths have undergone atrophy as puberty advanced. But taking all cases, the consistence of the neoplasms varies greatly. When we withdraw our finger we are certain to find it streaked with blood—a contingency seldom occurring when the post-nasal region is free of these vegetations.

In every case of the disease we should not omit an examination of the larynx and thorax. In the former we generally discover more or less chronic congestion and even thickening of the mucous membrane, while in the thorax we may discover various degrees of pigeon-breast, evidence of the obstruction to respiration.

And now as to the diagnosis. There are very few affections for which post-nasal growths can be mistaken, and practically errors need never occur. The age of the patient generally precludes the possibility of fibrous naso-pharyngeal polypi. The latter scarcely ever occur before puberty; while the same remark applies to mucous polypi occupying the posterior parts of the choanæ. The fibrous polypus is hard and resistant, bleeding readily, purple or red in color. There is frequent and severe epistaxis, distortion of neighboring parts, etc., so that a mistake can hardly arise.

Anterior rhinoscopy will eliminate other sources of nasal obstruction. Retro-pharyngeal abscess, when occurring insidiously in infants, may be mistaken for post-nasal vegetations. But the dysphagia, the rigid neck, with retraction of the head, and the nocturnal elevation of temperature, accompanied by a fluctuating swelling in the back of the pharynx, ought to establish the diagnosis without much difficulty.

The duration of post-nasal growths cannot be definitely stated.. Probably most cases advance or remain stationary until the fifteenth year or so, when, as is generally conceded, they atrophy, or lose their importance from the widening of the cavity in which they are placed. We must, however, never look forward to this atrophy as the *vis medicatrix naturæ*, since long before it occurs, much permanent mischief may result from their presence.

The etiology of this affection is extremely obscure, and has given rise to much discussion. In the first place, as you will have already gathered, it is an affection almost confined to children, although very rarely we may find the growths developing in after-life, and even after the fortieth year. The two sexes appear to be equally affected. No special diathesis, as far as my observation goes, can be charged with their genesis; although, seeing that many cases appear to result from chronic nasal catarrh, struma may indirectly have some influence. There are, however, certain associated conditions which throw some light on possible causes. I have mentioned the frequent co-existence of enlarged tonsils, and the badly-developed nasal cavities. And along with these, we sometimes find a narrow or typically V-shaped hard palate, with dental irregularities in the superior maxilla; while, in a large proportion of all cases of cleft-palate, we find the post-nasal region more or less crowded with vegetations.

So that we cannot, I think, infer that inflammatory action is always the cause of post-nasal growths. But before I can discuss further their etiology, I must make you acquainted with their pathological anatomy.

Speaking concisely, we say that the post-nasal growths consist of lymphoid tissue, and are nearly identical in structure with the tonsils. This lymphoid tissue is frequently called adenoid, after the nomenclature of His; but seeing that we have no authority whatever for assuming the tonsils or other accumulations of this tissue to have a glandular structure or function, the term is somewhat inappropriate. But anyhow, if we do prefer to speak of adenoid post-nasal growths, we must remember they are in no sense glands. This tissue, I need scarcely remind you, consists of a retiform connective tissue, the trabeculæ of which are formed of ramified corpuscles, which may or may not retain their nuclei. The network is more or less stuffed with lymph-corpuscles. Remembering that physiologists are unable to assign any definite function to this tissue, and that we find it, one may almost say, scattered everywhere; forming the sustentacular tissue of the blood-vessels and the network within the lymphatic glands; composing the solitary and agminated glands of the intestine, the tongue, and the tonsils; constituting the thymus gland and the spleen; situated in the intestinal mucous membrane,

and forming a distinct layer in the mucous membrane of the pharynx and naso-pharynx, in which situations it is gathered up here and there into distinct structures, especially in the region known as Luschka's tonsil, and in a band running round the posterior wall on a level with the margin of the superior constrictor; remembering, I say, the evident importance of the tissue, although we cannot discover what purpose it fulfils in physiology, we cannot but be in doubt as to its significance when it appears as a pathological product. Is it inflammatory in its origin? Is it strictly a neoplasm? Or is it still physiological? Such and similar questions naturally suggest themselves, and would be of considerable importance in the treatment of the sufferers if we could answer them truly. Are we, for instance, to operate whenever we discover post-nasal growths, independently of any interference or not which they may cause?

Although I do not pretend to answer physiological questions, I shall nevertheless venture to suggest an explanation of the special forms of lymphoid growths which we are discussing.

I must, however, diverge for one moment to show you how I regard the tonsillar structure and its hypertrophy, in order to warn you of what I cannot but consider a real danger. There is a strong tendency to regard the tonsils as physiologically unimportant. This view is partly maintained, I imagine, by the unfortunate facility with which the structures can be amputated, and our consequent self-gratulation on having done something. But when we remember the enormously rich blood supply of these organs, I think we must allow them an important part in the economy, although we may not have the least idea as to its nature. Blood cannot flow through any structure without either causing growth of that organ or being used by it for some work—unless, indeed, the tonsillar veins, etc., contain arterial blood—a supposition too ridiculous! And since the tonsils remain stationary in size, we know from physiological laws that they must perform considerable work. Therefore do not let us remove them unless their hypertrophy is causing real mischief and cannot be subdued by medicinal measures.

And since it is more than highly probable that the lymphoid tissue of the tonsils performs definite work, so must we

assume the same tissue elsewhere to possess its functions. Nor shall we be going too far in assuming that the collections of lymphoid tissue known as Luschka's tonsil, and others scattered over the naso-pharyngeal surface, are physiological.

But, as I just now casually remarked, if nutriment to the tissues is put to no other uses, it will augment the bulk of those tissues. In many organs of the body a special structure is provided for the absorption of superfluous nutriment, in what is known as adipose tissue. Only second to this, as it appears to me, in capacity for assimilating nutriment is the lymphoid tissue. Indeed the condition commonly described as hypertrophic pharyngitis, where the velum, the uvula, and the pharyngeal walls are uniformly thickened, is simply an hypertrophy of the lymphoid tissue existing in the deeper layers of the mucous membrane, and occurs almost invariably, as far as my observations have gone, in plethoric or overfed individuals. In my own mind it is quite certain that just as fat is deposited in the subcutaneous tissue, so does this lymphoid structure hypertrophy in the submucous tissue, at any rate in the case of adults.

But these remarks obviously do not apply to special collections of the tissue, such as the tonsils and those on the posterior naso-pharyngeal wall. Enlargement of these does not accompany systemic hypernutrition. Before puberty, however, these collections of lymphoid tissue apparently have a great tendency to hypertrophy under slight or imperceptible stimulation. The irritation leading to increased blood supply may be only local, as in enlarged tonsils and post-nasal growths. In subacute, running into chronic tonsillitis, where there can be no doubt as to the inflammatory condition of the mucous membrane, we may get permanent increase of the lymphoid tissue, and enlargement of the organ. And I imagine that it is either to some similar conditions that we must look for an explanation of the presence of post-nasal growths, or else to some other cause of local repletion.

In the first case, it is highly probable that a chronic inflammatory condition of the nasal mucous membrane is the cause of the hypernutrition in the lymphoid tissue of Luschka's tonsil and that of the posterior wall. Thus it is especially in cold damp climates that these growths have been detected. In Denmark—Meyer, of Copenhagen, being the first

to draw due attention to their importance—they appear to be very common. In the north of Germany, in Great Britain, and in France they must be nearly as prevalent. And it is just in such climates that acute and chronic rhinitis is most common in children. In America, where the atmosphere is peculiarly dry, the post-nasal growths are much less common; and here, although in adults post-nasal catarrh is almost the rule, there is less liability to take cold from simple exposure to the weather. As with ordinary catarrhs, so post-nasal growths run in families; but I have seen no evidence of hereditary influence as is claimed by Meyer and Löwenberg. Nevertheless, dental surgeons acknowledge distinct heredity in the matter of the V-shaped superior maxilla; and from the frequent associations of this with post-nasal growths, we may so far assume the influence of heredity in the latter. So that, considering these various facts, I think we may safely infer that nasal catarrh is an important element in the production of post-nasal growths. But many of such cases, I suspect, are assisted by other means through which local hypernutrition is established. To these I will now refer.

The frequency with which the post-nasal region is found crowded with lymphoid growths in cases of congenital cleft-palate, points to some other cause than nasal and post-nasal catarrh. It has been supposed that the food passes into the post-nasal region during deglutition, and so sets up a chronic inflammatory condition. But as a matter of fact, this is very rarely a source of complaint in cleft-palate patients. Owing to the sphincter-like contraction of the superior constrictor, referred to by Billroth, and often observed by myself both in these patients and by means of anterior rhinoscopy in ordinary cases, the posterior wall of the pharynx comes almost into apposition with the margin of the hard-palate, while the tonsils by the same agency are approximated; and it is by these means that the deficiency is effectually closed. So the theory of direct irritation is insufficient. According to Dr. N. W. Kingsley, of New York, the principal living authority on the subject of oral deformities, congenital cleft-palate is usually accompanied by more or less deformity of the sides of the alveolar arch. ("A Treatise on Oral Deformities," etc., English ed., 1881.) Sometimes they are abnormally far apart; but more frequently they are found pinched together. This

must correspond, as in cases of the V-shaped superior maxilla, with contracted nasal fossæ; and thus far the condition is similar. And it appears to me probable that for all cases where this contraction obtains, we have a very simple explanation of post-nasal growths. But to make this causation plain, you must pardon me if I refer once more to the physiology of the nose. The principal function of this organ appears to be warming of the inspired air; that is to say, the tissues are constantly giving up heat. Now we know that the generation of caloric implies work-potential as much as the secretion of bile, mind-energy or any other physiological phenomenon. That is to say, oxygenation of tissue must be carried on at a peculiarly high rate in the nose and post-nasal region. (According to Aschenbrandt's experiments, by the nasal cavities alone the inspired air is raised to the temperature of the body. "Ueber die Bedeutung der Nase in Respiration," Würzburg, 1886.) Now, if the post-nasal space is deprived of the cold air necessary to the performance of its functions, whether from congenital malformation as in cleft-palate, or from nasal obstruction caused by narrowing of the nasal fossæ in combination with chronic rhinitis; if also the tissues are themselves in a state of inflammation, and so their duties seriously interfered with; we must assume that the rich blood supply will be converted to other uses, and over-nourish that lymphoid tissue which in young people, and wherever situated, is so ready to assimilate property to which it is not strictly entitled.

That the post-nasal region has an important share in calorification, we may presume from the fact that, owing to the direction of the epiglottis backward during ordinary respiration, the inspired air is swept over the posterior wall. I may suggest, by the way, that this position of the valve has something to do with the difficulty attending buccal respiration.

Moreover, in cases of cleft-palate, apart from the usual accompanying contraction of the nasal cavities, the inspired air must enter the buccal cavity before it reaches the posterior naso-pharyngeal wall. Consequently the latter is prevented, partly for this reason, from performing its functions.

I hope, gentlemen, you will not think I am drawing conclusions from insufficient premises. I do not think, myself, I am trespassing beyond the rules of scientific deduction. We

have certain facts known concerning a given phenomenon; our reason tells us that there must be one or more causes both of the phenomenon and its various attributes; and by questioning how the individual points may be produced, we should arrive at the real nature of the phenomenon. In the present case we are given the facts of persistent nasal obstruction, either from congenital malformation, or from chronic rhinitis, or both. We are also given the fact that the tissue of which post-nasal neoplasms consists is everywhere prone to over assimilation; and we are given the fact that interruption in the performance of work must lead to hypernutrition. These are our data: and we can hardly be considered unscientific in considering them related to one another in the manner I have indicated.

The treatment of post-nasal growths need not detain us long. In the first place you should not remove them simply because your finger may detect a few; and unless there is marked buccal respiration with its accompaniments, they may be left alone and allowed the honor of physiological respect. But this rule has one reservation. If the hearing is affected —and for this we must not take the word of even enlightened patients, but rest solely on the aural speculum—if the hearing is faulty, I say, and we can detect any post-nasal growths, we should always remove them, even if they are causing no Eustachian obstruction.

Even where we have a perforation of the membrana tympani with a suppurative otitis, we may expect the most remarkable results from removal of the growths—even though they are too few to produce any direct symptoms. Such cases, together with a fact I have repeatedly observed, viz., that the improvement in hearing is less marked when we remove the growths by several operations than when a complete clearance is made at one sitting, make me believe that the advantages of the operation are largely due to a counter-irritant action. Another argument in favor of a single operation is the recurrence of the growths if they are not completely eradicated. I have seen several instances of this; and although the view is not generally held, Meyer has remarked on the fact.

The instrument I always employ is a modification of Löwenberg's forceps. The precision, rapidity, and accuracy with

which these can be used, has made me reluctant to employ other instruments. The cutting-edge should be confined to the posterior and upper surfaces of the spoon-shaped extremities, else there is some danger, especially when no anæsthetic is used and the finger cannot guide the instrument, of gripping the mucous membrane on the posterior surface of the soft palate. Whenever possible, chloroform should be administered, and this in preference to ether, as the latter produces congestion of the mucous membrane, and so increases the hemorrhage. The shoulders should be slightly raised, with the head inclining backward, in order that the blood may flow from the nostrils rather than into the pharynx. Then, with the forefinger of the left hand as a guide in the post-nasal space, we thrust the forceps behind the velum, and remove the growth piece by piece. When at all resistant, they should be twisted as much as possible before being torn off. Working in this manner we can rapidly remove every particle, and run no risk of doing mischief. In order to emphasize the sort of trouble that may arise from careless operating, let me tell you that, in one of my earliest operations, I removed inadvertently a splinter from the vomer; and in another case where I allowed a clinical clerk to operate, I found to my dismay that he had stripped the whole of the posterior wall of mucous membrane! But by feeling every portion with the finger before removing it, we obviate all such dangers. When operating at several sittings without an anæsthetic, we cannot use the finger in this manner, and must trust to our sense of touch transferred to the cutting-edge of our forceps, while making, at frequent intervals, digital examinations.

The hemorrhage is sometimes rather profuse, but seldom sufficiently so to necessitate the exhibition of styptics. I have never heard or read of a case of alarming hemorrhage.

The after-treatment is of some little importance. The temperature usually rises slightly for a day or two, and sore throat is complained of. But forty-eight hours in bed, with slop food, are all that is required in the way of treatment. From that time the patient's health begins to improve rapidly in every way. His hearing distance is, in nine cases out of ten, immediately increased, and often to an extraordinary degree. The snoring also disappears, but the buccal respiration and the faulty articulation take longer to overcome. The

patient needs careful education in speech and respiration if he is to surmount the long-enforced evil habits. And we must remember that chronic catarrh with hypertrophy of the mucous membrane, deflected septum, and general narrowing of the nasal fossæ, not only are often the causes of the post-nasal growths, but in themselves may be sufficient to produce buccal respiration. Therefore these points demand as much attention as the main trouble.

But there is one point upon which I must lay great stress, namely, the careful inspection of the membrana tympani for three or four days after the operation. Rarely, and even while the hearing is notably improved by the operation, we may find subsequently a slight suppurative otitis. Perhaps no pain is complained of in the ear, and the only indication of the mischief is a small perforation of the drum-skin. In the very few cases I have seen of this accident, no harm has resulted. The perforation readily healed, almost as though it were traumatic, and left no trace of its existence. I do not think that any blame is to be attributed the operator when this occurs and I do not consider it in any sense an argument in disparagement of the operation. Why it should occur I do not know; it may possibly belong to the little understood domain of counter-irritation. It is more probable, however, that the first opening of the Eustachian tube admits blood as well as air. This putrefying would indubitably excite a suppurative otitis. Not a few cases are on record where epistaxis has been followed by otitis, probably due to similar causes. I have seen one such myself, and can recall a very severe case of otitis following the removal of a polypus springing from the middle turbinated bone.

Various other methods are employed for the eradication of these growths, from the finger-nail to the cold snare applied through Zaufal's funnel. But to my mind nothing is so precise, speedy, and I may say miraculously beneficial, as the forceps. How a curette can avoid injuring the mucous membrane along with the neoplasms, I cannot see; nor can I believe in the removal of such dense structures, as one sometimes finds, with the finger-nail. But it is only fair to tell you that one of the most celebrated aurists in London affirms that if he finds post-nasal growths on digital examination, he removes, then and there, without introducing his finger a second

time, as many of the vegetations as he considers necessary—which I venture to think is not many. For my own part, whenever I have attempted removal by the finger-nail, which I must confess has not been often, I have found the improvement in hearing but slight, while sometimes a rapid recurrence of the growths has taken place.

I think it is generally advantageous to apply, a few days after the operation, some form of astringent to the post-nasal region. We may use a diluted glycerin of tannin, or a solution of perchloride of iron, applying it with a camel's-hair brush behind the soft-palate on alternate days, or even daily, as soon as all pain on swallowing has passed away. And besides this, it will generally be necessary to keep the passages of the nose and vault of the naso-pharynx clean with antiseptic sprays, such as the lotio alkalina, to which I have referred so often.

The paretic palate is as difficult to treat as the buccal respiration; but, at any rate in young and intelligent persons, we can hope for much improvement in the speech. Education in elocution gives the best results, seeing that the faulty habits in word-production must be eradicated. I always recommend my patients to accustom themselves to gargling with cold water on every opportunity, and, if they sing, to use head and falsetto notes as much as possible. By these means the levators of the palate and the superior constrictor are thrown into action. In long-standing cases I have had good results from applying faradism to the muscles at fault. But the process is tedious, and few possess the necessary patience for its satisfactory employment.

FORMULÆ.

Nebula alkalina:

℞	Sodæ bicarbonatis, ...	gr. xv.
	Boracis, ...	gr. xv.
	Acidi carbolici, ...	gr. iv.
	Glycerini, ...	♏ xlv.
	Aquæ fontis, ...	ad ℥ i.

Used as a nasal spray or for insufflation diluted with two parts of warm water.

℞	Tincturæ sanguinariæ Canadensis,	ʒ i. ad ʒ ij.
	Aquæ, ...	ad ℥ i.

As a stimulating nasal spray.

Insufflations:

℞	Pulv. gummi rubri, ...	p. i.
	Pulv. amyli, ...	ps. ij.

For use as a nasal stimulant and antiseptic

	Iodoform,	
	Pulv. Amyli, . . .	āā partes æquales.

As a powerful antiseptic in ozæna. Iodol may be substituted when the smell of iodoform is badly tolerated.

VOLUME V. NUMBER 2.

WOOD'S

Medical and Surgical MONOGRAPHS

Consisting of Original Treatises and Reproductions, in English, of Books and Monographs selected from the latest literature of foreign countries, with all illustrations, etc.

CONTENTS

THE FORMATION AND EXCRETION OF URIC ACID, AS ELUCIDATING ITS ACTION IN THE CAUSATION OF DISEASE. By A. HAIG, M.A., M.D.

THE INITIAL STAGES OF CONSUMPTION: THE NATURE AND TREATMENT, INCLUDING DIETETIC SUGGESTIONS. By HORACE DOBELL, M.D.

ECTOPIC PREGNANCY AND PELVIC HÆMATOCELE. By LAWSON TAIT, F.R.C.S.

PUBLISHED MONTHLY

PRICE, $10.00 A YEAR SINGLE COPIES, $1.00

FEBRUARY, 1890

NEW YORK
WILLIAM WOOD AND COMPANY
56 AND 58 LAFAYETTE PLACE
1890

2021

Entered at the Post Office at New York, as second-class matter.

Copyright by Wm. Wood & Co., 1889.

PRESS OF
THE PUBLISHERS' PRINTING COMPANY,
30 & 32 WEST 13TH STREET,
NEW YORK.

CONTENTS.

THE FORMATION AND EXCRETION OF URIC ACID, AS ELUCIDATING ITS ACTION IN THE CAUSATION OF DISEASE.

CHAPTER I.

PAGE

Uric Acid in Relation to Migraine, ... 305

CHAPTER II.

Uric Acid in Relation to Various Morbid Conditions, . . . 329

THE INITIAL STAGES OF CONSUMPTION: THE NATURE AND TREATMENT, INCLUDING DIETETIC SUGGESTIONS.

CHAPTER I.

The Object of the Treatise, ... 367

CHAPTER II.

The Tubercle Bacillus the most Dangerous of Several Irritants that Cause Tuberculization, ... 378

CHAPTER III.

Domiciliation, Localization, and Dissemination of Bacilli, . . 389

CHAPTER IV.

Bacillary Tuberculosis Considered in Detail, ... 402

CHAPTER V.

Answers to Questions Suggested in Chapter I., ... 414

CHAPTER VI.

PAGE

The Object of Correct Pathology and Diagnosis, ... 427

CHAPTER VII.

Diet and Regimen, ... 441

CHAPTER VIII.

Diet Tables, etc., ... 451

ECTOPIC PREGNANCY AND PELVIC HÆMATOCELE.

The Nature, Causes, and Treatment of these Conditions, . . 463

THE FORMATION AND EXCRETION

OF

URIC ACID,

AS ELUCIDATING ITS ACTION IN

THE CAUSATION OF DISEASE.

BY

A. HAIG, M.A., M.D. OXON., M.R.C.P.

URIC ACID
IN THE CAUSATION OF DISEASE.

CHAPTER I.

SOME researches on the excretion of uric acid on which I have been engaged for several years, now seem likely to throw new light on the part played by this substance in the causation of disease.

Some seven or eight years ago, being a regular sufferer from a severe form of migraine, and obtaining only inadequate relief from much advice and many drugs, I began to make careful notes of the circumstances of my trouble, and to regard especially all foods, drugs, exercise, and other conditions which had any real or supposed connection with it. For a long time there was little result, till on giving up meat, I at once found the intervals between the attacks prolonged, and persistence in this change of diet gradually produced almost complete immunity—an immunity which continues to the present time, but only so long as the diet is adhered to; and I have now been for six or seven years an almost total abstainer from butcher's meat, beer, wine, and cheese; though I have to some extent replaced the meat by an increased quantity of milk and fish; but even now any serious infringement of these diet rules is pretty sure to be followed by a headache.

The results I obtained by leaving off meat and the relationship noticed by many observers between this headache and gout, led me to examine the urinary excreta.

My analyses have extended now over a period of nearly four years, and have for a large part of the time been practically continuous, at least with regard to my own urine, which

was kept for periods of six weeks, two months, and, more recently, for two periods of six months each, without any interval, the analyses being performed every day and records kept of general health, and presence or absence of headache, with notes of the exercise, drugs, and food taken; and I have tested on myself, and occasionally on others, the actions as regards the urinary excreta of all the most important drugs used in the treatment of gout, rheumatism, and headache, as also the effects of various modifications of diet, so that in the records of these I have now a considerable accumulation of results bearing on the question.

The processes I have used in my experiments are: for urea, the hypobromite test with the apparatus of Dupré, temperature and pressure being always noted, and variations of volume due to them as far as possible allowed for. For uric acid, Prof. Haycraft's process, as described in the "British Medical Journal," Vol. 2, 1885, p. 1,100, and for acidity a solution of phenol-phthalein with a graduated solution of soda.

And having found out in this way the remarkable fluctuation in uric-acid excretion corresponding to the headache, I began to speculate as to its cause, and was then led by observing the strong acid reaction of ales, and their important connection with gout, to try the effects of acids on myself, and from that I went on to try alkaline and numerous other drugs.

And, first of all, I began to try the effect of butcher's meat and cheese on myself, having previously abstained from them for several years, and I continued to take them, in slowly increasing amounts, for some weeks. At first, they seemed to have very little effect; but gradually the threatenings of headache got more frequent and severe, till one or two severe headaches gave me the opportunity I wanted of testing their exact relation to the excretion of uric acid.

My results at first were very equivocal, and though it seemed to me that there was some relation between the headache and the excretion of uric acid, I thought that the uric acid was, if anything, slightly diminished on the days on which threatenings of headache occurred. When, however, I got a decided headache, it occurred to me to separate the urine passed during the headache from that before and after, and I at once got definite results, which were not only exactly repeated on subsequent occasions, but these latter results showed

clearly the diminished excretion before and after the headache, and so served to explain the equivocal results previously obtained.

I shall now give, in the form of a table, the result obtained during one of the first headaches I was able to observe carefully:

Date.		Urea.	Uric Acid.	Relation.	
December	28,	561.	16.	1:35.	
"	29,	499.	12.7	1:39.	
"	30,	415.	12.4	1:33.	
"	31,	401.	11.5	1:34.	
January	1,	362.	10.4	1:34.	
"	2,	368.	16.1	1:22.	(Headache day.)
"	3,	356.	10.2	1:34.	
"	4,	418.	11.2	1:37.	

Here several points are to be noted: 1st, the large excretion of urea on December 28th; 2d, its gradual and steady fall to January 1st; 3d, the fact that except on the 30th (when the excretion was 1:33, and therefore probably equalled the formation), uric acid was below its normal relation to urea, and was being kept back or retained the whole time, the amount that appears to have been retained on these days is about 4 grs., and the uric acid exceeds the urea on the day of headache (January 2d), by nearly five grains, so that we can account for the greater part of this excess by the previous retention.

From some results obtained on another occasion it comes out that, during a period of headache, lasting eight hours and fifty minutes, 5.7 grs. of uric acid were passed, this being at the rate of 0.64 gr. per hour (and I think that my average at this time was about 12 grs. per day, or 0.5 gr. per hour), and further, on the same day, during fifteen hours and ten minutes, free from headache, 6.9 grs. were passed, or 0.46 gr. per hour. On the other hand, the urea varied but little, being for the first period about 22 grs. per hour, and for the second, about 21 grs. per hour. The uric-acid variation was little short of $\frac{1}{3}$, while the urea varied less than $\frac{1}{20}$. In the table just given, we find precisely the same relation, only here the headache lasted the whole twenty-four hours, and we have the large uric-acid excretion of 16.1 grs., or 0.67 gr. per hour, against a total excretion on the previous day, when there was only about one and a half hours of headache, of 10.4 grs., while the urea only varied from

362 grs. on the day before the headache, to 368 grs. on the day of the headache; the uric acid was here increased by more than a $\frac{1}{3}$, while the urea only increased $\frac{1}{60}$.

Then I purposely divided the urine of the headache day into two parts. In the first part, 6:45 P.M. to 8:30 A.M., viz., thirteen hours forty-five minutes, 9.2 grs. of uric acid were passed, or 0.66 gr. per hour; and in the second part, ten hours fifteen minutes, 6.9 gr., or 0.67 grs. per hour, so that on this day, when the headache lasted the whole 24 hours, the uric-acid excretion remained much above normal the whole time.

It appears, then, that on both these days, when there was headache present, the uric-acid excretion was above 0.6 gr. per hour, but when the headache was absent either before or after the paroxysm it fell below the average, as 0.46 gr. on the first occasion, and 0.43 grs. on the second.

There is thus shown to be a very definite relation between the uric acid and the headache, viz., that it (uric acid) is diminished before, and increased during the paroxysm, and what is more remarkable, the increase and diminution very nearly balance one another. Thus, when there is an excess of 4 grs. on the day of the headache, it will generally be found that the uric acid has fallen below the urea, to just about 4 grs. in the previous five or six days. Where uric acid falls below the urea, after the headache, this, I think, merely shows that there is again some retention going on.

Having got thus far, it is necessary to consider what may be the cause of these fluctuations in the excretion of uric acid. It can hardly be due to anything wrong with the kidneys, for that would probably affect both urea and uric acid; besides the cause here is very transient in its effects, and therefore probably not organic. Moreover, we have proof that urates may accumulate while the kidneys are quite normal, for Ebstein has called attention to the fact that extensive arthritis uratica may be found in the post-mortem room, along with normal kidneys; and on the whole it appears to me that the best explanation that has yet been given and the one which seems to best fulfil the above requirements, is that of diminished alkalescence of the blood, which is no new theory, but in one form or another has existed for a long time. Thus Sir W. Roberts says, in the last edition of his work on "Urinary and Renal Diseases," Ed. 4, p. 73: "It may be regarded as prob-

able that the defective power of the kidneys to eliminate uric acid in gout, arises from a diminished alkalescence of the blood." Or, as I should say, the uric acid is not in solution in the blood, and is never brought to the kidneys to be excreted.

In this connection the researches of Dr. A. Auerbach (Virchow's "Archiv," 98, p. 512, "Ueber die Säurewirkung der Fleischnahrung") seem to me of some interest, as he shows that vegetable and animal feeders differ considerably in their power of preserving the alkalinity of their blood against acids formed in their bodies, or introduced from without, and that even where, as in animal feeders and man, this power is greatest, it may be gradually diminished, and finally overcome, by continued excess of acids. Now, beer and ale are strongly acid in reaction, and I found in a pint bottle of ordinary table beer, estimated by the same method as the acidity of the urine, the equivalent of about 14 grs. of oxalic acid. A pint of fourpenny ale, estimated in the same way, yielded the equivalent of 18 grs. of oxalic acid, and a pint of ordinary London stout 25 grs., so that it appears that the cheaper beers are more strongly acid than those of better quality. Mr. Winter Blyth ("Foods, Composition and Analysis"), says, that the chief acid in beer is acetic: so that people who indulge largely in these liquors may diminish the alkalinity of their blood, and produce retention of uric acid and gout.

To return again to the theory of diminished alkalinity of the blood, I will quote what Sir W. Roberts (previous reference, p. 56) says, with regard to the causation of "the alkaline tide": "If, as is believed, the normal alkalescence of the blood is due to the preponderance of alkaline bases in all our ordinary articles of food, a meal is *pro tanto* a dose of alkali, and must necessarily for a time add to the alkalescence of the blood."

And if an ordinary meal is a dose of alkali, a vegetarian meal, in which alkaline bases are so largely represented, must necessarily be a large dose of alkali; while, on the other hand, a meal in which butcher's meat, with its large containder of acid salts, and beer, with its high acidity, form a large part, will not be a dose of alkali at all, but rather one of acid, and the way in which this acid will prevent the excretion of uric acid, and cause it to be stored up in the body, is not difficult to understand.

Having thus found the relationship of migraine to the ex-

cretion of uric acid, and thinking over the connection of fourpenny ale to gout, it occurred to me, as above mentioned, to test the acidity of various ales and beers, and from that it was but a small step to try the effects, first of acids, and then of alkalies, on the excretion of uric acid itself.

And I was surprised to find that both in myself and other persons it was possible, within certain limits, to increase or diminish the excretion of uric acid at pleasure, acids having as their effect a decrease, and alkalies an increase of the excretion.

The following is an experiment which any one who has the apparatus for estimating urea and uric acid at hand may easily repeat.

Take the urine of 24 hours and estimate the urea and uric acid, and note the relation of the uric acid to the urea. It varies in different people somewhat, but may be something between 1:30 (*i.e.*, 1 gr. uric acid to 30 of urea) or 1:40; say for the sake of a definite number 1:35. Next day let him take several doses of acid, as ♏ xxv. of dilute nitro-hydrochloric, as lemonade, three or four times in the day, by preference with his meals; and estimate uric acid and urea as before; he will find a relation of uric acid to urea of, say 1:41, *i.e.*, the uric acid is relatively diminished, and probably also absolutely, but inasmuch as some alteration in food or exercise may have altered both urea and uric acid, it is safer to watch the relative excretion.

Then let him repeat the experiment on two other days, after an interval, using alkali (as 40 grs. of citrate of potash three times a day) instead of an acid, and he will get the reverse effect, and a relation say of 1:28 as compared with 1:35 before the alkali. These effects are invariable, so far as my experiments on myself and others go.

The results of experiments on these lines on myself and others have been constant, acids always causing a fall of uric acid, and alkalies a rise, and I shall now pass on to the theoretical explanation or working hypothesis of these results and explain by it as far as I can all the phenomena observed.

The theory which I am now going to speak of, is an adaptation of theories in the published writings of Sir A. Garrod and Professor Latham to my present purpose, and I think it will be seen that it gives us a fairly good working hypothesis of the action as observed by me of acids and alkalies.

I do not intend to enter into what may be said for and against the theories of Sir A. Garrod and Professor Latham; I only say, if they are true, they afford me a good working hypothesis. I will remark, however, with regard to the formation of urate of ammonium in the kidney, that the balance of evidence at the present time seems to be in favor of at least some formation of this substance taking place there.

We have, according to this theory, the following: 1st, the antecedent elements of uric acid formed in the liver, coming in the general circulation to the kidney. 2d, formation of urate of ammonium in the kidney. 3d, its partial excretion by the ureter. 4th, its overflow by the renal vein to some slight extent, and so its arrival in the general circulation in small amount. 5th, its passage in the general circulation to all the organs, and among others to the liver and spleen. 6th, its retention in the liver and spleen, because their tissues are less alkaline than those of many other organs, or the blood and tissue fluids in general. On this point Sir A. Garrod says (1 Lumleian Lectures, "Brit. Med. Journ.," 1, 83, p. 459): "May it not be the case that when uric acid exists in the blood it is attracted differently by different organs, and thus the spleen and liver more frequently contain appreciable quantities than other tissues? Or again, may it not be that in some organs, as the spleen, the substance of which, if not acid during life, rapidly becomes so after death, while the blood remains strongly alkaline, the uric acid becomes less soluble and more easily retained?"

We have only now to suppose, what I expect no one will dispute, that a dose of acid will diminish the alkalescence of the blood and tissue fluids and also that of the liver and spleen, and that a dose of alkali will produce the reverse effect, to see why and how they may alter the excretion of uric acid in the way I have shown that they do. And I suppose, therefore, that a dose of acid will increase the acidity of the liver and spleen, and cause increased retention of uric acid in them, while a dose of alkali will diminish their acidity and sweep out the uric acid accumulated there. And this corresponds exactly with the effects of acids and alkalies on the urine, and, as I have pointed out in my paper on headache above referred to, explains an important point which would otherwise remain obscure, viz., why one day we have a severe

headache with an excretion of 16 grs. of uric acid, and on another day little or no headache with an excretion of almost the same amount.

The explanation of this is now easy: on the day of headache the uric acid comes (apart from the urea) from the store in the liver and spleen, it is for a time in excess in the blood, and causes a headache; on the day of no headache the uric acid is formed along with much urea in the kidneys (see previous table, December 28th), and passes at once down the ureter with it, it is never in excess in the blood, and there is no headache. We have thus clearly two sources of uric acid, on these two days (see previous table, December 28th and January 2d), and the relation of the uric acid to the urea gives us the key to the situation, and tells us from which source it comes. On the 2d of January it is swept out of the liver and spleen and is in excess in the blood, on the 28th of December it passes out direct from the kidney. The overflow of uric acid from the kidney by the renal vein is small in amount, and is not sufficient to cause symptoms; but when this small overflow has gone on for several days accumulating in the liver and spleen, and is then suddenly swept out into the blood by alkali, there is a very considerable amount of uric acid in the blood, and we get headache and other symptoms of its presence. Again, why does the rise of uric acid excretion under alkali not continue as long as the drug is kept up? Because there is merely increased excretion, not increased formation, of uric acid, and the amount falls to normal or near it as soon as the store in the liver and spleen is exhausted, and the same with the plus excretion under salicylates; and conversely with acids after the liver and spleen have retained a certain amount the retention ceases, at least for the time.

There is another effect of giving acids of which I must say a few words; it may have been noticed from the figures that with the fall of uric acid produced by giving acid there is a rise of urea, and this is also, I think, a constant result; of course a large rise may be due to other causes, as much exercise or meat food, etc., but allowing for these variations there still remains the fact that a fall of uric acid, brought about by acid taken for the purpose, is accompanied by a slight rise of urea ("Journ. of Physiol.," Vol. 8, Nos. 3 and 4, p. 215). Thus, to take one instance out of many, the urine was taken for

two hours before and two hours after a dose of acid; in that before the acid the hourly excretion of urea was 20.5 grs., and the uric acid 0.57 gr.; after the acid the urea was 23.5 grs. per hour, and the uric acid 0.4 gr. per hour. Here there is a large fluctuation in the excretion of uric acid after the acid, and the reason is that the acid was taken to cure a headache, which was going on with the large excretion of 0.57 gr. per hour; there was a large amount of uric acid in the blood, and the acid was able at once to shut up a considerable amount of this in the liver and spleen, and so reduce the uric acid in the next two hours to 0.4 gr., but when an acid is given under normal conditions it can only act on the small overflow of uric acid by the renal vein, and its effect on the excretion is slighter and takes a longer time to show any results. What I believe may be the explanation of this increase of urea is as follows: It is well known that uric acid taken by the mouth does not appear as uric acid in the urine (Garrod, Lumleian Lectures, "Brit. Med. Journ.," 1, 1883, p. 549), but gives as its result a corresponding increase of the urea, and I think that precisely the same thing happens in the case under discussion. The dose of acid causes, we are supposing, a concentration of the circulating uric acid in the liver and spleen, and I think that a part of that which comes to the liver is converted into urea and so increases the excretion of that substance; and this is to some extent supported by the action of acids on this headache, as I have shown that a headache coincides with a large excretion of uric acid, and I am now able to prove that a dose of acid not only diminishes the uric-acid excretion, but cures the headache; and the headache is not merely put off and the uric acid retained for another occasion; but with the exception of a slight threatening during the "alkaline tide" of the next day (showing a considerable excretion of uric acid going on) the headache may go quite away, and not return for a long time, and this could be easily understood if we suppose that part of the uric acid retained by the dose of acid is at once converted into urea and so got rid of.

I will only note here in passing that in many sufferers from this form of headache the attack begins during the strongest "alkaline tide," *i.e.*, that due to the digestion of breakfast.

It has been noted that the excretion of uric acid is much affected by the digestion of food, and is three times as much

during the "alkaline tide" as at other periods (see Sir W. Roberts, "Urinary and Renal Diseases," Ed. 4, p. 71), but if the facts I have brought forward, and the explanation I have given, are true, a large part of this increased excretion must be regarded as a washing out of the uric acid accumulated in the liver and spleen in the (if I may so call it) acid tide period of sleep, and not as entirely due to increased formation of uric acid during digestion.

With regard to the question of increased formation of uric acid, the results given deal entirely with excretion, a surplus on one day being the consequence of previous retention, and taken over a long period of time results always come out with a relation of about 1 : 33, but I shall return to this point again, later.

Having thus found that acids always diminished the excretion of uric acid and caused it to be retained, and accumulate in the liver and spleen, and also that alkalies always increase its excretion by washing it out from these organs, it became at once easy to explain why my headaches had been less frequent and severe when I left off meat, beer, and cheese, and this was further elucidated by some experiments with meat taken for the purpose, which showed that while it increased the formation of both urea and uric acid, in their natural proportions, its phosphates and sulphates increased acidity, and so caused retention and accumulation of uric acid in the liver and spleen, and when on some future occasion, from an accidental diminution of acidity, or excess of alkali in the food, this store was swept out into the blood, a headache was the result. The acids in beer and wine, and I suppose the salts in cheese, act in the same way.

And I soon found that by causing retention of uric acid, for a few days, by taking an acid, and then rushing it all out into the blood with an alkali, I could produce the headache at will.

Also, that when uric acid was in excess in the blood and causing a headache, I could drive it out of the blood back into the liver and spleen, and stop the headache, by taking an acid; and I have repeated this experiment many times, both on myself and others, and have also numerous records of cases treated by myself or others, where diet and acids have acted in precisely the same way as in my case, so that I have come

to regard this uric-acid headache as a good starting point from which we may reason to other diseases connected with uric acid.

The first thing that directed my attention to acids was, as I have said, the relation of four-penny ale to gout, as seen among out-patients, and I have previously given notes of the amount of acid in several kinds of ale and stout, and further any one may convince himself of the presence of considerable amounts of acid in all wines and similar beverages, so that there can be no sort of doubt that these beverages will cause retention of uric acid in those who take them, and this will be great in proportion to the amount of acid in the beverage, and the quantity consumed.

Sir A. Garrod ("Brit. Med. Journ.," Vol. I., 1883, p. 653, Lumleian Lectures) has discussed the gout-producing power of wines and beers, and puts it down entirely to the unfermented matter they contain. In speaking of the question, he says incidentally, the light wines, which contain a considerable quantity of acid vegetable salts, do least harm; while the heavy wines, as those of Spain, do most harm, and are almost devoid of vegetable salts.

Again, speaking of acidity, he says that fresh fruits diminish acidity, through conversion of their vegetable salts into carbonates, and this, I think, gives us the key to the whole matter.

From my point of view, wines, ales, and porter may all be regarded as solutions containing a very small amount of albuminoids, a certain amount of alkali or alkaline salts, and a certain amount of acid either free or combined; and it is obvious that if the alkalies, or the vegetable salts which can be converted into alkaline carbonates, predominate, the beverages will act as doses of alkali, while if the acids and acid salts incapable of conversion predominate, they will act as doses of acid, and this consideration explains the difference Sir A. Garrod has noticed between the light and heavy wines, and also why wines that act as diuretics (probably from their containing an extra quantity of potash or its salts) are less harmful than others, also why old and expensive wines are less harmful than new and cheap wines, for one of the effects of keeping wine is a loss of acidity by etherification. Further, I think that these facts give us a much better explanation of the ob-

served action of wines and beers, in the production of gout, than their somewhat hypothetical unfermented matters can possibly afford.

I had worked at diet for a long time before I began to study drugs; but the effects of all the varieties of diet on uric-acid excretion may be summed up by saying, that all foods that increase acidity diminish uric-acid excretion, and all foods that diminish acidity increase excretion. I have not been able to satisfy myself that any food increases the formation of uric acid above its normal relation to urea (1:33).

Foods may increase acidity directly, by the acids or acid salts they contain, or indirectly by causing dyspepsia, and acid fermentation processes in the stomach or intestines (Bouchard). They may diminish acidity by the alkali they contain, or again through dyspepsia; especially if this is accompanied by nausea and vomiting, as these produce a lessened absorption of nourishment and entail fall in urea and acidity.

While speaking of this subject, I must not forget to mention a very interesting table of Lehmann's results which is given by Sir A. Garrod in his Lumleian Lectures, showing uric acid and urea excretion under different diets thus:

	Urea.	Uric Acid.
On mixed diet, ...	32.4	1.1
On animal diet, ...	53.1	1.4
On vegetable diet, . . .	22.4	1.0
On non-nitrogenous diet, . .	15.4	0.7

Sir A. Garrod concludes from this that the amount of uric acid excreted is not nearly so much affected by the nature of the food as the urea is, and that therefore there is no reason why a moderate amount of meat should not be given. But I have shown above that meat increases formation of both urea and uric acid, and that, as its salts increase acidity, it further causes retention of uric acid. So that I believe that there is very considerable harm in giving meat in these diseases. I believe I am able to prove this in the case of headache, and I feel pretty confident of being able to prove the same for other members of the group.

What, then, is the explanation of this table? Simply, I think, that Sir A. Garrod has never noticed, as I have done, the relation of uric acid to urea in the excretions produced

under each kind of diet, or he would have seen that it proved almost the reverse of his conclusions. I think if I put down the table again, and alongside of it, my figures expressing the relation of uric acid to urea, this will be seen to be the case

	Urea.	U. Acid.	Relation.
On mixed diet, . .	32.4	1.1	1:29
On animal diet, . .	53.1	1.4	1:38
On vegetable diet, .	22.4	1.0	1:22
On non-nitrogenous diet,	15.4	0.7	1:22

It may now be seen that what really happened was that, under the mixed diet, uric acid was slightly in excess of its normal relation to urea (taking that as 1:33); that on the animal diet it was decidedly diminished, *i.e.*, the rise of acidity on animal diet diminished the excretion (formation going on steadliy at 1:33); on the vegetable diet excretion was greatly increased, *i.e.*, uric acid previously formed and retained was swept out by the excess of alkali in the diet. I will further point out that, if the animal diet had been continued, the person experimented upon would soon have had a very considerable store of uric acid in his liver, spleen, or other organs, either doing them harm, or ready to be swept out in the "alkaline tide" into the blood, and when there to cause headache, hypochondriasis, epilepsy, or any other disease of this group to which he happened to be liable. So that I am quite unable to believe that in this class of diseases, meat, especially strong butcher's meat, with its large amount of salts, which act as acids, can be taken with impunity. I may add, that the results in the above table correspond in every particular with my own, obtained with similar changes of diet, and the relations of uric acid to urea are just such as I meet with in my every-day work.

I will now give, as shortly as possible, some of the results I have obtained by investigating the excretion of uric acid and urea, over long periods of time.

In 202 days, mostly in the winter, the relation was 1:33 uric acid and urea.

In 40 days, mostly in the spring, the relation was 1:30 uric acid and urea.

In 150 days, mostly in the summer, the relation was 1:29 uric acid and urea.

So that it appears that the excretion of uric acid is relatively greater during the warm months of the year, and I shall have something more to say of this fact when I come to speak of mental depression.

In 31 days, when fish, eggs, and occasional meat were being taken, the relation was 1 : 30.8.

In 213 days, when milk, fruit, and vegetables formed the chief articles of diet, the relation was 1 : 29.7.

Again in 591 days, including the above-mentioned days, there were 6,126 grs. of uric acid, and 186,032 grs. of urea, giving a relation of 1 : 30.3.

These figures refer to periods in which very various experiments, both in diet and drugs, were being made, and I have no doubt that these have influenced the result to some extent; probably making the excretion of uric acid somewhat greater than it would have been under perfectly natural conditions; further, the above results given for winter, spring, and summer, must be taken with some allowance for these sources of error, still I think that there is no reason to doubt that the excretion of uric acid is really greater during the warm months of the year, owing to an increased alkalinity of the blood and tissue fluids due to increased elimination of acids in the perspiration; and the increase of alkali in these same fluids, due to vegetarian diet, has naturally the same effect.

In none of the above results do I see any reason to suppose that uric acid is ever formed in greater proportion to urea than 1 : 33, but where over a long period, as that above mentioned, the relation comes out as 1 : 30, I think that this increase of uric acid is due to its being washed out gradually, and a little at a time, from stores and accumulations in more or less remote parts of the body (as, for instance, in joints and fibrous tissues, which have but little vascularity).

It thus appears that during the colder months of the year there is some retention of uric acid, and in the warmer months some plus excretion, the one variation balancing the other; this variation has, as I shall point out later, some important influence on the mental state and condition of the two periods of the year.

At this point I should like to show, in the form of a curve, the excretion of acid in the urine each hour throughout the day estimated as oxalic acid. See Fig 1.

In this figure there is clearly seen the low acidity of the morning slowly rising up to 12 noon, this is the so-called "alkaline tide" of Roberts, in which it is well known that the excretion of uric acid is greatly above that of any similar period in the twenty-four hours.

From 12 noon to 3 P.M. there is a further rise, and from 3 P.M. to 5:30 or 6 P.M., a marked and decided fall; this is also a very constant feature, and I have often spoken of it as the second or afternoon "alkaline tide."

I may note in passing that those who suffer from uric-acid headache, often have their worst suffering during one of these "alkaline tides."

From 5:30 or 6 P.M. there is a rise till 7 or 8 P.M., and after that often a slight fall, till about 10 P.M., when the acidity is generally maintained at about the same height through the night. The curve given shows too decided a fall at 9 and 10 P.M. owing to exposure to great heat in a close room.

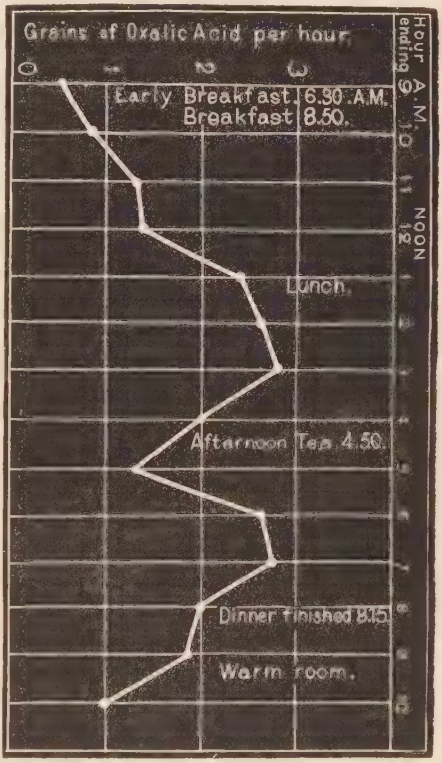


FIG. 1.

The high acidity of the night, which I have previously spoken of under the name of acid tide, is a very constant feature, and just as headache, with a plus excretion of uric acid, is most common during the alkaline-tide periods of the day, so gout with a minus excretion of uric acid is most commonly met with in the acid-tide period of the night. Of course, where, as in one of the accounts previously given, a uric-acid headache lasts through the whole twenty-four hours, the high rises of acidity at 3 P.M., 7 P.M., and during the night do not

occur, because there is some gastric disturbance and dyspepsia which interfere with the absorption of nourishment, causing a fall in urea and acidity, which is low throughout the twenty-four hours.

This figure represents what I take to be somewhere near the normal daily curve of acidity; it was, however, obtained on a lowly nitrogenous diet, so that the absolute acidity is probably less at every point than it would have been in an individual on ordinary diet, but I believe that the fluctuations are probably the same both in time and direction on other diets.

Taking such a curve as a base of operation, it is possible to alter it to any extent, and in almost any direction, by drugs, foods, and drinks previously spoken of, and also by exercise, which, when it increases the perspiration, at once diminishes acidity in a decided manner. Similarly, a hot room or exposure to heat of any kind will diminish the acidity of the urine. Conversely, exposure to cold and damp, by diminishing the excretion of acids in perspiration, raises the acidity of the urine (as noticed by Sir A. Garrod, "Gout and Rheumatic Gout," p. 258). We have here, I think, an explanation of the effects of profuse acid perspirations in rheumatic fever in eliminating acids from the body; we have also an explanation of the effect of exposure to damp and cold wind in producing rheumatism and gout by raising acidity.

So far as I know, the alkalinity of the blood generally corresponds to and fluctuates with the acidity of the urine; high urinary acidity accompanying low alkalinity of the blood, and vice versa; and the observations that I have been able to make with Prof. Haycraft's test-papers (prepared by F. Sutton & Co., chemists, Norwich) for estimating the alkalinity of the blood, as well as the results which he has himself obtained with them, seem to be in favor of this conclusion.

Having thus become familiar with the action of ordinary acids we will now consider that of an acid which has an effect on the excretion of uric acid quite peculiar to itself; viz., salicylic acid.

Shortly stated, the action of this acid, or a salicylate, is to dissociate the excretion of uric acid from its general relation to acidity, so that an increase of acidity, while there is plenty of salicylate in the blood, is not accompanied by any diminution of uric-acid excretion. The great importance of this fact

may be roughly estimated by considering the rôle played by acids in dyspepsia, in gout, and in rheumatism; and if, as I hope to be able to show, salicylates prevent these acids causing retention of uric acid, a large part of their action in these diseases will admit of easy explanation.

Thus of gout, Sir A. Garrod says ("Gout and Rheumatic Gout," Ed. 3, p. 245): "When a few glasses of wine, ale, or porter quickly and invariably produce, in any individual, an inflammatory affection of a joint, such inflammation is of a truly gouty character." The way in which these beverages act in producing the inflammation is now, I think, sufficiently clear. I shall not go further into the pathology of rheumatism and gout, but at once bring forward some of the results of my experiments with salicylates. To take one instance out of many, the relation of uric acid to urea, before salicylate, was 1:37; after it on the following day 1:28. It is here assumed, for reasons previously given, that the normal relation of uric acid to urea is 1:33, so that there was a minus excretion before the salicylate, and a plus after it. It does not appear to have any definite effect on the acidity of the urine, which rises or falls from other causes; but it is at once seen by reference to the curves, from which the above relations are taken, that the uric-acid excretion is not influenced by the acidity in the usual way, for it rises high above the urea, while the acidity remains the same as on the previous day, or again, on another day it falls below urea, though as acidity has fallen we should have expected a rise. So that we see that under salicylate the uric-acid excretion fails to keep up its usual inverse ratio to the urinary acidity; indeed, it almost seems as if the reverse effect were produced, so that a fall of acidity corresponds with a fall of uric acid, and a rise of acidity with a rise of uric acid.

On another occasion, the large excretion of 17.4 grs. of uric acid was obtained by the action of salicylate, in spite of high and rising acidity. Here, the uric acid exceeded its general relation to urea (1:33) by more than 6 grs. It is further to be noted, that this excessive excretion of uric acid was not accompanied by any headache; which it would almost certainly have been if it had occurred apart from the action of salicylate.

From these results, which I have elsewhere ("Med.-Chir.

Trans.," Vol. 71) given in figures of curves, two facts stand out clear and distinct. First, that under salicylate, uric-acid excretion is not kept down by a rise in acidity, but rather the reverse. Second, that a large excess of uric-acid excretion, when it occurs under salicylate, is not accompanied by headache.

Having arrived at this point with regard to the action of salicylates, it was suggested to me by Dr. Lauder-Brunton, to try whether benzoates would have the same effects, and I at once performed similar experiments with these salts. I found that the benzoate of ammonium, which increases acidity, diminishes uric-acid excretion, while benzoate of sodium, which diminishes or does not affect acidity, allowed an increase of uric-acid excretion. The benzoates, therefore, appear to act merely through their effect on acidity, and not as the salicylates do.

My impression is, that this difference in effect may be due to the different solubilities of the compounds formed with uric acid, by benzoates and salicylates, respectively. In the paper just referred to, I have given quotations from Watts' "Dictionary of Chemistry," with regard to the solubilities of uric acid, hippuric acid, and salicyluric acid, which, I think, show that both hippuric and salicyluric acid are far more soluble in water than uric acid, and are also more soluble in water containing hydrochloric acid. Salicyluric acid appears to be in many ways more soluble than hippuric acid; that is to say, its salts generally are more soluble; and the acid itself is more soluble both in water and ether, and probably in weak acids than hippuric.

If we may suppose that, on giving salicylate, the uric acid in the blood and tissue fluids is converted into salicyluric acid, we should have to deal with a substance which, as compared with uric acid, is far more soluble in those fluids, and also more soluble in slightly acid fluids; and if alteration of solubility is the explanation of the action which I have shown that acids and alkalies have on the excretion of uric acid, we have here also a simple explanation of the result I have obtained with salicylates, viz., that a rise in acidity no longer entails a fall in uric-acid excretion.

The same difference in solubility will, I think, also serve to explain the fact that there is no headache when a large excre-

tion of uric acid is going on under salicylate, when as we may suppose there is an excess of salicyluric acid in the blood, this latter substance, possibly on account of its greater solubility, having no irritant action on the vaso-motor system, while uric acid combined with an alkali apparently affects the vaso-motor nerves (Latham) and is also liable to be thrown out of solution by contact with acid, or but slightly alkaline fluids, in any part of the circulation.

We now come to some drugs, the action of which on the excretion of uric acid is in marked contrast to that of alkalies and salicylates of which we have just spoken.

Lead.—An experiment with this metal, the result of which has been given in the form of a curve in "Med.-Chir. Trans.," Vol. 71, shows that on the day before lead was given there was a relation of uric acid to urea of 1 : 36, and on the first day of lead being taken, a relation of 1 : 51, so that there is no doubt that lead produces a retention of uric acid, but if the lead is continued for several days in succession the retention becomes much less, or altogether ceases, because the lead (taken as acetate), causes so much intestinal irritation that after the first day or two there is a fall of urea with a corresponding fall of acidity, and the plus excretion of uric acid, which more than counterbalances the retention produced by the lead, but, till the intestinal irritation is produced, the retention of uric acid is, so far as I know, an invariable result of taking a lead salt.

Iron.—On the day before iron is taken, we find in similar curves a relation of 1 : 32, and on the day when iron is taken the relation of 1 : 43, so that iron, like lead, produces retention of uric acid, and like lead also, if it produces intestinal irritation, the retention at once comes to an end.

Sir A. Garrod has pointed out that the urates of lead and iron are extremely insoluble, and this I suppose is the reason why they produce retention of uric acid in the body, just as we have previously seen that the extreme solubility of products formed is the reason of the plus excretion of uric acid under alkalies and salicylates.

Lithium.—Here we have a drug whose urate has been shown by Sir A. Garrod to be extremely soluble, and yet, my experiments appeared to me to show that it produced retention of uric acid; thus we get, on the day before lithium is

taken (as the citrate), a relation of 1:34. On the first day of lithia 1:40.

When I first noticed this action of the salts of lithia I thought it must be an error, but repeated experiments showed me that the result was invariable. I was then inclined to conclude that this was a fatal blow to the theory of solubility of the compounds formed, being the cause of uric-acid excretion or retention, for, how could a metal, whose urate is the most soluble of all urates, yet cause retention of uric acid?

There could be no doubt as to the great solubility of urate of lithia, and I could see no way out of the difficulty in that direction.

Thanks, however, to Dr. Neale's "Digest," I found a reference to the "Lancet" twenty-nine years ago, which said that lithia was no use as a solvent of uric acid when taken by the mouth, and on looking up this reference I found the following statements ("Lancet," 2, 1860, 185): "Mr. Ure does not recommend the internal use of this salt [carbonate of lithia] in the treatment of vesical calculus, for the reason that it would undergo decomposition and precipitation when coming in contact with ammonio-phosphate of soda, and thereby be rendered inert. Given internally it forms a nearly insoluble triple phosphate with phosphate of soda, or with the triple phosphates of ammonia and soda, salts generally present in animal fluids. Lithia solutions differ in this respect from those of soda and potash." (Rose, "Chem. Anal.," p. 15.)

The insoluble lithia phosphate above mentioned is, I believe, well known to chemists, and if we take the chemical explanation as correct it is easy to see that lithia, taken internally, not only never reaches the uric acid to form a soluble urate, but, in forming an insoluble compound with phosphate of soda, it practically removes from the blood one of the well-known solvents of uric acid, which is generally present there, this accounting, I suppose, for the retention of uric acid it causes and we have here, therefore, no real exception to the rule that the solubility of the compound formed determines the plus or minus excretion of uric acid with any drug.

I have now shown that three drugs, lead, iron, and lithia, when taken internally, cause retention of uric acid, and I now propose to say a few words as to their action in disease.

With regard to lithia, there is not much to say. It was at

one time much used, then fell into disuse, and has finally been brought forward again, in the treatment of uric-acid disease; but at the present day there is by no means a unanimous opinion as to its value; many observers (Ebstein, Sir W. Roberts, Lecorché) doubting its efficacy, or preferring the salts of potash or soda.

With regard to lead, I stand on very firm ground, for Sir A. Garrod has shown that it diminishes the excretion of uric acid, and he has also shown that, given medicinally, it will precipitate attacks of gout.

Dr. Ross ("Dis. of the Nervous Sys.," Vol. 2, p. 915) also speaks of chronic lead poisoning as a cause of epileptic convulsions, and Mr. Pepper ("Lancet," Nov., 1887) makes some interesting remarks on lead as a cause of epilepsy, which he regards as curious, or even mysterious; but I cannot see that there is any mystery about it, if, as I shall have to relate further on, the epileptic attack has in many instances an important relation to the excretion of uric acid; for the lead, by retaining uric acid, furnishes a store which, during its subsequent excretion under alkali, produces some of the vascular or other conditions which are probably the immediate causes of the epileptic attack. With regard to iron, Sir A. Garrod has pointed out that, given soon after an attack of gout, it will cause a relapse, and M. Brown-Séquard (Quain's "Dic. of Med.," p. 450) says, that iron does harm in many cases of epilepsy, so that these two drugs, which are exactly alike as regards their action on the excretion of uric acid, are also exactly alike in their effects on disease.

Their effect in gout is a simple and direct one, they render the uric acid insoluble, and cause it to be retained in various structures in the body, the joints being one of these, and the signs of this retention in the joints are generally present in the shape of certain pricking and shooting pains which correspond, in time, with the diminished excretion of uric acid in the urine, produced by the drug.

There are one or two other drugs whose action is very similar to that of lead, iron and lithia, which I may just mention here, in passing.

Antipyrin.—This is a drug which has been much used in headache, and often with a considerable amount of benefit; my experiments with it show that, taken in sufficient dose to over-

come accidental fluctuations, it clearly and distinctly raises the acidity of the urine, and I think that any one who will watch the acidity from hour to hour, as I have done in Fig. 1, and will then take a good dose of antipyrin, will have no difficulty in obtaining similar results; as the acidity rises, uric acid is kept back and retained, just as it is when the rising acidity is produced by any other cause. In the "Brit. Med. Journ.," Vol. 1, 1888, p. 1,007, I have suggested that the curative action of antipyrin in such headaches as are due to uric acid, is the result of the rise of acidity it occasions, and I have spoken of my results as follows: "I now find that a dose of 20 grs. raises the acidity of the urine within the first hour after taking it, and the rise continues and increases for some four, five, or six hours more, and a dose of one drachm in the twenty-four hours causes a very distinct rise in the acidity of the urine, and a decided fall in the excretion of uric acid." Further, in experiments on myself antipyrin caused the pleasant feeling of well-being which I have noticed in several papers, as one of the results of taking an acid (see further on, Mental Depression).

Opium and Morphia.—These again, like antipyrin, and taken under the same conditions and with the same precautions, raise the acidity of the urine, and cause retention of uric acid; the way in which they bring about this result may be open to some doubt, but my impression is, that it is due to their action on the intestines. I have previously spoken of the way in which dyspepsia and intestinal irritation lessen the excretion of urea, and lower the acidity of the urine, as I believe, by diminishing the absorption of nourishment and the absorption of acid from the contents of the large intestine, and obviously, if opium and morphia delay intestinal peristalsis, they may increase the absorption of food and also of acid from the contents of the large intestine, thus accounting for the rise of the acidity of the urine, and the retention of uric acid which they produce, and, as I have gone into these points at great length in a paper recently read at the meeting of the Brit. Med. Assoc. at Leeds, and shall have to speak further on of dyspepsia, and its effects, I shall not go more deeply into the matter just now. Before leaving opium and morphia I will just say that they, in common with all the substances I have previously mentioned as producing retention of uric acid,

do at the time retention is taking place also produce feelings of happiness and well-being, and at the same time some pricking and shooting pains in the joints, corresponding to retention of uric acid in them.

We now come to some drugs whose action is to cause a plus excretion of uric acid, resembling in this alkalies and salicylates previously spoken of.

Phosphate of Sodium.—This is a very old-fashioned remedy for various gouty conditions, and Sir A. Garrod has pointed out that it is a solvent of uric acid. My experiments with it show that it increases the excretion of uric acid, but they also show that, unlike salicylate of soda, it cannot act independently of acidity, for, though it acts fairly well when given with a small dose of alkali and when there is nothing in the food or circumstances to raise the acidity, a very small dose of acid given along with it, or in sequence to it, or any rise of acidity, however occasioned, is sufficient to prevent its causing any plus excretion of uric acid, and this is, I believe, the reason why it fails to give relief in acute gout, as the acidity in this condition is almost necessarily very high. (See paper on Gout case, "St. Bartholomew's Hospital Reports," 1888, and Trans. of the Med. Chir. Soc., 1889.)

Iodide of Potassium.—When given in doses of 10 grs. and upward, this diminishes the acidity of the urine and causes a plus excretion of uric acid; so far as the plus excretion is concerned, the citrate of potash will probably do as well, but when we come to chronic disease we get, probably, the alterative action of the iodine coming in, and with this, I take it, the excretion of uric acid has nothing to do; so that if a man is suffering from pains in his joints, which are due to uric acid in those joints, a large dose of either the iodide or the citrate would probable relieve his pains; but if he is suffering from chronic structural changes in the joints, not immediately due to the presence of uric acid, then the alterative action of the iodide may be necessary; such, at any rate, are the inferences which I draw from my results.

Colchicum.—This is a drug whose value in acute gout is very well known, and my experiments with it show that it diminishes the acidity of the urine and increases the excretion of uric acid; further, my impression is, that this fall of acidity is due to the intestinal irritation it produces, and I have al-

ready spoken once or twice of this subject; there are some who say that colchicum only does good in acute gout when it purges, and I can easily believe that this is the case, its action in this matter is exactly the reverse of that of opium, and we have seen that it produces the reverse effect; opium causes retention of uric acid, and precipitates an attack of gout; colchicum causes a plus excretion of uric acid, and relieves.

I have now come to an end of all that I have to say at present about the action of drugs on the excretion of uric acid. I have experimented with a very considerable number, besides those just spoken of, but the results are at present either too unfinished for publication, though some of them may eventually prove of very considerable interest, or they are of comparatively minor importance.

To summarize in a few words the main points of interest, I would say that I have attempted to show:

1. That in all uric-acid disease excretion of uric acid is much more important than formation, and that while uric acid is formed and excreted in proportion to urea, 1:33, all may be well.

2. That drugs may be divided into those that increase and those that decrease the excretion of uric acid; pre-eminent among the former stand the salicylates, because acids are powerless to interfere with their action; among the latter, acids are by far the most important, on account of their being so widely distributed in all kinds of food and drink, and of their formation in the intestines, or retention in the body, through insufficient activity of the skin glands.

3. No drug that causes a plus excretion of uric acid has power to make that excretion continue for more than one or two days, that is to say, the store of retained uric acid becomes exhausted, and there is no more for the time being that it can wash out, hence, I believe, that all these drugs affect only the excretion of uric acid, but have no influence whatever on its formation.

CHAPTER II.

As the action of acids on uric acid is, I believe, the same in everybody, and their presence in the body has been seen to be, from one cause or another, almost universal, it follows that everybody must have some retention of uric acid, and this I believe is the case; everybody also must have, at one time or another, some slight excess in the accumulation of uric acid, and as the result of this, must have at some future time, when it comes to be excreted, some excess of it for a time in their blood; but everybody does not suffer from uric-acid headaches, and we are therefore driven to suppose that everybody has not the other factor which is required, viz., a specially sensitive nervous system (Latham, "Brit. Med. Journ.," 1886, p. 629) or, to particularize, a specially sensitive vaso-motor system.

Headache.—We are now in a position to go more deeply into the causation of migraine, or as I prefer to call it the uric-acid headache; I have shown that it is contemporaneous with a large excretion of uric acid, and, I believe, with the presence of a large excess of this substance in the blood; it is also, however, contemporaneous with a very marked alteration in the pulse, which has been noticed by many observers to be slow and hard, and it is shown by the sphygmograph to have all the characters of high tension; further, when the headache has been cured, as by a dose of acid, when the excretion of uric acid has also been diminished in the urine, the pulse alters and will then be found to be quicker, softer, and with the sphygmogram no longer showing high tension, so that in this form of headache we have three series of phenomena which always alter together: the pain, the excretion of uric acid, and the tension of the pulse. It would lead me too far to go minutely into the many theories of vascular and vaso-motor causation of migraine, but there is a large consensus of opinion that the pain is due to some vaso-motor effects in certain regions of the pericranium, especially to spasmodic contraction of certain

vessels (Du Bois Raymond), and if, as I shall shortly have to show, uric acid produces contraction of all the arterioles in the body, it is not unlikely that it may produce spasm of certain vascular areas.

With regard to the action of drugs on uric-acid headaches, practically all drugs that produce retention of uric acid relieve the headache, and they act well or otherwise in proportion to the certainty of their action on uric acid; for instance, when there is great nausea and intestinal irritation, producing a large fall in acidity, it is useless to expect that a small dose of acid will relieve a headache; if its action is observed it will be found that under such circumstancs it quite fails to raise the acidity of the urine, or cause any retention of uric acid, that is to say, it is overpowered by the alkali present; under such circumstances a dose of morphia will probably be the best thing, as it would relieve the intestinal irritation and quiet peristalsis, and thus naturally raise the acidity, cause retention of uric acid, and relieve the headache.

As to diet, butcher's meat, which increases the formation of uric acid and urea, and by contributing to a high acidity causes retention of uric acid, obviously lays up a store which will be the cause of a future headache.

We have thus seen that the headache is very definitely associated with certain conditions of uric-acid excretion and pulse tension; some of its clinical features are also in many cases clear and distinct, and generally render it easy to identify. Among these features, family history is often an important landmark; the presence of gout, "rheumatism," or headache being very common in it. The headache itself has also some important distinguishing characteristics: it is generally present in those who suffer from it, on and off for a very large number of years, or the whole of life, and it often begins early in childhood; it generally recurs once every week or ten days, or from that to once a month, but, and this I think is an important point, it does not often last for more than twelve to thirty-six hours continuously, and if we bear in mind its causation by the passage through the blood of retained uric acid, it is not difficult to understand that it should be so, for obviously the store of uric acid will after a time become exhausted. It also comes on, or is more severe, at those hours of the day in which excretion of uric acid is naturally greatest.

Dyspepsia.—I hope that by saying a few words here on this subject, and its relation to headache, I shall not be obliged to return to it except by name, further on.

To estimate properly the relation of dyspepsia to headache, we must imagine that for several days the patient has been feeling extremely well, eating heartily and displaying great bodily and mental activity in his occupation; he will thus have been forming a large quantity of uric acid and urea, and owing to high acidity some of the uric acid will have been retained in the body; sometimes I believe that this uric acid directly irritates, or by other means interferes with, the circulation in the liver and spleen, and so in the chylopoietic viscera; in this way bringing about congestion of the organs and catarrh of the stomach, this in turn produces dyspepsia and diminished appetite and nausea, and these again are the cause of a fall in urea and acidity, and then comes the plus excretion of uric acid and the headache, and the working of this vicious circle explains the periodicity of the headache and gives us a reason why it tends to recur every week or ten days (see Table of Excretions previously given).

With regard to the treatment of headache, my impression is that the only treatment which promises any permanent success is that by diet; we have seen how butcher's meat, by laying up a store of uric acid, accumulates the material for a future headache, and it is obvious that a vegetarian diet, with which there will not only be an absolutely diminished formation of uric acid, but also a free excretion of all or nearly all that is formed, will best reverse the conditions which do harm in the diet containing butcher's meat; and I have had a very considerable number of cases in which diminution of, or even complete abstention from, butcher's meat has produced great and permanent reduction in both the frequency and severity of the attack, so that a patient may come to have one or two headaches in the year, in place of the same number or more, in a week or month.

Drugs are, I think, of use only at the time of the attack, either to relieve the pain, as may be done by a dose of acid or opium, or a hypodermic injection of morphia, or if there is any warning before the headache and nausea are severe, a dose of salicylate of soda may prevent the attack from becoming severe, and if continued for one or two days afterward, this

drug may completely prevent any return of the pain for some little time.

Nux vomica is also in some cases a very useful drug, taken as recommended by Dr. Ringer ("Handbook of Therapeutics," Ed. 12, p. 512), 1 minim every 10 minutes, till 10 or 15 have been taken, and this will, in my experience, often relieve or cure a headache in a satisfactory manner. Its mode of action is, I think, probably an indirect one, and due to its effect on the stomach; for where, as is so often the case in these attacks, there is much tendency to nausea, it seems to combat this condition and promote appetite and the absorption of food, thus indirectly raising acidity and driving the uric acid out of the blood; it is possible also that it may have some action on the vaso-motor centre, and thus prevent some of the effects of uric acid on the vessels of the general circulation or the brain.

Pulse Tension.—I have previously spoken of the high tension of the pulse, which accompanies the uric-acid headache, and in a paper, in the "Brit. Med. Jour.," Feb., 1889, I have suggested that excess of uric acid in the blood, possibly by acting on the vaso-motor nerves and causing contraction of all peripheral arterioles, may be the cause of the high-tension pulse, and have further said that I believe that I am able by influencing the excretion of uric acid to influence the tension of the pulse in either direction; thus all drugs which cause retention of uric acid and drive it out of the blood, diminishing its excretion in the urine, do at the same time lower arterial tension, and conversely an alkali, which increases the excretion of uric acid, will at the same time raise the tension of the pulse; it is quite possible in both cases to follow all the changes that occur by testing the hourly excretion in the urine and taking frequent tracings of the pulse, or, as I have done more recently, taking the amount of acid secreted in the urine every ten or fifteen minutes, together with the corresponding pulse trace, taking for granted, what I have no doubt is true, that the excretion of uric acid follows the acidity in its usual relation. I have also been able to get, in a very considerable number of patients, practically similar variations in the tension of the pulse by the use of acids, opium, or other drugs affecting the excretion of uric acid.

I will now give a few figures of pulse traces to illustrate

some of the points I have been speaking of. In Fig. 2, A is the pulse of a patient suffering from chronic nephritis, and having also some headache and threatenings of fits (uræmic?); B is the same pulse an hour and a half later, after two doses

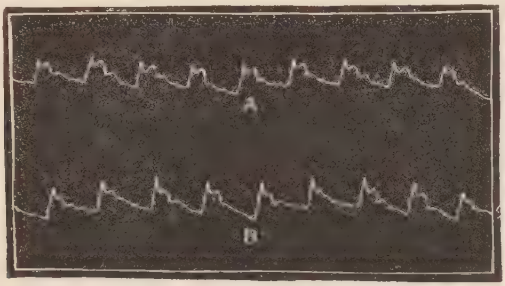


Fig. 2.

of 10 minims of dilute nitro-hydrochloric acid, and I think it shows that there was some decided difference in the tension, the patient also felt better, and considered that the headache and threatenings of the fits had gone off. I may here say that this patient was afterward taken into hospital, and that acids and opium were on several occasions found to relieve

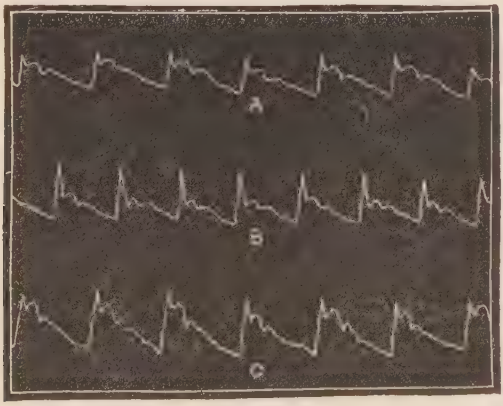


Fig. 3.

her symptoms, lower her pulse tension, and quicken her pulse; and her nephritis improved and the albumin diminished under the same treatment, so that this is probably one of those cases in which opium does good in nephritis and uræmia. In Fig. 3,

A is a tracing of my own pulse during a slight headache. B is the same two hours later, when the headache had been removed by acids, and the difference between them, though not shown quite so well in the figure as in the original trace, is sufficiently distinct. The first wave in A has not been made quite as large as it ought to be, and the second wave is a trifle too large. C is the pulse of a patient suffering from chronic interstitial nephritis, given for comparison with A, which it at times somewhat resembles. Fig. 4 illustrates the effect of opium on pulse tension; A is a trace of my own pulse, showing some little plus tension, and B is the same two hours later, after a dose of 10 minims of tr. opii; the difference between them is fairly well marked, the first wave is in A much the most important. In B it is little marked, while the second

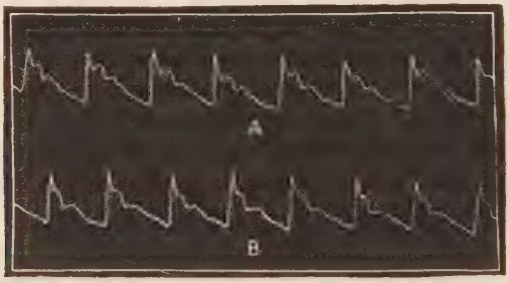


Fig. 4.

wave is large. At the time A was taken, the mental condition was one of lethargy and depression, when B was taken this had given place to mental activity and feelings of well-being. Fig. 5 refers especially to the pulse in various mental conditions, and is practically a further illustration of the points in Fig. 4. A is the pulse of a patient about 50 years of age, who suffers from mental depression. Here again the first wave has not been represented quite large enough, and the second wave was therefore relatively too large. B is my own pulse at a time of mental depression, the condition being more marked than in A. Fig. 4. C is my own pulse, at a time of mental brightness and well-being, occurring naturally, or as the result of some food or other conditions, more or less accidental. D is the pulse, also in myself, of a period of mental brightness and well-being, produced by antipyrin, and it is not very unlike C, which is due to acids, or B of Fig. 4, which is due to opium.

In my own person I have never experienced a feeling of mental brightness and well-being without having at the same time a pulse like that in B of Fig. 4, or C and D of Fig. 5, nor, on the other hand, have I ever had headache or mental depression without also having a pulse like B in Fig. 5, or A in Fig. 3, and I have observed these changing conditions on myself on such very numerous occasions as to leave in my mind no room for doubt that they are constantly associated; and all my experience with patients suffering from mental depression or uric-acid headache supports this belief, but in cases of depres-

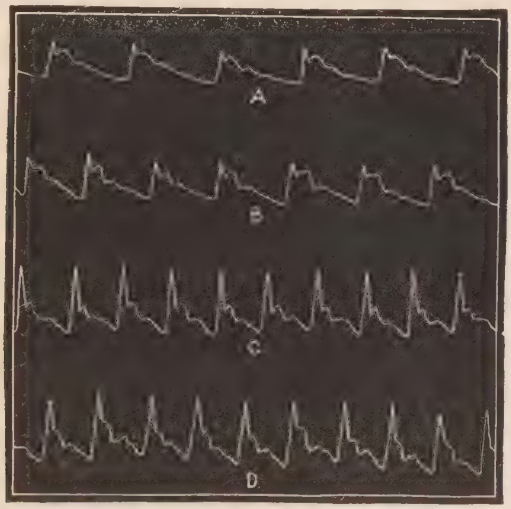


Fig. 5.

sion which gave no signs of uric acid there has also been no high tension of the pulse.

Mental Depression.—Soon after I discovered the relation of headache to the excretion of uric acid, I had my attention directed to certain mental conditions which generally accompanied or preceded the headache; these were of the nature of heaviness, depression, mental dulness, and inability to make any mental efforts quickly and satisfactorily, and I soon came to be practically certain that this mental condition was not accidental, but bore just as constant a relation to the excretion of uric acid and the tension of the pulse as did the headache itself. In the Croonian lectures of 1887, Dr. Broadbent made some

very interesting remarks, which seemed to me to bear on this matter. Speaking of the pulse of melancholia he says, "an unbroken series of gradations may be traced from the irritability and depression of spirits attending functional disorder of the liver up to complete melancholia with delusions." He also says that protracted high arterial tension is a very constant symptom of these cases, and he considers that this is, if not the cause, at least the index of the condition on which the mental state depends. Here we have at once both the conditions I have observed, the mental depression and the high arterial tension, accompanied also, as I can show, by a plus excretion of uric acid in the urine, and due, as I believe, to an excess of that substance in the blood. Dr. Broadbent's reference to functional disorder of the liver may, I think, be read alongside of what I have previously said about the action of dyspepsia in the causation of headache.

In the same lectures Dr. Broadbent speaks of other mental conditions associated with high tension of the pulse and their cure by milk or farinacous diet, and of one patient he says, "When the tension was high she was suspicious, abusive, violent and unmanageable. When it was normal she was cheerful and tractable."

Since my first observations on mental depression I have made almost as many experiments in connection with it as with headache, and I have now no practical doubt that it can be produced or removed, just like the headache, by influencing the excretion of uric acid, which we have seen will influence at the same time arterial tension; and my experience with patients has served to show me that the mental depression which is due to uric acid bears just that relation to gout which its dependence on the excretion of uric acid would lead one to expect, that is to say, one never meets with acute gout and mental depression in one patient at the same moment. The uric acid cannot be in two places at once, and when it is in the joint causing inflammation and pain, it cannot be in the blood, producing high arterial tension, mental depression, and headache. Further, in confirmation of this there are not a few cases on record in which mental depression or melancholia has come to a sudden termination in an attack of gout. Here again, when the uric acid was driven out of the blood into the joints (probably by rising acidity), the effects of its presence in the blood came to an end.

Mental depression and absence of mental depression are not the only results which can be produced by influencing uric acid and arterial tension, for just as it is possible to produce a fit of the blues by bringing a large quantity of uric acid into the blood with an alkali, it is, on the other hand, not difficult, by completely driving uric acid out of the blood by a strong dose of acid, by opium, morphia, antipyrin, and other drugs, to produce feelings of well-being and mental brilliancy in more than ordinary degree, and I have suggested in my paper on opium and morphia, previously mentioned, that the happy feelings and mental brilliancy which lead so many to take these drugs as a daily habit, are due to their action on the excretion of uric acid, the tension of the pulse, and so, I believe, on the cerebral circulation and the mental condition

It seems to me probable that the contraction of the arterioles by uric acid may be quite sufficient to make such difference in the cerebral circulation, and so in its function, as will account for the condition of happiness and mental brilliancy on the one hand, and despondency, mental inactivity, and dulness on the other. It is obvious, however, that other things besides uric acid may affect the cerebral arterioles in a similar way, thus morbus cordis may produce similar effects as regards the nutrition of the cerebral cortex, or again malformation of vessels, or their disease and obstruction, or the external pressure of tumors and growths may produce similar results, so that I am far from claiming that all mental depression is due to uric acid; indeed, I have seen not a few cases in which it was due to some other condition, still the mental depression of uric acid, just as the headache due to the same cause, has certain signs and symptoms by which it may be generally distinguished, thus there is the family history of headache, gout, rheumatism, or minor uric-acid troubles, there is the fact that the depression is not constant, or at least is markedly worse at those times of day, and under the conditions which generally lead to an excess in the excretion of uric acid, and as regards treatment, acids or opium soon produce a marked effect on the mental depression and the pulse tension, so that the functional origin of the depression cannot very long remain in doubt, and I have had patients under my care who, coming for mental depression, or a uric-acid headache, have these cured by acids, with the concomitant production of an attack of

gout, or again, patients who come with gout, and are treated with alkali, suffer from depression or a headache and this not once, but half a dozen times; but I have never seen a patient who complained of gout in the toe, and at the same time of headache or mental depression. My previous writing on mental depression and the excretion of uric acid will be found in "The Practitioner," Nov., 1888, and since it appeared I have heard from Prof. C. Lange (Prof. of Pathology in the University of Copenhagen), that he had noticed the connection of a certain form of mental depression with the uric-acid diathesis, and had therefore been much interested in my writings on the subject. His paper, "Om Periodiske Depressionstilstande," Copenhagen, 1886, is founded, he says, on the observation of a large number of cases in which he has been able to get good results by the use of a somewhat vegetarian diet and a considerable amount of exercise; strong wines and beer being forbidden. As regards diagnosis, he lays great stress on the periodicity of the depression which is connected with gout, and considers that it is quite a different matter from melancholia with delusions. There is a good deal to be said for this distinction, and I have mentioned it above, but the cases spoken of by Dr. Broadbent, in the lectures I have previously quoted from, and the fact that not a few cases of melancholia have been recorded in which all the symptoms promptly cleared up on the supervention of an attack of gout, show, I think, that true melancholia, with delusions, has sometimes, at least, an important relation to uric acid. Prof. Lange considers that regular and sufficient exercise, as by walking, riding, rowing, or gymnastics, is an important part of the treatment; and this is the case as I know with several other troubles which are due to uric acid; and even the uric-acid headache may be driven away by persevering in exercise, which, though it makes the pain worse at first, eventually relaxes the arterioles and removes the headache or depression. A hot or Turkish bath may also relax the arterioles and remove the depression: but exercise not only relaxes the arterioles for the time, but also improves digestion and prevents dyspepsia.

A further point of interest, I think, is to be found in the greater frequency of mental depression at certain seasons of the year, especially the warmer months, as July; and looked at apart from the uric-acid hypothesis it appears a somewhat

strange thing that mental depression should be greater in the brightest and warmest month of the year.

I have previously pointed out that there is probably some relatively minus excretion of it in the cold months (from the effects of temperature on the skin excretion and so on the acidity), and in the paper on mental depression previously referred to, I have drawn attention to the fact that statistics show that in France there are 790 suicides in July, as against some 550 or 600 in each of the other months, and have suggested that the plus excretion of uric acid and the consequent mental depression may account for this; and I can answer for it that the mental depression from which I suffer a good deal is much worse in July than in any month in the year. A further instance of the dependence of mental depression on the excretion of uric acid is to be seen in the very marked manner in which it is associated with the alkaline tide of the morning. Ask a case of uric-acid mental depression when it is worst, and he is almost certain to say in the morning after breakfast. Further, there are plenty of people who would not own to mental depression who yet manifest very clearly the effects of the excretion of uric acid in the alkaline tide; thus, look at a middle-aged high-feeding man, who is fond of his dinner and his wine, and notice after breakfast as he sits in his carriage or train, how he nods over his paper, how he yawns and stretches and even perhaps lapses into a snore, and ask him further as to his mental condition, and he will perhaps be able to tell you that attention to business is at this time an effort, and can only be done after great stimulation and rousing, till a few hours later the alkaline tide comes to an end and he is himself again. The same thing is seen in the alkaline tide of the afternoon (see Fig. 1) after a heavy lunch, when mental work for a time may again become a labor, and these changes occur day after day in numbers of men who are otherwise quite well and have a good night's rest, who would not acknowledge to mental depression, but yet show very clearly by the above signs the effects of uric acid in their blood. And probably on close questioning it might be got out that they took a more gloomy view of matters at those times than at others. The mental depression of uric acid is, however, so insidious that I have myself, though fully aware of its effects, been sometimes led to believe that my affairs were really go-

ing all wrong, it only very gradually dawning on me that it was only my old friend, uric acid.

Before I leave the subject of mental depression, I will just mention one more point of interest with regard to the action of acids on the feelings and mental condition. In the "Prager Med. Wochenschrift," 1889, No. 28, Dr. G. Hofmann has written a paper on some of my researches on uric acid, and speaking of the subject of this action of acids on the mental condition, he adds the following very interesting note. "Es ist ganz merkwürdig dass in Westböhmen, vielleicht auch anderswo, das Volk den sauren Speisen einen günstigen Einfluss auf Gemüthsstimmung zuschreibt; wenigstens ist dort das Sprichwort: Sauer macht lustig, gang und gäbe," which shows, I think, that the mental effects of acids had long been known to the inhabitants of Western Bohemia, and probably not to them only, for I believe that all drinks, whether wines, beer, or non-alcoholic beverages which are popular in England today, are mostly extremely acid, and do produce mental brightness and well-being; the result so often attributed to them, of cheering the spirits, being, I think, due more to the acid than the alcohol they contain: though both probably promote a free circulation in the brain.

Epilepsy.—In investigating cases of uric-acid headache, I was not very long in coming across some sufferers who had both headache and epilepsy ("St. Bartholemew's Hosp. Reports," Vol. 23, p. 210), these diseases occurring either together or alternately, sometimes a headache, sometimes a fit, and sometimes a fit in the middle of a headache; and on looking up the matter, I found that Dr. Liveing ("On Megrim and Sick Headache," Churchill, 1873) had written a good deal on the relationship of megrim to epilepsy and at p. 205 he says: "To begin with epilepsy. This is doubtless the particular neurosis which exhibits the closest connection with megrim, both in the occasional replacement of the one affection by the other, and also in the occurrence of cases of a character intermediate between the two." He also quotes from numerous other observers to the same effect, notably from Dr. du Bois Raymond, and his hypothesis that migraine is due to a tetanus in the muscular walls of the vessel of the affected part of the head, and further refers to what Du Bois Raymond says as follows: "If Kussmaul and Tenner's doctrine is right, which places the

origin of many epileptic seizures in a spasmodic constriction of all the arteries of the head, then my migraine would be distinguished from this kind of epilepsy less by the nature of the disturbance which prevails in it, than by its degree and extent." It appears to me that if the headache is due to a spasm in certain vaso-motor regions, and some cases of epilepsy are due to spasm in certain other neighboring regions, and if, as I have suggested, the spasm in the case of headache is due to the irritation of uric acid in the blood, it is but a small step to suppose that the epileptic spasm may be due to the same cause.

After observing this relationship I began to examine the excretion of uric acid in cases of epilepsy, and in "Neurologisches Centralblatt" (No. 5, 1888) I have given the results obtained in one of the first cases I was able to observe, and these show that before the fits began uric acid was to urea as 1:50, at the time of the first fit it rose to 1:38, between the first fit and the second it fell 1:40, at the second fit it rose to 1:33, and fell again after it, and later in the same day the urine excreted during a series of very severe fits gave a relation of 1:20, falling again to 1:32, or nearly normal, when the fits ceased. Since that I have been able to observe several cases, and wherever I could get the urine of the fit separated with sufficient accuracy from that excreted before and after it, I found a plus excretion of uric acid. The reason why such great care is necessary to separate the urine of the fit may, I think, be seen from the figures I have just given, for as the uric-acid excretion was much below normal before the fits, but much above normal during their continuance, if the urine of the two periods were mixed the plus would balance the minus, and no result would be apparent (it may be remembered that I met with a similar difficulty in estimating the correct relation of the excretion of uric acid to headache). There is this further point of interest with regard to the minus excretion of uric acid that precedes the fits, that fits have been observed (Ross, "Diseases of the Nervous System," Ed. 1, Vol. 2, p. 916) in many cases to be preceded by a condition of mental brilliancy and well-being, which, after what I have said on mental depression, will easily be understood to depend on the complete absence of uric acid from the blood which accompanies the period of minus excretion preceding the fit, so that here again we have many symptoms linking themselves together and tending to show

that the relation of the epileptic fits to the excretion of uric acid is not a mere coincidence. The same thing also has been noticed by sufferers from uric-acid headache, as several of these have placed on record (and I have observed the same thing myself) that a severe headache is often preceded by a few hours of unusual mental brilliancy and well-being, and thus the parallel between uric-acid headache and uric-acid epilepsy appears to me to be very complete.

Here again, as with headache and mental depression, I do not claim, or think of claiming, that all epileptic fits are due to uric acid, and indeed, other causes for them are sufficiently well known, but I do think that some fits are due to uric acid or, perhaps, rather to its effects on the pulse tension and the cerebral circulation, and there are, I think, a good many observers who believe that arterial tension is raised at the time of the fit in many cases. Again I think it is quite possible that even where there is an organic cause for the fits the vascular changes produced by uric acid may precipitate the attacks, the thickening of membranes or the pressure of a new growth may be practically constant, and only when the effect of these are for a time intensified by the vaso-motor effects of uric acid, do we get a fit. Epilepsy has in family history and concomitant symptoms many of the marks which serve to distinguish mental depression and headache, due to uric acid; thus, gout and headache are often prominent among ancestors, and dyspepsia is a very common occurrence among epileptics, even if it be not the absolute cause of the uric-acid storm and the fit; some epileptics, indeed, remark that the eating of certain indigestible articles of diet is sure to be followed by a fit, and taking uric acid as the cause, the sequence is not difficult to explain; then again reedy nails, some amount of high tension of the pulse, or the signs of this high tension in the heart or vessels, are not uncommon in epileptic patients.

Taking this view of the causation of epilepsy it is now not difficult to understand why lead should have got the credit of producing epilepsy, and why iron should do harm in those subject to it; both these drugs cause retention of uric acid and lay up a store of it in the body, which at the end of a few hours, or days, is swept into the blood and produces its evil effect. With neither iron nor lead is the retention, as we have seen, permanent; it lasts for a few hours, and then gives place to a

normal excretion; further, in such cases of lead poisoning followed by epilepsy, as I have been able to observe, the fit did not come while the patient was about, and fairly well; but after he had come to hospital and was ill and generally upset, as regards his digestive processes; so that here again we have the action of dyspepsia in sweeping out retained uric acid and producing epilepsy, headache, mental depression, or all three.

The treatment of epilepsy may, I think, throw some further light on its causation, for it has long been known that some cases of epilepsy improve very greatly or may even get completely rid of their fits on a farinaceous diet, and Dr. Broadbent, in the lectures previously mentioned, has pointed out that cases of epilepsy in which there is high tension of the pulse, are especially benefited by this diet. With regard to drugs, I at one time thought that much good might be done in uric-acid epilepsy by the use of a drug, like salicylate of soda, which would provide for a constant free excretion of uric acid, while preventing it from acting on the arterioles and causing headache or epilepsy, and to some extent this was true; but in a disease like epilepsy, which is, so to speak, always on the look-out for its opportunity, it is impossible, even with salicylate of soda, to keep the blood so free from uric acid that there shall never be a little of it, which, escaping from the influence of the salicylate, may come under the influence of an alkali and so effect arterial tension, and produce bad results. And while salicylate of soda has appeared, in some cases, to do very great good for a little time, it has never resulted in anything like a cure; but just the same might be said of salicylate of soda as a cure for the uric-acid headache, for though, while its influence is kept up, and all the uric acid in the blood is combined with it, there is no pain, there is no surer method of getting a headache than that of taking on one day, say 20 to 30 grs. of salicylate of soda, which brings into the blood a large amount of uric acid, some only of which will be excreted, the rest, however, on the following morning, when the influence of the salicylate begins to fail, will come under the influence of the ordinary alkaline tide, and a headache will be the almost certain result, and indeed, this is one of the most certain methods of producing high arterial tension and a headache. Now, if the same thing were to occur in a patient subject to epilepsy, I should expect that a fit would be the result, and this explains

the comparatively small success which has, so far as I know, attended the use of salicylate of soda in this disease. Drugs that appear to me at the present moment to offer more hope of success at the time of the attack of epilepsy are those which, like morphia, produce retention of uric acid, and it has occasionally been used with some success in cases of epilepsy; and nitro-glycerin, which also, I believe, raises the acidity of the urine, and causes the retention of uric acid, but has the great advantage as regards epilepsy that it acts with extreme rapidity and produces either directly or indirectly a marked fall in arterial tension, and the use of this drug in epilepsy has, I notice, recently been attracting some attention. Taking it, however, that some cases of epilepsy are due, in the way I have attempted to point out, to uric acid and its effect on the vessels, I have no expectation that drugs, other perhaps than bromides, which quiet the centres and prevent their reacting to the irritant, will ever contribute very much to the cure of epilepsy; the most I should expect of them would be, that in certain cases, where there was some warning association with headache, or other signs by which the onset of the fits could be foretold, they would perhaps keep them off or render them somewhat less severe; but I do think, in properly selected cases and in patients who are both able and willing to follow prescribed rules of diet, a farinaceous or milk diet, of a more or less pronounced character, may produce great and lasting benefit.

I think that at this point it may be worth while to stop for a moment and just review shortly the physiological changes which can be produced by influencing the uric acid and its amount in the blood. On the one hand we have a plus excretion of uric acid under alkali, a low acidity of the urine, high alkalinity of the blood, relatively high arterial tension and slow pulse, cold surface and extremities; deficient circulation in the brain cortex, causing depression or melancholia, and possibly, in addition, spasm in various vaso-motor areas causing headache or epilepsy. On the other hand, we have minus excretion of uric acid in the urine under acid, high acidity of the urine, low alkalinity of the blood, reduced arterial tension, and quickened pulse, warm surface and extremities, active circulation in the cerebral cortex, causing mental brilliancy and a feeling of well-being. Further, if there was much uric acid

in the blood at the time of the rise in acidity, we shall have various amounts of shooting pains in joints, up to an acute attack of gout. Such a sequence I have seen hundreds of times in myself, and have come across various portions of it more or less perfect and complete in a very large number of patients.

There is one little point which I think of some interest as a corroboration of my views on the effect of uric acid on pulse tension, and I may perhaps as well mention it here. A good many people, I think, must have noticed that in conditions of dyspepsia the pulse will occasionally be found to intermit in a patient who has no obvious signs of heart disease, and whose pulse, when in his ordinary health, is quite regular, and I have noticed the same thing in myself, and have several pulse traces in which this intermission is shown; and it is remarkable that out of a very large number of pulse traces this intermission shows itself only in those in which there is high tension, so that I have come to regard this intermission, not as a nervous phenonemon, but as a flutter or failure of the heart before rising tension. I will here mention what I consider to be a very interesting passage in the works of Abernethy ("Surgical Observations," p. 233, Ed. 3, "On the Constitutional Origin of Local Disease"), speaking of his own symptoms, he goes on to relate how one day after considerable fatigue, he was seized with pain and a sensation of coldness in the region of the stomach, he had no appetite, and the biliary secretion was suppressed; he also relates that while this disorder continued his pulse intermitted very frequently and he was distressed with hypochondriacal sensations, but that upon an alteration in the state of the digestive organs, and a renewal of the biliary secretions, which happened quite suddenly after taking five grains of mercurial pill, his pulse became perfectly regular, and his mind tranquil. Here we have an almost absolutely complete picture of a uric-acid storm; the uncomfortable feeling and coldness in the region of the stomach, corresponding, I believe, with some gastro-hepatic congestion, followed, no doubt, by minus absorption of food, a fall in acidity, and a flood of uric acid in the blood, causing a pulse of high tension, which accounts for the intermission, and also for the distressing hypochondriacal symptoms; the absence of bile was probably due to catarrh of the duodenum and ducts, and the mercury,

in promoting the excretion, relieving the catarrh and congestion, soon brought about more vigorous digestion, and the rising acidity, even if the mercury itself did not directly affect the uric acid (see further on the effect of mercury on metabolism in the liver), then drove uric acid out of the blood, reduced the tension, and thus the pulse became regular and the mind tranquil.

I am indebted to Dr. Gee for the reference to these very interesting remarks, and the history which Abernethy here gives of his own symptoms. It will, I think, be seen that the action of uric acid on the pulse and the mental condition, as I have observed it, enables us to explain nearly every symptom he mentions.

Gout.—From what has been previously said, a gout attack appears to be a tolerably simple matter, and need not detain us very long; practically I believe that anything which drives uric acid out of the blood and into a joint will produce an attack; and Sir A. Garrod has pointed out ("Gout and Rheumatic Gout," p. 292) that the cartilages and ligaments of joints are less alkaline than the blood and tissue fluids in general, so that, if the uric acid is retained in the liver and spleen because they are less alkaline than other organs and tissues, it may be retained in the fibrous tissues and cartilages of joints for a similar reason; further, the joint structures differ from the liver and spleen in being but lowly vascular, so that changes in the blood and circulation will not affect them as soon as the liver and more vascular organs, and thus, while uric acid may after a time be deposited in the joint tissues, it will, when there, be but slowly influenced by the changes in the blood and will only be removed by slow degrees.

The most common way in which uric acid comes to be driven out of the blood and deposited in the joints is by a rise of acidity, but any drug which diminishes the solubility of uric acid, as lead or iron, will act in the same way, so that in simple terms a gout attack is a retention of uric acid in a joint, and the cure of that attack is the washing of the uric acid out of that joint by salicylate of soda, alkalies, or colchicum, which I think acts as an alkali. In "St. Bartholomew's Hospital Reports," Vol. 24, p. 217, I have given notes of a case in which I followed the excretion of uric acid and urea through several attacks; in all of them, when the pains were increasing and

the temperature rising, there was a minus excretion of uric acid in the urine, that is to say, it was being retained in the body, and on all occasions when the pains were diminishing and the temperature falling there was a plus excretion of uric acid, which was brought about for the most part by the action of salicylate of soda. These attacks, or the rise of acidity which occasioned them, was in one case due to a diet containing meat and stout, in the other cases to phosphate of soda, about the action of which I have already spoken, and in one last attack it was possibly due to a preparation of iron which was being taken, aided by constipation, which was troublesome throughout. Of the action of constipation and its effect in causing a rise of acidity, I have already spoken in discussing the action of opium. Practically I think that all drugs which cause a retention of uric acid will, in a gouty patient, precipitate an attack, but only when they do produce a retention of uric acid; if anything prevents their action, as, for instance, the presence of much alkali which will overpower an acid, or the presence of dyspepsia and diarrhœa, which may overpower anything but a very large dose of opium, then there is no diminution of uric acid in the urine and no attack of gout results; so that in estimating the result with any given drug, it is necessary to bear all the facts in mind, and not to be misled into attributing a failure to the drug when it is really due to other conditions present.

With regard to drug action in any given case, there is further to be borne in mind that absorption may be interfered with, as during certain conditions in attacks of headache, or epilepsy, and that drugs put into the stomach may remain there for hours untouched. That such is the case may easily be understood by any one who observes the phenomena of an ordinary dyspeptic attack, a patient perhaps takes a meal at 7 or 8 P.M., probably not feeling very well, but having no actual vomiting; several hours later, as in the early morning, he may very probably vomit this meal in a completely undigested condition. I had an extremely interesting instance of something of this kind in a case of epilepsy which I was treating with salicylate of soda, as on several occasions the urine passed during the fit gave no reaction with perchloride of iron, though that passed after the fits gave a strong reaction without any more of the drug having been given. The fits in this case were

often accompanied, just as in the case of headache, by a certain amount of nausea, and I believe while this was present there was no absorption of the salicylate lying in the stomach, and the supply in the blood and urine soon failed, thus accounting for the absence of reaction with iron. Some people who have noticed some similar facts with regard to epilepsy, have sought to explain them as the result of a failure of execretion during a fit, but there is, I think, nothing to support this hypothesis, as I have shown that the urea excretion goes on during the fit much as usual, while the uric-acid excretion is considerably in excess of the normal; both these substances come from or through the blood, why should not the salicylate do the same if it is there? My impression is, as I have said, that it is not there because it has not been absorbed from the stomach.

A good deal has been said lately on the subject of the treatment of obesity by a diet of lean meat and hot water, and several people have asked me whether such a diet would not be very harmful in gouty cases. Their view seemed to be that, if a large proportion of meat with a mixed diet does great harm in many diseases connected with uric acid, a pure meat diet should do more harm. I may say that this view commends itself to me as little for its logic as its physiology; first of all it does not follow that, because a large proportion of meat in a mixed diet does, as I firmly believe, great harm in gout, a pure meat diet will do more harm; secondly, physiology furnishes us with several instances in which articles of diet, not in themselves harmful, yet do injury when combined with certain other articles. To mention one instance, a great many people know that a very limited quantity of fat and butter taken along with an ordinary mixed diet, will cause dyspepsia and biliousness, but I know, and have heard several other people when making a change of diet remark the same fact, that on a milk or farinaceous diet, butter and fat can be taken to almost any extent without harm; and the explanation of this is not, I think, difficult; for Parkes, in his work on the urine, p. 58, tells us that fat prevents the oxidation of nitrogen, so that it is easy to understand why it should do harm when taken with much nitrogenous food, but not when taken with a vegetarian diet. It seems to me very probable that the fatty and starchy elements in an ordinary mixed diet may interfere with the metabolism of the nitrogen in the meat,

and thus do harm, but it does not follow that meat alone will do any harm. As regards uric acid, I will go into this matter a little more fully. In my paper in the "Journal of Physiology," previously referred to, I have spoken of the action of acids in driving some of the uric acid in the blood through the liver, and causing it to be there converted into urea; and I believe that under certain conditions, when the liver metabolism is extremely active, all the uric acid that finds its way into the blood may be thus converted into urea, and I think also, that mercury and perhaps some other drugs which stimulate and quicken metabolic processes in the liver, do for a time cause such reconversion of uric acid into urea. Looking at the matter from this point of view, it seems to me that if the diet of lean meat does, as those who use it assert, stimulate the entire metabolism of the body, one of its effects may be that all uric acid which overflows from the kidney into the blood, and under ordinary circumstances might be stored as uric acid in the liver and spleen, is under these circumstances converted into urea during its passage through the liver and so completely got rid of, so that with a pure meat diet we might have really less uric acid to deal with, and less stored in the body than with an ordinary mixed diet. With regard to this point, my object in all that I have written has been to point out that people on a mixed diet generally form uric acid in the relation to urea of about 1:33, and that those of them who are gouty are so, not from an increased formation, but from a failure in the excretion of this uric acid; but I am not prepared to say that there may not be, under certain conditions and as a result of the action certain drugs, what practically amounts to a lessened formation of uric acid below the level of 1:33; for though the uric acid may be originally formed in about this relative proportion, if any part of it which escapes immediate excretion is at once converted into urea the result is the same as regards uric-acid disease as if there had been a lessened formation.

I have previously given my results showing the excretion of uric acid over long periods, these results ranging from a relation to urea of 1:30 to 1:33. In the table from Lehmann previously given, the relations run from 1:22 to 1:38, but I have given my reasons for thinking that these are probably variations in excretion; I have got just such variations in my

daily work, and indeed can cause them at pleasure, but taken over a long period the result comes out about 1:33, which is, I think, a rough guide to the real relative formation of the two substances. Further, MM. Yvon and Berlioz have given ("Rev. de Méd.," Sept., 1888) relations of 1:30 or 1:40 as the results of two separate sets of investigations, and my 1:33 is intermediate between these. Again Lecanu, quoted by Sir Dyce Duckworth ("A Treatise on Gout," p. 120) has arrived at the same relation that I have: 1:33. It seems, then, that there is good reason to believe that the natural relation of uric acid to urea in excretion, and I would add formation, on a mixed diet is 1:33. But a man who never excreted uric acid in a greater relative proportion to urea than 1:33 would, if my results are correct, never have a uric-acid headache or mental depression, and to get either of these there must be first of all some retention or storage followed by some plus excretion.

Results with drugs point to the same thing, none of them can cause a plus excretion for more than a day or two, it then falls down to somewhere near 1:33. Further, if a powerful solvent, such as salicylate of soda, is given for a day or two causing a plus excretion, an alkali given in sequence to it will have nothing to act upon and will cause no plus excretion whatever; but if an acid be given for some days, causing retention, an alkali following it will cause a plentiful excretion. It is a very common thing in the action of a salicylate to find that on the day of its first great rise of uric-acid excretion, there is some rise of urea also, and I think that this, in part at least, may be due to conversion of uric acid, some of that washed out of the spleen being converted into urea as it passes the liver.

Such are the arguments against plus formation of uric acid above the relation to urea of 1:33 and in favor of all fluctuations met with in practice being those of excretion.

There are, however, several facts which point to a possible conversion into urea of more or less of the uric acid which is formed, and I am inclined to believe that when a meat diet quickens the metabolism of the body, as in the case of stout people who become thin on it, it may promote a more extensive conversion of uric acid in the liver, and thus actually reduce the amount in the body.

It must, however, I think, be, in many cases at least, an

absolute diet of lean meat and water, for before I began to alter my diet in the direction of vegetarianism I had tried a large amount of meat food in a mixed diet, meat, eggs and fish, forming roughly about two-thirds of my solid food, I took also a large amount of muscular exercise and was living under most favorable conditions for general health; but with all this, my headaches were both frequent and severe. Now, however, on a diet of milk, fruit, and vegetables, with an occasional egg, they are almost nil, even under the general conditions of a sedentary life, which are much less favorable to health.

Dr. Broadbent records cases in which a farinaceous diet lowers arterial tension and cures mental depression or epilepsy. Prof. Lange has got good results with a similar variation of diet in mental depression; and the value of a milk diet in acute rheumatism or gout and all forms of nephritis hardly requires to be mentioned.

So that while I do not expect that a pure meat diet would necessarily do harm and believe that in some cases it might even do good, as for instance where gout is conjoined with obesity, I think that the evidence of experience, not to mention physiology, is in favor of the treatment of this class of disease by a reduction of meat food; and there seems no reason to doubt that in the absence of organic disease this can be done with impunity, as in my own case, and I have seen even in this country powerful working men who said they never touched meat from year's end to year's end, and I have seen in Scotland plenty of men who could compare favorably with any English navvy, the staple of whose diet was skim milk and oatmeal, bread and butter; meat or fish being obtainable only once a week, or market days, and not always then.

In myself a change from a diet of fish, eggs, and occasional meat in small amount to one consisting of milk, fruit, and vegetables, with only very occasionally an egg, reduced the urea formation from the average level of about 400 grs. in the twenty-four hours, down to 300 grs., in the same time, and the uric acid and acidity fell to a corresponding extent. This diet continued for eighteen months produced no change in weight or physical strength, and the only evil I could lay to its charge was some slight increase of mental depression, this being specially marked in July and the summer months, but at worst it was a mere trifle compared to the headaches which I avoided,

and even this will probably cease when all the uric acid accumulations of previous years have been excreted. (My previous change from an ordinary diet with meat once or twice a day, to one with fish and eggs also produced a like alteration; my urea formation on ordinary diet being about 500 grs. per day.)

It appears, then, as if there is at times some small retention of uric acid even on vegetarian diet, and as so many things besides diet affect the alkalinity of the blood and tissue fluids, and the acidity of the urine, this is not perhaps to be wondered at, but the absolute amount of uric acid being small, the most it can do is to cause a little depression. The same reasoning must, I think, apply to gout, for if there is less uric acid formed and less retained, there must be less to cause joint troubles than on a diet containing a larger proportion of meat and animal food.

Acute Rheumatism.—Gout is considered to be due to uric acid, and it is cured by salicylate of soda, which causes a large excretion of uric acid. Acute rheumatism in the same way is cured by salicylate of soda, which in this case also causes an enormous excretion of uric acid, but yet, acute rheumatism is said to be due to miasm, microbes, and all kinds of things, other than uric acid. I shall now as shortly as possible give a few of the facts which seem to me to point to the possibility of acute rheumatism being due to uric acid, just as is the case with gout. First of all there is the argument with regard to the action of salicylate of soda, and there is no doubt whatever that as this substance relieves the pain and brings down the temperature of acute rheumatism, it also causes a relatively enormous excretion of uric acid. Thus in one case under my care I got, in the first twenty-four hours, under salicylates an excretion of upward of 26 grs. of uric acid, this occurring in a patient whose normal excretion would be about 10 or 12 grs., and though there was a rise in urea at the same time, the relation of uric acid to urea was about 1 : 17, this showing, I think, that a very large amount of the uric acid excreted did not come from the kidney direct, but from a store in other parts of the body, as the joints or viscera, and my impression is that if this uric acid came from the joints, its presence there was quite sufficient to account for all the pain and fever of an attack of acute rheumatism. Besides the salicylates, the only drugs which have obtained any great repute in the treatment of

acute rheumatism are the alkalies, and these again promote a free excretion of uric acid, though at the present time they are not considered to be nearly so useful as the salicylates; and this, I think, is chiefly due to the fact I have so often mentioned, that the free excretion of uric acid under salicylates is not hindered by the presence of an acid, while an alkali has to be given in sufficient dose to overpower the acid before it can be of any use at all. Generally speaking, at least by the public, rheumatism and gout are little, if at all distinguished from each other, unless perhaps by the age of the patient in whom they occur; many of the causes which produce the one disease are well known to produce the other also; thus, deficient skin activity, or exposure to cold and wet, will in a young patient precipitate acute rheumatism, in an older gout, and in correspondence with this there is active metabolism in a young patient and larger production of uric acid and urea, weight for weight, than in an old one. Again, meat, beer, and acid beverages generally are harmful in acute rheumatism, and we have already seen the injury they may do in gout. Again, acute rheumatism may bear practically the same relation to other minor uric-acid troubles that gout does; thus it is no very uncommon thing for a patient suffering from slight subacute rheumatism to take a few doses of salicylate of soda, which relieves the joint pains, but as the attack is passing off and the influence of the salicylate is coming to an end, an attack of migraine or uric-acid headache may supervene, this being produced just in the way I have previously spoken of by uric acid, which the salicylate of soda fails to carry off completely, and leaves under the influence of the alkaline tide. Dr. Stephen Mackenzie ("Brit. Med. Jour.," Vol. 1, 1886, p. 100) gives it as his opinion that rheumatic fever is a nervous cyclone, because it is connected with migraine, which he calls a "little nerve storm." I have shown that the "little nerve storm" is really a uric-acid storm, and I believe that the great nervous cyclone of rheumatic fever is also a uric-acid storm.

With regard to the exact causation of rheumatic fever, it appears to me that there is a great deal to be said in favor of its production by the exact same causes that are active in the production of acute gout, and as acute gout is, as I have attempted to show, the result of the retention of uric acid in certain joints by acids, I believe that acute rheumatism is due

to practically similar retention by acids; and a glance at its relation to other diseases and the common history of its onset will, I think, bear this out. Acute rheumatism, as is well known, may follow in the wake of almost any fever, or of tonsillitis, some inflammations, and even an ordinary febrile catarrh, and the reason of this, I think, we shall very soon see. A good deal of work has recently been done with regard to the alkalescence of the blood under various conditions of health and disease. I have previously spoken of Prof. Haycraft's papers for the estimation of this alkalinity, and a good many other people, by more or less similar methods, have been investigating the same matter. One of the most important points which seems to have been brought out by these investigations is the fact that in all fevers the alkalinity of the blood is decidedly diminished; thus, Dr. Peiper, in a paper (Virchow's "Archiv," June, 1889, p. 337), gives among other results his opinion that the alkalescence of the blood is diminished in all fevers, unless they are accompanied or complicated by dyspnœa or cyanosis, and if this is true, we have in all fevers just the most important condition which is necessary to produce a retention of uric acid and drive it into the joints and viscera. It is little wonder, then, that acute rheumatism should follow certain fevers and febrile states; the wonder rather is, that it does not follow every fever; but I take it that the effect of the fever in producing retention of uric acid and driving it into the joints must obviously depend on the amount of uric acid that is present in the blood and ready to be acted on. With regard to the common sequence of acute rheumatism on an attack, say, of tonsillitis, the following is, I think, the process of causation. The exact way in which the tonsillitis is produced I am not able to say, but I know for a fact that it is a disease which is very liable to occur in young people after exposure to excessive fatigue, this fatigue having, as one of its results, the formation of a large amount of urea and uric acid; then comes the tonsillitis and with it some febrile movement and a fall in the alkalinity of the blood; then there is, as we have seen, reason to believe, a large amount of uric acid and urea in process of metabolism, and as the fever raises the acidity, which is the same thing as diminishing the alkalinity, of the blood, this uric acid is driven out of the circulation into the joints, and we have then to deal with an acute rheumatism

supervening, and as the fever rises still higher, the retention of uric acid will become greater and greater until salicylate of soda comes in and cuts the vicious circle by promoting a free excretion. Such a sequence gives us, I think, a very fair explanation of all the phenomena of an attack of acute rheumatism, and tells us why it should follow fevers in the way it has been observed to do. I have also had under my care some cases which illustrate, in a very interesting manner, what I consider to be the causation of acute rheumatism by fever; thus I had not very long ago under my care a small boy of about fourteen years old, who came in with an ordinary attack of acute rheumatism, yielding in the usual way to treatment by salicylate of soda; however, a few days after his temperature had become natural, it began to rise slowly again, but this time there were no joint symptoms, and an increased dose of salicylate of soda had no effect on the temperature. After this had gone on for some little time it was discovered that he had an abscess round one of the molars in the upper jaw, and that this was the cause of the trouble and the rise of temperature; the mischief, however, had gone too far to be easily cured, and the temperature rose on one or two days to a considerable height, then all at once there came a relapse of the acute rheumatism, with pains in his joints and a temperature which was again under the influence of salicylate of soda, and when this had relieved his joints and brought down his temperature and the tooth had been removed, he was soon quite well. This history, I think, furnishes one or two points of interest; first of all, when the temperature was rising, on account of the dental trouble, salicylate of soda, which had previously brought down the rheumatic temperature, had no effect on it; but when again the high temperature produced, as I believe, acute rheumatism, the salicylate once again gave prompt relief. Secondly, the relation of the relapse to the dental irritation and high temperature is of great interest, and I believe that, if the matter were carefully looked into, a great many relapses or attacks of acute rheumatism would be found to have similar causation. Thus I have seen attacks which appear to bear just the same relation to febrile disturbance that this one did, as attacks following various disturbances of a more or less functional nature; or again, the rheumatic pains which are so well known in ordinary febrile cold

(influenza). It may be interesting to remark here that in such colds salicylate of soda often gives extremely satisfactory results, and I believe that in all colds, whether there are or are not well-marked rheumatic symptoms, it is a decidedly useful drug.

The fact that Sir A. Garrod was not able to find excess of uric acid in the blood of rheumatic fever is probably considered by some to be a strong argument against the causation of this fever by uric acid; but I think it follows from what I have said above, and from my other experiences with uric acid, that if the pains of acute rheumatism are due, as I have supposed, to the uric acid, which is driven into the joints by a high and rising acidity, that any one examining the blood during the height of the rheumatic fever would almost necessarily find no uric acid there, so that from my point of view, Sir A. Garrod's failure to find any uric acid under these conditions is not a matter of surprise, and does not, I think, prove that the joint troubles are not due to uric acid, but rather the reverse. Sir A. Garrod has noticed (previous ref., pp. 187 and 274) that there is no uric acid in the fluid of a blister or in blood drawn directly over the inflamed joint in gout, and he has argued from this that inflammation destroys uric acid; but if, as I have argued above, fever and inflammation cause both a local and a general rise of acidity, it is not difficult to see that the high acidity causes the precipitation of the uric acid in the joints and tissues, completely removing it from the blood, so that I should attribute Sir A. Garrod's results not to a destruction of uric acid, but to its precipitation from the blood and tissue fluids by a high and rising acidity.

I have not examined the blood in acute rheumatism for uric acid, but I have several times examined its alkalinity, and have found that it is, as one would expect, lower at the time of the fever than afterward, when the patient is in his ordinary health. I have also examined the blood for uric acid in some local inflammatory conditions, and have found that it is absolutely free from uric acid, the small trace to be commonly found in healthy blood being conspicuously absent; so that here again local inflammation has destroyed the uric acid, or, as I should prefer to put it, has driven it out of the blood, by the high acidity to which it gives rise. We thus see how any slight injury to a joint causing inflammation may lead to a precipitation of uric acid in it, and so to an attack of gout.

About chronic rheumatism and rheumatoid arthritis I have very little to say; what are commonly called rheumatic pains in middle-aged or elderly patients, are, I think, really very often gout; and chronic rheumatism, if it means anything more than this, means some change of a chronic nature gradually progressing in cartilage, ligament, or fibrous tissue, which, though it may have had its origin in the irritation set up by uric acid, has not now any very close connection with it. Rheumatoid arthritis is from my point of view a similar alteration in the nutrition and structure of joint tissues, and though it may now and then, as I believe, be kindled into fresh activity by, so to speak, a fresh invasion of uric acid, and may then for a time be improved by treatment with salicyate of soda, it is for the most part best treated by drugs directed to improve the general nutrition, such as cod-liver oil, or by alteratives, as the iodides or arsenic, by local applications, baths, and frictions. Such is the idea which my observations of the action of various drugs, and especially of the salicylates, on this disease lead me to entertain as a provisional hypothesis.

Albuminuria.—In a paper recently read at the meeting of the Brit. Med. Assoc., I have gone into what I believe to be the relation of albuminuria to high pulse tension at such great length that I shall only say very little about it here. The late Dr. Mahomed, of Guy's Hospital, showed, I believe, conclusively that high arterial tension may precede by several years the onset of albuminuria and chronic nephritis, and similarly with regard to the albuminuria following scarlet fever, he showed that high tension of the pulse was generally present before the albuminuria. In his writings he has urged that the high tension pulse and other symptoms of the "pre-albuminuric stage" of chronic Bright's disease, are due to a poison in the blood, and the signs and symptoms produced by this poison have appeared to me to resemble so exactly those of uric acid that I have gradually come to believe that uric acid is the cause of the high tension pulse found by Dr. Mahomed in these cases, and to think that the high tension pulse, possibly together with the irritation produced by the uric acid in passing through the kidneys, is the actual cause of the albuminuria and nephritis. The way in which the high tension pulse may produce an albuminuria I have endeavored to explain by a reference to the writings of Prof. Semmola ("Arch.

de Physiol.," 3. Series IV., p. 300), of Naples, on the etiology of Bright's disease and albuminuria. Semmola considers that an albuminuria may be produced among other things by deficient activity of the respiratory and metabolic functions of the skin, and I have suggested that uric acid, when it causes high tension, contraction of the peripheral arterioles, and coldness of the skin and extremities, brings about just those conditions which will interfere with proper activity of skin function; it will thus produce albuminuria, and Semmola believes that the albuminuria will eventually produce nephritis. I am quite aware that there are some facts which point to uric acid and other products of nitrogenous metabolism having an irritant action on the kidneys when they have to be excreted in large amount, but this will not account for the presence of a foreign albumin in the blood, and Semmola has the support of other observers with regard to the existence of such a foreign albumin in the blood in Bright's disease. Deficient activity of the skin will, however, on Semmola's theory, account for the presence of a foreign albumin in the blood, and I have suggested in the above-mentioned paper that uric acid may be the cause of albuminuria and nephritis through its action on arterial tension, and so on the functions of the skin.

Uric acid, as a cause of high arterial tension and contraction of the peripheral arterioles, has some interesting relations, I think, to the causation of Raynaud's disease, in which, contraction of arterioles has such important results, and I think also, that paroxysmal haemoglobinuria has some interesting relations to a uric-acid storm, but I have spoken of both these diseases in the paper read at Leeds. It may here be called to mind how constantly cold surface and extremities are met with in the uric-acid headache, or generally where there is high tension due to uric acid.

Diabetes and Glycosuria.—The relationship of glycosuria to gout has often been noticed, it has also been observed that they do not occur together, but alternate: that is to say, when there is gout there is no glycosuria, and conversely, when there is glycosuria there is no gout. The prominence of gouty history and symptoms in some cases of glycosuria led me to try salicylate of soda, and I soon found that it will practically cure some cases of glycosuria, at least for a time, and even in some cases of severe diabetes it will at once very greatly

reduce the sugar. I have also observed that several drugs which do harm in gout, and would, if given in that disease, precipitate an attack, as for instance, antipyrin or opium, do when given in glycosuria or diabetes produce good results, and considering the way in which glycosuria and diabetes have been observed to alternate with gout, this is just what we should expect. (See paper in "St. Barth. Hosp. Rep.," 1889.)

Asthma.—Here again we have a possibly functional disorder, which, like so many I have mentioned, has long borne a somewhat indefinite relation to gout, and it has occurred to me, since my investigation on uric acid has brought to my notice its action on the arterioles, that directly or indirectly it may affect the circulation in the bronchi and lungs, and so produce asthma. There are also not a few points in the history and symptoms of asthma and the onset of the attack which may be taken as pointing to its possible origin in a uric-acid storm. I can only now add, that in some well-marked cases of this trouble I have found salicylate of soda to be of very great value in treatment.

Phthisis; Hæmoptysis.—I have mentioned in several papers the frequency with which gout and phthisis occur together in the same family history ("St. B. H. Reports," Vol. 23, p. 208 and reference), and this fact has been noticed also by others (see views of authors, quoted by Sir Dyce Duckworth, "A Treatise on Gout," p. 172). Beyond noting the fact, however, I had till quite recently no very definite idea as to the exact nature of the connection, though I rather thought that some vascular changes in the lung, produced by uric acid, might directly or indirectly produce local disease and afford the bacillus a foothold. The very interesting paper recently read at the Medical Society by Sir Andrew Clark ("Brit. Med. Jour.," Oct. 26th, 1889) will, I think, possibly enable us to go further, as he there shows, in a very definite manner, the connection between the vascular changes so common, at least, in the general circulation of arthritic subjects, and hæmoptysis. It seems to me that uric acid may affect the pulmonary circulation in two ways, either by producing high tension in the general circulation and so hampering the heart, which in turn affects the pulmonary circulation, or it may produce similar changes in the pulmonary arterioles to that which I believe it produces in the general circulation.

There is an interesting case bearing on these points in a letter by Dr. Moorhead, in "Brit. Med. Jour.," Vol. 2, 1889, p. 980. It is unfortunately not given with great detail, but so far as it goes it illustrates, I think, some points in the possible connection between gout and hæmoptysis. The history is this: a gentleman of eighty years of age, after some exposure to cold in an open boat, suffers from copious hæmoptysis with bronchitic symptoms; he is given a saline aperient, and next day the hemorrhage is found to have ceased, but an acute attack of gout has come on in the right foot. My explanation of this sequence would be as follows: The exposure to cold caused, in the way I have already pointed out in speaking of Fig. 1, a high and rising acidity, which produced a temporary retention of uric acid, and this, when he got into a warm room again, would give place probably to a falling acidity and an excess of uric acid in the blood, this accounting, as I have suggested, for the high tension in the circulation, and possibly for the hæmoptysis. Then comes the saline aperient, which was, I suppose, a mixture containing sulphate of magnesia, and possibly some sulphuric acid; either of these would drive the uric acid out of the blood by raising the acidity and thus lower the tension, relieve the pulmonary circulation, and further, by driving uric acid into the joints, produce just what occurred in this case, an attack of gout. It appears to me that the attack of gout following the day after the hæmoptysis, was almost in itself positive proof that there was an excess of uric acid in the blood on the previous day, for had there not been such an excess, the rising acidity would have had nothing to act upon and could not have produced gout. Speaking of sulphate of magnesia reminds me of a point which I have not mentioned before, viz., that all sulphates, and, in fact, most salts of the mineral acids, act as acids and raise the acidity, and when they do this they at the same time diminish the excretion of uric acid, lower arterial tension, and produce feelings of happiness and well-being, and I have seen in some medical journal, though I cannot now find the reference, a remark which Byron is reported to have made to the following effect: "Talk of champagne, there is nothing cheers your spirits like a dose of Epsom salts." Now I take it that this is very valuable testimony to the action of sulphate of magnesia as an acid, and in the way I am suggesting, and Byron was a man

who apparently suffered very greatly from mental depression, due in all probability to uric acid; and of the truth of his remark it is not difficult for any one to convince themselves, and if they, at the same time, follow the curve of urinary acidity, they will see that it very distinctly rises at the time that the dose of the sulphate is causing feelings of well-being.

I have now come to an end of the points which up to the present time seem to be of special interest, and I shall, in conclusion, review very shortly the more important of them. We have seen that the excretion of uric acid can be controlled and either increased or diminished, within certain limits, by very various means; but the formation of uric acid, in its relation to urea, cannot be controlled, though under certain conditions some part of the uric acid formed may be converted into urea.

Uric acid is in excess in the blood, not from direct overflow from the kidney, but from continued retention and storage of the small normal overflow by the renal vein.

Such excess of uric acid in the blood is a cause of headache, mental depression, high tension pulse, and possibly of epilepsy, and by its being driven out of the blood into the joints it causes gout and acute rheumatism.

It is further suggested that the plus tension pulse as produced by uric acid is the cause of albuminuria and nephritis, also that many other diseases until now more or less indefinitely connected with gout may be found to be directly connected with it, through the effect of uric acid on the pulse tension and so on the circulation in the skin, brain, lungs, liver, kidneys, and other viscera.

That foods which increase the actual formation of uric acid, as meat, and further by raising the acidity tend to prevent its excretion, do harm in such diseases as gout and rheumatism, which are due to its retention, and also indirectly in such diseases as headache, mental depression, high tension pulse and others, which result from its subsequent excess in the blood.

That absolutely large formation of uric acid has by itself no evil effect in these diseases, but inasmuch as a large formation of uric acid and urea is practically always accompanied by a high acidity, some of the uric acid formed is almost certain to be kept back and retained in the body, except when the large formation is the result of exercise, of which I shall have to speak presently.

It follows from this that the best treatment of these diseases is to diminish the formation of uric acid, urea, and acids as far as is compatible with healthy function, and further, to use some means of diminishing acidity, so that nothing should interfere with the free excretion of all the uric acid formed.

These indications are best fulfilled by reducing very greatly the animal food, eating largely of fruit and vegetables, and avoiding beer and wine. (If acid drinks are required, those containing citrates and tartrates, or the juice of lemons and other fruits, which do not affect acidity, should be made use of.)

Exercise is of use in several ways, for though it increases the formation of uric acid and urea, in their normal relation to each other (North, Proc. Roy. Soc., 1885), it does not raise acidity, and indeed may even lower it, so that it promotes rather than hinders the excretion of all the uric acid formed; it therefore does no harm in gout, and further, by improving digestion, and tending to prevent dyspepsia, it wards off attacks of headache, mental depression, or epilepsy, and, as I have previously mentioned, it removes high arterial tension and, with it, mental depression, I suppose by relaxing the arterioles of the skin, and produces a happy feeling of strength and well-being, by freeing the circulation in the nerve centres. Sir A. Garrod (previous reference, p. 230) says: "Cullen remarked that gout seldom attacked persons employed in constant bodily labor, or those who live much upon vegetable diet." The result of both these conditions of life would be to lower the acidity of the urine and increase the alkalinity of the blood and tissue fluids, thus promoting excretion of all the uric acid formed. Cullen's observations, it will be seen, exactly bear out my deductions as to the best method of treating uric-acid disease.

Drugs are useful at a crisis and to prevent the continuance of severe symptoms, but the only treatment which goes to the root of the matter is that which aims at prevention by diet.

Among drugs that promote excretion, I regard the salicylates as by far the most useful, and among those that cause retention, opium is probably the most powerful, as it rarely fails to produce a marked rise of acidity and overpower any alkali present.

Drugs may fail to act from non-absorption, which in the case of salicylates may be proved by the absence of reaction

with perchloride of iron in the urine, or acids and alkalies may overpower each other; thus, with organic disease and great failure of nutrition, nothing will avail to raise the acidity, and conversely, when there is inflammation or fever, or formation of acids in the stomach or intestines, or retention of acids, from deficient skin activity, alkalies are liable to be overpowered. By bearing these facts in mind, apparent failure of action on the part of drugs may often be easily explained.

Such are my results, and the deductions I have been led to make from them; some of these being, I hope, very close to the truth, others, perhaps, more or less valuable working hypotheses, and all tending to show that the part played by uric acid in the causation of disease is much more important than has hitherto been realized.

THE

Initial Stages of Consumption:

THE NATURE AND TREATMENT,

Including Dietetic Suggestions.

BY

HORACE DOBELL, M.D, Etc.,

Fellow of the Royal Medical and Chirurgical Society; Fellow of the Royal Meteorological Society; Consulting Physician to the Royal Albert Orphan Asylum, Bagshot; Consulting Physician, Late Senior Physician, to the Royal Hospital for Diseases of the Chest, London.

THE INITIAL STAGES OF CONSUMPTION.

CHAPTER I.

In the commencement of the American Civil War, the North was stirred to its core by the following rough lines vociferated by a patriot whose zeal out-did his English:

> "Here's Hell let loose, and we lie flat,
> And all the world around is singein',
> While General This and Governor That
> Are quarrelling o'er the garden engine."

A somewhat similar spirit of zealous impatience is apt to stir the mind of the medical practitioner, who is called upon to prescribe, somehow or other, for patients threatened with death from diseases concerning which questions as to their exact nature are still seething in the cauldron of scientific discussion. He cannot wait for the ultimate result of the scientific investigation of disease! He naturally exclaims, "Tell me, in the state of our knowledge this day, what is the best thing I can do to repel 'this enemy at our gates'?"

The doctor is amply justified in this impatient demand, and with equal right it is reiterated by the public.

The purpose of the present treatise is to attempt some sort of articulate answer to this call with respect to "The True First Stage of Bacillary Consumption."

It has been the first object of my life-long study of tuberculosis to discover the nature, causes, and symptoms of the pre-tubercular stage, in order to find out the means for its cure, and thus to be able to prevent the occurrence of tuberculization.

We will proceed at once to consider the position which these questions hold to-day.

The announcement in 1882 of the discovery by Koch and Baumgarten of the tubercle bacillus, necessitated that every physician, whether scientific, practical, or both, should reconsider his position with regard to the etiology, diagnosis, and prognosis of consumption, but especially as to its prevention and treatment.

I had just completed and published a reconsideration of more than thirty years' work on the subject,[1] the whole of which had preceded the discovery of the bacillus. I stated that my work had been dominated by the conviction "that intelligent, accurate, systematic clinical observation is the source to which we must principally look for the discovery of the nature, cause, sequence, prevention, and cure of such affections as destructive lung disease;" and in reference to "the opinion held by some that the causes of consumption are not general, but local—that they begin in the diseased organ itself not in a constitutional state"—I added that "no question in practical medicine can be more vital than this. It strikes at the root of all ideas of prevention as well as of treatment, and the only way to clear it up is to push a searching investigation into the early clinical history of destructive diseases of the lungs:

To ascertain in what proportion and at what intervals the earliest signs or symptoms of local disease are preceded by signs or symptoms of constitutional disease.

To ascertain the typical characters of the signs and symptoms of such local and constitutional diseases.

To ascertain whether the earliest stages of local disease can exist without indicative signs and symptoms. It is here that the aid of morbid anatomy may be called in with signal value in the class of accidental cases (referred to in the work).

To accumulate any cases which appear to be exceptions to general rules. To scrutinize these with the greatest care, and endeavor in every possible way to explain their exceptional character. If it is found that in a large number of cases of pulmonary consumption (destructive lung disease with constitutional decline), local disease is preceded by constitutional disease by an unequivocal interval; and yet that in a certain number of cases local disease precedes constitutional disease

by an unequivocal interval; the conclusion is almost inevitable that there are at least two modes in which pulmonary consumption may commence. It then becomes a point of the greatest interest and importance to ascertain whether these two classes of cases are to be considered as absolutely distinct throughout, or whether there is a period in their causative history at which they meet on common ground. That is to say, is there a stage at which the constitutional disease sets up the local disease? and is this first stage of the local disease, in these cases, the same as the first stage of the local disease in the cases not preceded by constitutional disease? And again, starting from this point, are the subsequent symptoms of constitutional decline in the two sets of cases due alike to the effects of the progress of the local disease? or are they, in the first set of cases, due to the progress of the original constitutional disease plus the effects of the local disease? And, in the second set of cases, are they due to the effects of the local disease alone? Again, does this local disease set up a state of constitutional disease of the same nature, and leading to the same local effects, as that which precedes the local disease in the first set of cases? or is a new constitutional disease, of special character, set up by the local disease in the second set of cases? And, if so, is the local disease of the first set of cases also competent to set up a special constitutional disease the same as that of the second set? Finally, are the symptoms of constitutional decline which follow the establishment of local disease in each set of cases devoid of all special character and only such as may accompany the progress of any local disease proceeding to a fatal termination?"

Now, I think it will be admitted by all fair-minded men that such an inquiry as I have detailed, honestly conducted, must yield results the importance of which will not be destroyed—although they may be modified—by any new discoveries of facts; but that, on the contrary, the results of such an inquiry will aid us in associating newly discovered facts with appropriate ideas as to their place, sequence, and import. This is strikingly illustrated by the case of the discovery of the bacillus which followed close upon the heels of the publication of my answers to the foregoing questions. (See Chapter V.) It will be found that it readily takes the place of one of the irritants enumerated by me as causes of tuberculization,

the one which then I could only hypothetically explain. It also readily takes the place of "the virus," the existence of which was asserted as a general conclusion from "the discussion on the Pathology of Phthisis Pulmonalis" at the Pathological and Clinical Society of Glasgow, February and March, 1881, just after the publication of my work, but still before the discovery of the bacillus.

Dr. Joseph Coats said, "We cannot accept any theory of phthisis which does not take into account a state of the system predisposing to it. . . . That a state of the system is not sufficient to account for tuberculosis is abundantly evident in cases of acute general tuberculosis. I have seen a strong muscular man with no lack of adipose tissue die within a few weeks of this disease, evidently a virus of the most active kind. . . . Physicians are, perhaps, liable to take too much the appearances during life into account. And in this reference I would say that pathology undoubtedly points to a virus as the cause of phthisis, and clinical facts point to a state of the system as at the bottom of it. We have to accept both, and my belief is that this position will be that of the immediate future."

As these were the inevitable conclusions from collected observations—so far, of course, as those observations had been accurately made, and the conclusions rationally drawn—they could not be vitiated by the discovery of the bacillus, that is, of a fact which, although not known, yet existed, and exerted as much influence upon the clinical characteristics of the cases, while unknown, as it could exert after its discovery. Therefore, such clinical observations of facts must still take their place among the foundations of sound medical and scientific conclusions.

It is quite different with regard to any theories or hypotheses based upon them. These must inevitably require reconsideration whenever a new discovery of facts is made. And no discovery could more emphatically call for such a reconsideration than that of the tubercle bacillus. Therefore, the moment its discovery and the means of its identification became known to me, I instituted a new series of observations with a view to endeavoring to estimate its clinical importance, and to assign it its appropriate place and sequence in relation to my former observations. From that time to the present

the sputum of all my chest cases has been submitted to a double analysis, first for bacilli, second for débris of air-cells (by Fenwick's process); and I have done my best to keep *en rapport* with the work which has been prosecuted in all parts of the civilized globe. No praise can be too great for the spirit and untiring industry with which the rising young physicians and scientists have applied themselves to this subject, both in the laboratory and at the bedside. A mass of accurate observations has been accumulated, which is astonishing when we consider that it was only in 1882 that the discovery of the tubercle bacillus was announced.

This ghastly denizen of our microscopic world, although so recently unveiled, is no new comer. We find that our forefathers pickled it unconsciously, and put it away in their bottled specimens of "Tubercle" to be found in our museums. It was as busy at its secret work then as now; and, therefore, as I have said, all clinical and pathological observations truthfully made and recorded, in whatever day this was done, relate as much to the work of the bacillus as do those made yesterday—so far as the means of observation and record of each day have been equal to the task.

It is the duty of the investigators of to-day, as it will be the duty of those yet to come, and as it has been the duty of the investigators of the past, truthfully, circumspectly, candidly, and I will add reverently, to seek to find out the exact place which each newly discovered element has hitherto secretly filled in the great histogenetic puzzle of metabolism.

We are all too apt, especially in the fervor of youth, to think that each newly-found wonder must upset all that went before! But the more sedate wisdom of experience teaches us that this is not so—fortunately indeed that it is not so, for otherwise the foundations of human knowledge would be as shifting as the sands of the sea. The plan of the great puzzle is the same, though each new cycle of scrutinizers may find that its elements are again and again divisible.

The question now before us is no longer whether there is a specific bacillus tuberculosis; but what is the place which it now occupies and always has occupied in the histogenesis and metabolism of tuberculosis?

It is fortunate for my present purpose that, recently, several important contributions have been made to the clinical

and experimental history of the tubercle bacillus and its effects, by men having at their command exceptional opportunities of research in laboratories, museums, and hospitals. I especially refer to the works of Barker, Brown, Coats, Crookshank, Kidd, Klein, McFadyean, Philip, Troup, Watson Cheyne, Weigert, Williams, Woodhead, and the reports of the Congress on Tuberculosis which met at Paris in July, 1888. These bring our knowledge quite "up to date," and thus completely fill the important interval since the publication of my Resumé in 1880 and the discussion at Glasgow in 1881, which occurred immediately before the discovery of the tubercle-bacillus gave a *point d'appui* to the surmises as to the nature of the specific inoculable "irritant" or "virus," the existence of which most investigators had ceased to doubt.

Mr. Barker,[2] after enumerating the leading facts which have been gradually accumulated in support of the view that scrofula and tuberculosis are identical, says: "But though all these facts seemed to point very clearly to the identity of scrofula and tuberculosis, it was not until the discovery of the bacillus tuberculosis, and its demonstration in the initial lesions of the two affections, that positive proof was actually forthcoming; almost at the same time Koch and Baumgarten pointed out that in typical tubercular and scrofulous nodules this organism could invariably be demonstrated by different methods of preparation; . . . and it is a noteworthy fact that many of those who at first, on the strength of their own observations, had stood out most firmly against the theory of the specific or parasitic nature of tubercle, have since become its most ardent supporters; and, moreover, have given in their adhesion to the view that this bacillus is the specific organism of tuberculosis. . . . Experimental research repeated, with proper precautions against error, left no doubt on the subject, and not only was the inoculation of fresh tubercle shown to produce in all cases a definite tuberculosis, but it was also demonstrated that the organisms in question, cultivated for many generations in various media, until all trace of contamination with matters from the original living tissues had been got rid of, were capable of producing this disease, when introduced into the animal body, with as great a certainty as deep sleep is produced by the hypodermic injection of morphine. Again, the bacillus could be obtained in any quantity from

tubercles produced in internal organs by the inoculation of these cultivations in distant parts, and could be multiplied *ad infinitum* by cultivation, and propagated among some of the domestic animals with unerring certainty. Now, it mattered little whether the tissue from which the inoculations were made was taken from a scrofulous joint, or from a typically tuberculous lung, the result was practically the same. . . . The researches bearing upon these facts are far too numerous for detailed description. . . . But to a candid mind they bear but one construction, viz., that tuberculosis and scrofula are diseases produced by the introduction into the animal body of a specific poison, and that this poison is always associated with the presence of the bacillus in question, if it be not actually the organism itself, and further, that without the presence of this organism typical tubercle does not exist."

Dr. Troup[3] sums up the matter by saying, "The only thing certain, however, is that the lung disease is tuberculous where they (the bacilli) are present."

Dr. Sims Woodhead[4] says, "There is now, however, sufficient evidence to justify pathologists in stating that many of those forms which different clinical observers have from time to time described as tuberculous are undoubtedly tubercular [bacillary?] in character; from the gray, gelatinous, or fibroid nodule, to the large caseous masses leading to cavity formation; and the presence of the specific bacillus has time after time been demonstrated in all these forms, both by staining and by inoculation. There can be little doubt that these forms are essentially the same, and that the differences observed are due firstly to the resisting power of the tissue attacked, and secondly, to the numbers and activity of the attacking bacilli. If the behavior of other tissues under the action of mechanical or micro-organismal irritants be borne in mind, there will be little cause for wonder that there should be these numerous varieties of manifestation of the action of the specific irritant in tuberculous lungs."

We cannot attach too much importance to the fact stated by Mr. Barker, that the bacillus is still the specific infective element of inoculable tubercle after having been cultivated for many generations in various media, till all traces of contamination with matters from the original living tissues have been got rid of; because this disposes of the suspicion, which natu-

rally arose when the bacillus was first discovered in tubercular matter, that it might not be the bacillus itself, but some matter which it carried with it which was, in truth, the specific inoculable element.

After the deepest thought and elaborate reconsideration of the whole subject, in the light of my former investigations and of all that has happened since, I accept as a fact that the bacillus tuberculosis—the organism itself pure and simple—is the specific transferable element, capable, under certain conditions, of domiciliation in animals, and the presence of which in a tissue is capable of setting up the formation of tubercle. But to call the bacillus "tubercle" would be absurd, for it is not proved to be more than one of its causes. Tubercle, however produced, is more or less of what I described, in 1880,[1] as "a foreign mass made up of hyperplasia, of adenoid tissue, epithelial and connective-tissue proliferation, angioplasia, and other attempts at repair of disintegrated tissues, and the results of degradation and decomposition of these new formations." In this description "angioplasia" was made to include "giant cells." It is suspected by many that giant cells may have a specific relationship to the bacillus, and should this prove to be true, they ought not to be present in any mass not caused by the bacillus. Their presence in that case would be as typical of disease caused by the bacillus as the presence of the bacillus itself; although, if from any cause the bacillus had disappeared from the mass its former presence had set up, the mass would no longer be capable of transferring the disease by inoculation. It would cease to be infective.

In this way it might puzzle experimenters, and lead to confusion in the conclusions drawn from their experiments. It is of the utmost importance, therefore, to bear in mind this possible source of fallacy, and to continue our researches into the life-history of the giant cell, and of the other essential elements of the mass we call tubercle till absolute certainty is arrived at.

Klein says,[5] "In human tuberculosis, in tuberculosis of cattle, and in artificially induced tuberculosis of guinea-pigs and rabbits, there are met with tubercles in various stages—young and adult—in which no trace of a bacillus is found; whereas in the same section caseous tubercles may be present containing numbers of tubercle bacilli."

Dr. Woodhead[4] and Professor McFadyean carried out a most careful and systematic examination of over six hundred cows in the Edinburgh dairies, and they "found thirty-seven beasts in which there was mammitis, but only six (16 per cent) in the milk of which they could demonstrate the presence of tubercle bacilli, and then only in small numbers. In one of the six, and subsequently in five other cases, they made sure of the existence of the bacilli in enormous number in the udder by microscopic examination. They found new tubercular tissue disseminated in patches of various sizes throughout a portion of the gland, and that all the more minute elements of tubercle could be distinguished—the small round cells in which the nuclei are comparatively large, and the epithelioid cells, between or among which is a fairly well-developed reticulum. The giant cells were very numerous, but not so well formed as in the human subject; they were scattered throughout the new tissue. The tendency to caseation of tubercle in the udder is not nearly so well marked as in other parts of the body, but it does undoubtedly occur at points. The new growth of tuberculous tissue gradually invades the lobules of the gland, passing in along the lines of the lymphatics of the interlobular septa, so that a gradual transition from the healthy gland substance to the dense tubercular mass may be seen. In the mass itself the characteristic bacilli are present in almost inconceivable numbers. They are seen first as small stained rings (masses of bacilli) around a slightly granular or homogeneous mass—in fact, the giant cells seem to consist of the débris of cells, the result of the activity of the bacilli. In the smaller cells bacilli may also be seen, and others may be demonstrated lying in the spaces between the cells. On careful examination of the more healthy parts of the gland, especially at the margin of the new growth, ulceration into the ducts may be made out. In consequence of this interference with the nutrition of the tissues immediately around the ducts or acini, the basement membrane has given way, and a mass of tubercular granulation may be seen projecting into the lumen; the epithelium is also proliferating. In the granulation tissue, in the epithelial cells, and even lying free in the lumen, there are frequently numerous bacilli, and it can be easily understood how, once in this position, they find their way into the milk. This ulceration is not, however, of such

frequent occurrence as might be expected, for in the greater part of the gland substance left there is little or no catarrhal proliferation, and the ducts and acini appear to be obliterated in great measure by compression." Dr. Woodhead pointed out that, "in connection with this, it would suggest itself to most of those who examined carefully the sections he exhibited that what are called giant cells may in reality be nothing more than acini or ducts in which the bacilli have attacked and destroyed the epithelium. This idea would be greatly strengthened if some of the larger ducts were examined, for in these similar degenerative changes of the epithelium could be seen; in this epithelium bacilli could be demonstrated, and in the lumen itself there are frequently similar granular or homogeneous masses in which a few bacilli may be seen. In addition to these positions bacilli could be demonstrated in epithelial cells still attached, and also, in rare cases, in those lying free in the apparently healthy milk ducts, in which position Professor McFadyean had first demonstrated them." Dr. Woodhead also pointed out that, in reference to the question of the structure and importance of giant cells, each authority had his own theory. "It was now becoming evident that each of many of these observers, though describing different conditions, might still claim right on his side. Those who advocate that the giant cell is a lymph space with proliferating endothelial cells around are apparently justified. Then, again, Weigert has proved that a giant cell is nothing more in some cases than a collection of cells in which the bacilli are causing proliferation at the margin, fusion and degeneration in the centre—a mass of caseous material in the centre and proliferating cells with bacilli between them at the periphery resulting. Klein saw the giant cells being formed by the fusion of epithelial cells of the air vesicles. Small blood-vessels in transverse section have, like the lymphatics, been described as giving rise to giant cells. Dr. Barrett finds them in the seminiferous tubules in tubercle of the testicle, and Dr. Woodhead has seen them developed in connection with minute bile ducts in the liver, and in the milk ducts and acini in the mammary gland of the cow. In all cases the process Weigert describes occurs, but at different rates and with slightly varying results. The presence of these giant cells affords evidence that the cells are making a determined resistance against the advance of the bacilli, are giving way slowly, and so limiting the area

of caseation. In many cases where the giant cells with their rings of nuclei are best marked, very few bacilli are to be found, as they have been destroyed by the phagocytes at the margin, *i.e.*, the active cells with deeply-stained nuclei. In other cases, however, the bacilli have taken the place of the nuclei at the margin of the giant cell, the boundary line in such cases being determined for a time by the basement membrane of the tube in which the mass is formed."

Dr. Theodore Williams,[6] quoting Watson Cheyne, Green, and others, classifies the forms of cells which accumulate in the alveoli in lung tuberculization as—1. Ordinary epithelial cells lining the alveoli. 2. Epithelioid cells. 3. Giant cells. The epithelioid cells "are generally large and spheroidal, about four or five times the size of a leucocyte, containing granular matter and a large, and often oval nucleus, and occasionally a nucleolus; more than one nucleus is occasionally seen. These cells are developed from the epithelium, according to Watson Cheyne, and some of them are transformed later into giant cells. . . . In many cases of phthisis, the alveoli are stuffed with the epithelioid cells, some perfect, and others toward the centre undergoing caseation; they are of great interest on account of being the principal haunt of the tubercle bacilli, which are found in these and in the giant cells, and often in no other lung structure. It is curious to see how the bacilli are attracted toward them, and are seen in large numbers both around the cells and within the cell walls. The third element is the giant cell, as to the origin of which there has been so much controversy, ending for the most part in the general conclusion that it is epithelial. . . . Giant cells are sometimes not found in the very early stage of tubercle development, and appear generally after the products of exudation have become absorbed." "Klein states[7] that in some of the most rapid cases of acute tuberculosis, no adenoid growth is to be found, but only desquamative or catarrhal pneumonia." He considers that these different appearances of tuberculous matter "represent different stages of the same process, that the fibrinous exudation comes first, and is afterward absorbed by the surrounding tissue, which becomes infiltrated with fluid and shows distended blood-vessels. The exudation is replaced by numerous cells or by giant cells. If the irritation lasts long enough, the small-celled adenoid tissue appears in the alveolar wall."[6]

CHAPTER II.

GRANTING, then, that the tubercle bacillus introduced into an animal tissue is one of the irritants which will cause the formation of the heterogeneous mass which I have described, and which, however produced, we agree to call tubercle; and granting also that it is the most important and dangerous of these irritants; the utmost interest, in a medical point of view, must attach to the earliest steps by which this formation of tubercle—this tuberculization—advances after the bacillus has entered the tissue.

The most interesting experiments illustrative of the process of bacillary tuberculization, are those of Baumgarten. They were minutely and accurately made, and observed under circumstances which seem peculiarly free from the chance of error; and, as Mr. Barker says,[2] Baumgarten is peculiarly qualified for such work by his profound histological research in other fields, and his experiments have been repeated by many other observers with the same results, and the tissue changes observed have a close parallel in those demonstrated in human organs, as shown by the careful studies by Arnold and others of tuberculization of the lung. "But as they can be followed in their various stages, and uncomplicated by ordinary inflammatory processes, only in experimental inoculations on animals, the latter are taken for illustration. . . . In the experiments in question, a small fragment of caseating tissue is removed from a scrofulous joint or gland, with due precautions against contamination from without, and is inserted in the anterior chamber of a healthy rabbit's eye, through an incision in the cornea (the same operation is performed on a number of other rabbits); . . . one or more of these eyes is excised daily, and placed in preserving fluid for histological examination. Now what is found? (1) In the first few days absolutely no change is observed, except the cicatrization of the corneal wound, and the formation of a capsule of

granulation tissue around the foreign body introduced. (2) But from the second day on, a very evident increase of the bacilli is demonstrable within the inoculated fragment, until they are present in profusion. (3) From this spot, at which they first multiply, they are now seen to spread rapidly. In the first place they permeate the granulation tissue (capsule) around the caseous particle in enormous numbers. . . . Subsequently they penetrate into the adjacent parts of the iris and cornea. In the latter (cornea) they spread chiefly along the fresh scar-tissue, a fact of much significance to the operator, explaining as it does the occasional appearance of caseating foci in perfectly healed scars in the skin, perhaps weeks after an operation for deep-seated scrofulous disease. (4) On the fifth day, scattered bacilli are seen in those portions of the cornea nearest to the point of inoculation, but without, so far, producing the slightest deviation from their normal histological texture. No traces of other species of organisms are to be seen with or near these immigrants, or throughout the whole iris or cornea. (5) On the sixth day the bacilli are present in great numbers throughout the tissues of the cornea, which are still, however, practically unchanged. (It will be seen later on that I call the period from No. 1 to No. 6 "The eventful interval.") But at certain points, especially where the bacilli lie in dense groups, certain newly formed cells are to be seen having the general appearance of endothelial or epithelioid elements, rather than of white blood or lymph corpuscles. (6) Each day now adds to the hosts of invading parasites, which occupy an ever increasing tract of the tissues of the iris. The nearer to the point of inoculation, the more closely packed are they; the further off, the more scattered. Where the organisms are very sparse no histological changes are to be detected, but close to the point of inoculation, young tubercles in the form of aggregations of epithelioid cells are now noticed. (7) A little later, every intermediate form, up to the perfect type of miliary tubercle with their lymphoid and epithelioid cells are to be seen. . . . These formations are constantly found associated with foci of bacillus proliferation, no organism of any other kind being present. Moreover, in this experiment, no tubercle, not even the smallest, is ever found at a spot free from the presence of the bacilli, or having only a few of them. The amount of cell proliferation always cor-

responds to the richness in bacilli of these nests of parasites. (That is, it is proportionate to the amount of the irritant, there being no other irritant present but the bacilli.) (8) Thus developing and extending, the tubercular process advances further, until, on about the tenth or eleventh day, a visible macroscopic iritis or keratitis is produced. (9) In the distant internal organs of the inoculated animals an entirely analogous process of tuberculization was observed by Baumgarten. This was best seen in the kidneys, at about the fifth week after inoculation. Here the vessels of the glomeruli were frequently found packed with bacilli, and, where the latter were spreading into the parenchyma around, typical tubercles were produced. (10) Large numbers of bacilli were excreted by the kidneys. Similar experiments have been repeated by many other observers, with cultivated bacilli, and with exactly similar results. The dosage of microbes was shown by Watson Cheyne materially to influence the rapidity of the development of the disease, a point of the utmost importance as bearing upon clinical experience." Striking examples were given by Mr. Barker, in which similar direct inoculation of tubercle on the surface of the body in the human subject, both in children and in adults, had accidentally taken place, and numerous cases have been put on record within the last few years, which place the matter beyond doubt.

In these important experiments, then, it is clearly demonstrated that the tissue changes (*i.e.*, tuberculization) did not precede the presence of the bacillus, but followed it; that the bacillus first became domiciled in the tissue, and that then, after a distinct interval (see p. 369)—in the case of the cornea, at least five or six days—histological changes began, proportionate in amount to the number of bacilli, *i.e.*, to the amount of the irritant.

These appear to be unquestionable facts. But we must remember that in these experiments the bacilli were artificially and intentionally—that is forcibly—inserted into healthy tissues in animals constitutionally healthy. It does not follow that the same sequence of events would occur if the animals were constitutionally defective; or if the introduction of the bacillus into the tissue, instead of being by force, were due to some condition of the tissue which made it specially attractive to the bacilli, and hence invited their domiciliation.

It is a clinical and histological fact that there are cases in which the animal organism appears to be patterned out, as it were with a mordant, so that when a poison or virus gains admission to the system, it is attracted and domiciled in the patches by an elective affinity—as a mordant fixes the color of the dyer, and maps out a pattern on the fabric. A strikingly mordant-like condition of the tissues is demonstrated by the simultaneous development of symmetrical patches of disease when the blood is flooded with certain poisons. This indicates that the normal condition of tissues symmetrically distributed is acted upon by a common abnormal influence equally diffused through all. But when, as also happens, the patterning out of the disease is non-symmetrical, it indicates either that the mordant-like condition of the tissue is abnormally localized, or that the poison or virus is not equally and generally diffused. (See Mr. Jonathan Hutchinson's remarks, quoting Paget, Budd, and others, in the discussion on Syphilis at the Pathological Society.)

One of the points of greatest interest and importance, clinically and pathologically, is to find out what is this mordant-like condition of tissues which determines the domiciliation of the bacillus tuberculosis.

In bacillary tuberculization, both of the above conditions are seen. Most frequently the mordant-like condition is not symmetrical, but indicates some localized tissue fault, or other state, which specially invites the bacillus to domiciliation in the part or parts. Exceptionally, the symmetrical and general distribution of the local disease indicates that the whole torrent of the blood has become infected, the localization of diseased changes corresponding, not with any abnormal condition of tissue, but with the intensity of normal vascularity, that is to say, with the amount and intensity of the local poisoning. It is evident that from this cause young and growing tissues, although normal, will invite a special incidence of the blood disease.

But in the exceptional cases referred to, although the whole torrent of the blood may be poisoned, and the tissues patterned out with a general symmetrical diffusion of local disease, some want of symmetry is in most instances developed, because it is so rare to find all the tissues of the body equally normal in condition, even in states which may be called

"health." Hence, acompanying the general symmmetrical diffusion of disease, there are generally found patches of specially intense or extended local disease, giving a want of perfect symmetry in the diseased processes.

It is, therefore, extremely rare to find even acute general tuberculosis without some organ or tissue being unsymmetrically affected by the domiciliation of a special number of bacilli.

Many other complications of a puzzling and confusing character arise in the case of bacillary tuberculosis, in consequence of the number of the portals by which the bacillus may be admitted to the organism, the differences in these portals, and the variety of the contrivances by which its diffusion is opposed and its domiciliation frustrated at the several portals.

It is necessary, therefore, to take a bird's-eye view of the general plan of the organism with regard to these portals and the intercepting contrivances in connection with them. We find a venous system separated inviolably from an arterial system by the lungs, and a great food-supplying apparatus, with the liver placed as peremptorily between the food supply and the pulmonary circulation as are the lungs between the veins and arteries, intercepting every particle of new food that can be absorbed by the portal system of veins—that is to say, the carbohydrates, albuminoid materials, oleinous fat, alcohol, water and any matters soluble in water. Next we find a similarly peremptory arrangement, by which everything absorbable by lacteals and lymphatics is scrupulously kept out of the way of the liver, and conveyed by the thoracic duct to the lungs, but not until it has been submitted to an intercepting system of glands; the materials thus conveyed include all fat not absorbable by the portal system, that is to say, all the solid ingredients of fats emulsified by the pancreatic secretion; also all worn-out tissues, and the products of interstitial nutrition taken up by the lymphatics. Then as regards matters that can be admitted by the respiratory apparatus, we find all sorts of difficulties placed in the way of any foreign irritants contained in the inspired air—to prevent their admission to the alveoli, in which it is intended that only blood and atmospheric air shall come into intimate relationship. Thus there is the long and complicated system of mucous channels, all amply provided with secretory and excretory glands for the

expulsion of offending matters before they reach the regions of residual air; thus, also, the air in inspiratory and expiratory motion is not allowed to traverse these residual-air regions, in which the interchange of respiratory materials is only permitted to take place by the diffusion of gases; thus also, throughout the whole of this important tract, cell proliferation is set up with great promptitude if any abnormal irritant accidentally penetrates beyond the mucous surface. Throughout the cutaneous surface we find, again, elaborate arrangements for the interception and expulsion of abnormal matters before they can reach the blood.

Yet notwithstanding all these fortifications, garrisons, and armaments for the protection of the citadel—so subtle is the tubercle bacillus, that it succeeds in gaining admission to the organism by all these portals. Although repelled, expelled, blockaded, imprisoned, killed, cremated, thousands of times, every now and then it succeeds in establishing a domiciliation in the tissues, where it may become encapsuled and indefinitely localized, or, breaking through its prison walls, it may gain admission to the blood, or possibly, under peculiar circumstances, it may elude all barriers and directly enter the circulation. Once in the torrent of the circulation, there may begin that mordant-like patterning out of elective or selected tissues which I have already described. But even then it has to run the gauntlet of "the battle of the cells," by which, according to some, it may at last be destroyed.

Unhappily, it too often happens that it finds out some weak point in the organismal defences, and wins the fatal battle in the end. In the faithful observations of the actual facts of bacillary life, in man and other animals, we find full confirmation of the truth of each item of the foregoing sketch.

Cutaneous Portal.—Referring to the introduction of the bacillus by the cutaneous portal, Mr. Barker says:[2] "Pfeiffer's case deserves a passing notice on account of its completeness. The patient was a healthy veterinary surgeon, with a good family history, who, while dissecting a tuberculous cow, punctured the joint of his left thumb. [The normal organismal defences of the skin were forced by the puncture.] The wound soon healed, but was followed by induration of the scar, and later by swelling of the whole joint, which underwent the typical changes of scrofulous synovitis, but without the

formation of sinuses. Some months later the patient began to show signs of pulmonary phthisis, which rapidly increased, and he died of this disease a year and a half after the wound of the thumb. . . . The joint, on being laid open, showed all the destructive changes of scrofula both in the bones and synovial membrane; and in the latter, as well as in the broken-down material which filled the intervals between the bones, an unusually large number of tubercle bacilli were found. The microscopic appearance of the diseased tissues was also typically tubercular. Such cases as those just described, leave no room for doubt that tubercle can be inoculated in any part of the surface of the body, and may spread from the original point of entry throughout the whole system, until a fatal amount of general disease is produced."

Alimentary Portal.—With regard to the admission of the bacillus by the alimentary portal, Dr. Woodhead says[4]: "It has, of course, often been objected that if all tuberculous cattle gave tuberculous milk, the human race would run a risk of being rapidly exterminated. But it may now be maintained, as the result of most careful clinical observation, that it is only when the functions of the intestines are interfered with, and when, in consequence, there are temporary or permanent alterations in structure and in the chemical constituents of the fluids and gases in the alimentary canal that tubercle bacilli can make their way unaltered through the epithelial barrier. . . . It is not only the intestine itself, however, that is affected by these functional disorders. The mesenteric glands are also placed at a great disadvantage. This may be easily understood when it is remembered that the slightest irritation in any portion is almost immediately followed by changes in the lymphatic channels and glands. Every one is familiar with the peculiar condition of enlargement, congestion, and succulence that is found in such glands. This condition must be looked upon as the result of stimulation; the cells are roused into greater activity, they proliferate more rapidly, and take up the foreign matter; the gland as a whole acting as a kind of sieve. In this process, however, the store of resistant energy, if such a term might be used, is gradually diminished, and should tubercle bacilli find their way into the gland during or shortly after this extra stimulation, they run less risk of being destroyed by active epithelioid and lymph

cells, than when these cells are not already partially exhausted. This is true in greater degree only in the glands than in the other tissues. Every one engaged in practice could recall numerous cases in which tubercular disease of bone or synovial membranes, etc., has followed on measles, scarlet fever, small-pox, and similar conditions, or in which an attack of typhoid or intestinal mischief of some form has been looked upon as marking the date, soon after which serious tubercular mischief was developed. All this should be considered, leaving out of account the fact that healthy glands might find it difficult to cope with any large number of bacilli finding their way through the unhealthy surface. It should be borne in mind, also, that during the growth of the individual these glands are, under normal conditions, called upon to do much more work than during later life, owing to the fact that in the building-up process in the body there is excess of nutritive material required for the tissues, in consequence of which the secondary digestional function, which the lymphatic system performs, is provided for; but in imperfectly and badly nourished children this is not the case.[9] In the first instance the glands act as vital filters, but should the stimulation be too great, there comes a period at which they are destroyed; they degenerate, and may become encapsuled in fibrous tissue, or they may suppurate and so become foci from which other areas are affected. Where this sequence is preserved there is a still greater danger at the second stage. So long as the gland is active, the irritant material passed along to it is so modified that it is removed almost inert, but as soon as the degeneration sets in the area connected with that gland is much more liable to ulceration, especially in the case of children."

Respiratory Portal.—Speaking of the admission of the bacillus by the respiratory portal, Mr. Barker says:[2] "Ever since the disease (tuberculosis) was first defined, the respiratory passages have been recognized as the point of attack at which the human body most frequently suffers, and clinical and pathological experience has shown that it is from this tract of mucous membrane that the infection most commonly spreads to the other parts of the system. The mode of this infection has so often been demonstrated by experiments on animals as to require only a passing notice here. Not only

has it been shown that the injection of phthisical sputa, or crude tuberculous matter, or the bacillus tuberculosis, derived pure by cultivation from it, into the air passages will produce typical tuberculosis of the lungs, but it has been observed that even healthy animals of certain species simply living with others thus affected speedily develop the same disease; and again, that healthy animals placed in an atmosphere contaminated by tuberculous dust are soon similarly affected. Not only has all this been proved by many independent observers, but the various stages of the disease, from the first initial lesion of the catarrhal mucous membrane to the fully developed general tuberculosis, have been traced step by step in animals thus dealt with. And when we come to consider these experiments, and to compare their results with our clinical experience of phthisis in the human body and its minuter pathology, we cannot doubt for a moment that its etiology is the same in both cases."

Dr. Woodhead[4] says: "It is now generally accepted that all cases of rapid infective phthisis are the result of the action of the tubercle bacillus, although it cannot be denied that certain lesions may be produced as the result of the action of other and non-specific irritants. In these conditions, however, tubercle is very frequently associated with some other lesions, and it is often extremely difficult to say which changes are due to the one and which to the other. For instance, it has long been held that in stonemasons' phthisis all the characteristic lesions met with in chronic tubercular phthisis are present, but my experience of stonemasons' lung has been that, along with the chronic interstitial and arterial changes set up by the stone particles, there are structural alterations which can be accounted for only on the assumption that they are of tubercular [bacillary?] origin; and in some few cases, in confirmation of this, the presence of tubercle bacilli has been demonstrated in certain of the new growths." (Those subjected to the causes of grit-phthisis are, of course, liable to be subjected to those of bacillary phthisis also, and from the condition of their health are specially predisposed to infection; but the concurrence of the two diseases does not prove their identity. See Dr. J. C. Hall, "On the Trades of Sheffield," Second Edition, 865.) In Julius Arnold's observations on the course taken by dust when inhaled into the respiratory tract,

he "pointed out: (1) the very important part played by the walls of the small bronchi and their terminal passages in the disposal of inhaled dust particles. (2) There is the alveolar epithelium, which, under the influence of any irritant material, undergoes proliferation more or less rapid—this in certain cases terminating in what we know as catarrhal pneumonia. Here again, in proof of the statement, take what may be seen under the microscope when particles of colored dust have been inhaled into the lungs. The cells lining the alveoli are seen to be in an active state of division, some are still adherent to the wall of the air vesicle, and in these small particles of the pigment may be seen imbedded in the protoplasm. The epithelium in this position is a structure which may be attacked by the tubercle bacilli, just as in it coal or other particles may be found. Passing still further, and following the course taken by the pigment granules, the lymph spaces around the air vesicles are reached, then the lymphatics in the interstices and interlobular tissue, the peribronchial and perivascular lymphatics, and, lastly, the glands at the root of the lung, either directly or by the deep layer of the pleura, over the surface of the lung, and so to the root. As may be seen on reference to a section of coal-miner's lung (exhibited), the pigment—in this instance a material which gives rise to little irritation—is carried to every part of the lymphatic system, and is seen to have accumulated in very considerable quantities along the lines of the septa, around the bronchi and blood-vessels, and in the deep layer of the pleura. On microscopic examination, the pigment acting as an irritant, and so giving rise to a slight excess of fibrous tissue in all those various positions, may also be seen. Lastly, the small points of lymphatic tissue which occur at intervals along the lymph channels, first described by Burdon Sanderson, then by Klein, Arnold, and others, are the seats of pigmentation. Tubercle formation (see p. 374), also may be met with in any of these positions." [On the subject of grit tuberculosis see (1) Prof. Nothnagel's observations on the passage of cells from the alveoli into the interstitial tissue, related, p. 168, in the second edition of the Author's work On Loss of Weight, etc.; (2) Dr. Osler's (Montreal) description of microscopical examination of miner's lung, "Dobell's Reports on Diseases of Chest," 1876; (3) Dr. Noble Smith's beautiful plates of anthracosis in Dr. Shepherd's Gul-

stonian Lectures, 1877; (4) Introductory chapter on the perivascular system, in the third edition of the Author's work on Winter Cough; (5) Klein's "Anatomy of the Lymphatic System;" (6) Williams on Consumption, *op. cit.*, 1887, etc.]

Genital Portal.—With regard to the genital portal, M. Cornil reported to the Paris Congress, July, 1888, that "he had twice examined a uterus removed during surgical operation; in each case the uterus was tuberculous, in one instance contagion by coitus was out of the question; the disease was seated in the Fallopian tubes; in the other the cervix was attacked, also the surface of the uterine mucous membrane beneath the epithelium presented tuberculous granulations. As the annexed organs, which were removed, did not present any tuberculous lesions, M. Cornil considered that this was a case of the transmission of tubercle by coitus. In order to ascertain the possibility of this mode of contagion, he introduced some bacilli into the uterine cavity of guinea-pigs [the "Lancet" report says into the vagina]. The first result was catarrh of the cervix uteri, afterward a considerable increase of lymphatic cells containing bacilli. On the fourth day small tubercles were observed beneath the stratified epithelial cells; the tubercles then invaded the connective tissue between the uterus and the bladder. M. Cornil concludes that sexual intercourse may furnish a means of contagion." ("Brit. Med. Journal," July 21st, 1888.)

From the foregoing illustrations we have seen how the bacillus tuberculosis may be introduced by all the chief portals to which I have referred; and this list might, no doubt, be increased by the addition of other less frequent means of access. But those which I have given are sufficient for our present purpose.

CHAPTER III.

It has been one of the most serious obstacles to advance in the study of tuberculosis, that it has been too exclusively identified with tuberculization of the lungs, *i.e.*, with pulmonary consumption. Thus the area of observation has been narrowed, and the bases for wide generalizations as to cause and effect have been shut out of the investigator's field of vision. I have long striven to divert the attention of the profession from this too narrow view, and, as is well known, I have specially endeavored to direct the thoughts of observers to the alimentary tract, as one, at least, of the most important sites of the *fons et origo mali.* The late Dr. Hughes Bennett, of Edinburgh, made most valuable contributions in the same direction (1853–59); and Mr. Jonathan Hutchinson, before he was known to fame, published an admirable paper "On the Form of Dyspepsia which often Precedes and Attends Phthisis" (1855); and in 1864 Dr. E. Symes Thompson published in the "Medical Mirror" a most intelligent article on "Indigestion in Early Phthisis," beginning with the words, "Deranged digestion is one of the most common deviations from health that ushers in tubercular disease." These and other similar observations and suggestions will be more fully appreciated, now that we have learned how easily the bacillus tuberculosis (unknown at the time those works were published) enters by the alimentary portal, and how easily and fatally, once within these gates, it finds admission to every part of the citadel.

On the present occasion, instead of seeking for illustrations of the clinical history of tuberculosis, as is most usual, among cases of pulmonary consumption, I shall take them from the histories of tuberculous joint disease as recounted by Mr. Arthur E. J. Barker,[2] whose observations on joint disease are applicable to tubercular diseases throughout the body, and the joints afford a very favorable field for their investigation.

Mr. Barker says: "It is well known that the disease generally finds its most fertile soil among the very young, probably, because, among other reasons, the vitality of the young cells is unable to cope with the invading organism; and if this be true, we should expect to find that, with the bacillus once in the blood, those tissues which are youngest, or in other words approach most nearly to the embryonic type, should be most liable to suffer from its attacks. Now such embryonic tissue is abundant in childhood, and most so at those spots where growth and physiological activity are greatest. . . . If one were asked, then, to suggest the most potent factors in the predisposition of joints to tubercular disease, one would be inclined to say, in the first place, the inherent low vitality of the youngest tissues; and, in the second, high physiological activity. . . . One point, I think, may be accepted as almost certain, namely, that tubercular disease starts more frequently in the bone alone in childhood, and in the synovial structures alone in adult life. If this preponderance of primary synovial disease in the adult be a fact, as I think it is, it would be best explained by the cessation of physiological activity in the bones, while it continues in the synovial tissue, the other factors in the production of the disease remaining unaltered. From the above considerations it would appear that there are several kinds of predisposition to tubercular disease of joints. In the first place a peculiar debility may be inherited from phthisical or otherwise unhealthy parents, rendering the tissues of the child generally less capable of repelling the attacks of the parasites (bacilli) than had it come of a robust stock. Next there is the general predisposition of early age, the tissues of the young having less resisting force than those of the mature. Thirdly, certain localities in the body appear to be particularly open to the attacks of the organism (bacillus), owing probably to the hurried physiological changes involved in rapid growth, which are going on in them—changes which are associated with the presence of abundance of almost embryonic tissue and great vascularity. . . . Finally, there is the predisposition of external injury, using the term in its widest sense. In the case of the joints, injury may render their tissues more open to the attacks of the bacillus tuberculosis circulating in the blood, in several ways, acting singly or together. In the first place the violence may cause more or

less active inflammation, with exudation of plastic material into the tissues around. Such exudation material resembles embryonic connective tissue, and seems to have very little power to resist the attacks of the organism; in other words, it offers a favorable soil for its growth. This we have seen in the case of the inoculation of the rabbit's eye with tubercle by Baumgarten, where the bacilli spread more rapidly into the granulation capsule, and along the cicatricial tissue of the corneal wound than in any other direction. Further, the increased blood-flow to an inflamed part would also, *cœteris paribus*, involve a greater supply of bacilli to it than if the vascularity were normal. Again, without any active inflammation, or following upon it, vascular atony may result from violence to a joint, and passive congestion of its tissues be the result, as is so often seen clinically. In this condition not only is the general nutrition of the part damaged, but the slowing of the blood stream in the smaller vessels, amounting often almost to stasis, offers a better opportunity for the deposit of the bacillus in the parts around than is afforded with a normal circulation. And here, too, the greater amount of blood entering the congested area would bring a larger army of organisms to the attack. Finally, injury to a joint will often produce more or less extravasation of blood into its tissues, which remains for a time unabsorbed. With this blood the bacilli, too, escape from the vessels, and in their new position, withdrawn from the current of the circulation and in a state of rest, they meet with the most favorable conditions for further growth, and soon form large colonies. This last-mentioned effect of injury is probably a most potent factor in the determination of the points at which tubercular disease shall start throughout the body, and will best explain its appearance in many unusual situations. Those who are not familiar with Schüller's experiments bearing on this point will do well to give them their careful attention." Parasitic, tubercular or scrofulous disease, "in its modes of spreading by contiguous infection through the lymphatics, and by the entrance of its virus into veins, offers, as we have seen, a close parallel to the modes of generalization of the so-called malignant neoplasms. . . . But that, in spite of its capacity for wide dissemination, it may remain for a long time localized in a joint, the rest of the system continuing unaffected, I need not undertake to prove;

every clinical observer is thoroughly familiar with the fact. (Prof. McFadyean and Dr. Woodhead found that a cow's udder might be affected with bacillary tuberculosis [as proved microscopically], without the slightest falling off in the general condition of the animal, so long as the disease remained local.—"Lancet," July 14th, 1888.) On the other hand, we must all have been impressed with the experience that in many cases, after a long period of quiescence as a purely local affection, tuberculosis has rapidly become general. We must have noticed that this has usually followed upon some injury to a fully developed caseating focus, or the bursting of some caseous abscess, or, what is perhaps of greater importance for us, as surgeons, to remember, it has followed soon after a surgical operation upon a tuberculized part."

From Wartmann's statistics of 837 resections, it appears that in at least 10 per cent of the total number of deaths following operation, rapid general miliary tuberculosis supervened. And I may refer here, in passing, to the frequency of a sudden and disastrous descent in cases of fistula in tuberculous patients after surgical interference.[9] Mr. Barker says that "not infrequently in examining a tubercular focus in the cancellous tissue of the end of a bone, we observe that its spread has been prevented, as it were, by a process of sclerosis in the tissue around. . . . If such a sclerosis exist in any given case, we should be careful, in gouging out the diseased matter, not to go beyond this barrier, erected by the natural processes against the spread of the disease," and in a previous passage he says: "Now as long as this limiting layer of plastic exudation remains undisturbed and unbroken . . . we are justified in believing that a very considerable obstacle is offered to the dissemination of the bacilli within it, and that the disease remains, in many cases, almost or altogether localized. But if in any way this barrier is dissolved by suppuration or broken through by violence, a means of escape for the bacilli is provided, and more or less generalization of the disease takes place. The organisms may be distributed over a wider local area, may be carried along the lymphatics to be arrested partially or completely in the glands; or may gain access to an eroded or torn vein, and thus be carried in overwhelming numbers into the general circulation, as shown by Weigert." That bacilli may be got rid of from the general system by ex-

cretion, Mr. Barker thinks, has been proved beyond doubt, for they have been found in several of the secretions, and it is also probable that "to a certain extent, when the dosage is small, and the individual vigorous, they can be totally destroyed within the body before they find a suitable soil for their growth."

At the Paris Congress, July, 1888, M. Nocard detailed some very ingenious experiments, from which it would appear that the muscles have the power to destroy or "digest," as expressed by M. Nocard, the bacilli in such a way that the meat of animals affected with generalized tuberculosis presents but little danger. Thus four cats ate with impunity the flesh of a tuberculous cow, while a fifth cat, that had eaten a lymphatic gland of the same cow, succumbed in a very short time to experimental tuberculosis. ("Lancet," Report, August, 1888.)

We have now seen that it has been demonstrated in various ways that the tubercle bacillus may be introduced into the animal organism by all the portals that I have enumerated and discussed, but that by far the most usual and important are the respiratory and the alimentary portals. We have also seen that it may be thus introduced again and again without succeeding in effecting a domiciliation; and that even when it has succeeded in finding a domicile it may yet be restricted to a limited area for an indefinite time, by the natural protective processes; also, that it may never succeed in penetrating beyond this localized domicile; and that, under certain exceptional combinations of circumstances, the organism is itself capable of exterminating the bacillus after it has effected a domiciliation. But it is, unfortunately, quite certain that so long as any bacilli or spores of bacilli are allowed to continue their domiciliation, there is no security against their breaking through the boundaries of their limitations and extending their operations to an unlimited extent. It appears, also, to be demonstrated that, under certain conditions, the bacilli or their spores may be so introduced from the first as to defy all known possibilities, natural or artificial, of limitation or extermination.

It is the variations—

(A.) In the portals of admission;

(B.) In the localities in which domiciliation is effected;

(C.) In the areas of limitation;

(D.) In the means by which escape from these areas is obtained;

(E.) In the potency or impotency of the organismal efforts to imprison or expel the enemy;

(F.) In the auxiliary forces raised for the assistance of the regular forces;

(G.) In the consequences of success or failure in these operations—that account for the variations in the clinical and pathological features of different cases of tuberculosis, due alike to the introduction of the bacillus tuberculosis.

But these clinical and pathological variations are still further multiplied and complicated by the fact that other bodies beside the tubercle bacillus may enter the animal organism by the same portals, and set up processes closely resembling, if not analytically the same, as those due to the tubercle bacillus itself. Thus, as I have stated—quoting from what I wrote in 1880, just before the discovery of the bacillus —any irritant capable of setting up in the living tissues "a foreign mass made up of hyperplasia of adenoid tissue, epithelial and connective-tissue proliferation, angioplasia, and other attempts at repair of disintegrated tissue, and the results of degradation and decomposition of these new formations" may produce symptoms which, when fully established, are clinically and pathologically so analogous that the differentiation of the original irritative cause can hardly be made clear without the aid of chemistry and the microscope, and of a critical inquiry into the etiology of each case. I attempted to describe[1] the various irritative causes of the heterogeneous mass called tubercle when affecting the lungs, as "a large and important group embracing all foreign substances which find their way into the peri-vascular and peri-alveolar tissue of the lungs, and by their irritation there" set up the changes just detailed; and I enumerated the following items as "some of the principal constituents of this important group.

"(*a.*) Lampblack, coal, steel, stone, flint, and other substances inhaled by workers in various trades. (See Chap. II.)

"(*b.*) The products of inflammatory destruction of tissues.

"(*c.*) The products of catarrhal affections (especially in scrofulosis).

"(*d.*) The débris of tissues disintegrated by the extravasation of blood, and possibly the débris of the blood so extravasated.

"(*e.*) Albuminoid materials disintegrated by oxidation in true tuberculosis (hypothetical)." And I added that this hypothetical item (*e.*) "is the only one in the list which involves that initial loss of weight characteristic of the true first stage of consumption. In my opinion this is the irritant which, in true (or pure) tuberculosis, sets up the hyperplasia of adenoid tissue, the cell proliferation, angioplasia, etc., and their results, so well described by Portal, Virchow, Sanderson, Rindfleisch, Charcot, Malassez, and others. But whereas they—ignoring the clinical fact of initial loss of weight—place these processes first among the pathological changes of tuberculosis, I, on the other hand, believing it to be essential to take cognizance of the initial loss of weight and of strength give precedence to the (hypothetical) disintegration of albuminoid matter by oxidation *in situ*, of which the hyperplastic, proliferative, angioplastic and other changes are but the effects." In 1885, in a chapter on "Disease Tracts"[10]—written after the announcement of Koch and Baumgarten's discovery—in the following enumeration of the "morbid states connected at different stages with the phenomena of phthisis," I added the bacillus to the list, as follows: "(1.) Abnormal physiological states, principally connected with the digestive and assimilative processes, producing constitutional decline. (The true first stage.)

"(2.) Disintegration of tissues and organs dependent upon abnormal nutritive processes.

"(3.) Abnormal growths of adenoid, epithelial, and connective tissues, and the results of abortive formative processes.

"(4.) Catarrh, hyperæmia, inflammation, gangrene.

"(5.) Decomposition of the débris of disintegrated tissue, of the products of disease, and of the abortive attempts at repair above referred to.

"(6.) General septic disease, or septicæmia.

"(7). Secondary local diseases of septic origin (auto-infection).

"(8.) The presence of microbes (bacilli)."

It will hardly be disputed that any one of the foregoing list of interstitial irritants is capable of setting up the formation of an abnormal conglomeration of matter such as we are accustomed to call "tubercle," and that, unless it should be proved that "giant cells" have some typical connection with

the bacillus tuberculosis, we have no means at present of determining chemically or microscopically whether or not the irritant, in any given case, was the bacillus, except by discovering the presence of the microbe itself or its spores. Nor will it be disputed that the list I have given fairly represents "the morbid states (including the bacillus) connected, at different stages, with the phenomena of phthisis."

But now that the existence of a bacillus tuberculosis is undoubtedly established, it may well be questioned whether the item (*e.*) hypothetically described as "albuminoid materials disintegrated by oxygen"—which, as I have said, is the only one in the list which involves the initial loss of weight characteristic of the true first stage of consumption—ought not to be henceforth considered as inseparable from the bacillus itself, the first palpable effect of which is molecular disintegration.

But we are met by this difficulty, that, notwithstanding all the elaborate work spent upon the bacillus, no clear evidence has yet been produced that its presence in the animal organism excites any symptoms of constitutional decline, until, like other irritants, it has set up local disease. We have seen that it has been experimentally proved that, in bacillary tuberculization, no tuberculous matter is formed until a definite interval has elapsed after the local domiciliation of the microbe.

But we have not learnt by this experimentation that any symptoms are excited by the bacillus during this eventful interval. In Baumgarten's experiments, on the fifth day after inoculation scattered bacilli were seen in those portions of the cornea nearest to the point of inoculation, but without so far producing the slightest deviation from the normal histological texture. On the sixth day the bacilli were present in great numbers throughout the tissues of the cornea, which were still, however, practically unchanged. But at certain points, especially where the bacilli lay in dense groups, certain newly formed cells were to be seen having the general appearance of epithelial or epithelioid elements. A little later every intermediate form, up to the perfect type of miliary tubercle with their lymphoid and epithelioid cells, were to be seen.

On this subject we have also the important observations of Klein (op. cit.) that "in the most favorable cases (anthrax and tuberculosis) a single organism introduced into a suitable locality in the animal body will be capable of starting readily

a new brood. But in other cases it is necessary that an appreciable number of the organisms be introduced in order to start a brood. . . . The period between the time of introduction of the organism into the body (blood, skin, mucous membrane, subcutaneous tissue, lungs, alimentary canal), and the production of the new brood large enough to produce a definite effect, locally or generally, corresponds to the incubation period of the disease, and, as is well known, there is in this respect a great difference in the different dieases. . . . In tuberculosis, after the introduction of the bacillus tuberculosis into the subcutaneous tissue, the nearest lymph glands [11] show the first signs of swelling and inflammation after one week, or even later, and the general disease of the internal viscera does not follow until one, two, or more weeks have elapsed. This is also borne out by the observations of the behavior of these bacilli in artificial cultures." The result of all these observations and experiments is to bring us back to the great clinical question: In what relation does the tubercle bacillus stand to the initial loss of weight and of strength—that pre-tubercular stage so fraught with interest in its connection with preventive treatment—the constitutional decline of bacillary consumption?

In considering this question we must ask (A), on the one hand:

(1.) Are these general symptoms due to the presence of the bacillus in the general blood current?

(2.) Are they antecedent to its localization?

(3.) Are they subsequent to its localization, and due to its local ravages before these have advanced to a stage at which we can detect them by our present diagnostic appliances?

And, on the other hand (B), we must ask:

(4.) Are these general symptoms altogether antecedent to the domiciliation of the bacillus either in the blood or in the tissues?

(5.) Do they proceed from a local defect or from a constitutional defect?

(6.) Do they indicate a constitutional or local defect which invites or attracts the bacillus to effect a domiciliation—which, in fact, provides those mordant-like conditions referred to in pp. 381, 382, those feeding grounds for the bacilli of which I shall soon have to speak?

(7.) Or do they indicate a constitutional or local defect which—without specially "inviting or attracting" the bacillus—simply renders the general or local organismal defences impotent to repel the invader?

These are burning questions in relation to practical medicine; and it is evident that the proper preventive and curative treatment of "The True First Stage of Bacillary Consumption," must be utterly dependent upon their being properly answered.

Crookshank says (Second Edition of Manual) that "bacteria are never to be found in the blood in health. The organs removed from a perfectly healthy animal, with the necessary precautions, into sterilized media, can be kept indefinitely without undergoing putrefaction or giving any development of bacteria." (See also Watson Cheyne.)

Klein says (op. cit.): "One of the most important points, and the most difficult of comprehension, is the power of the pathogenic organisms to resist the influence of the healthy tissues of the living animals, a power which . . . is not possessed by the non-pathogenic organisms. A careful analysis shows at the outset that this power of the pathogenic organisms is not possessed by them indiscriminately, for while a particular species is in some animals capable of overcoming the influence of the living tissue, *i.e.*, to multiply and produce the particular disease; in other animals it is not capable of doing this, and hence the animal remains unaffected—it is said to be not susceptible to the disease." "Now," says Klein, "where are we to look for this difference in behavior? . . . The tissues *per se* cannot be said to possess any inimical action on the organisms. The living condition *per se* can also not have this power, since we see that the power to overcome the influence of the living tissue is precisely the great distinguishing character of pathogenic organisms."

I fear we must conclude from the results of the forcible experimental introduction of bacilli into the bodies of healthy animals, that the organismal forces alone, even in health, are not, as a rule, capable of either expelling or destroying the bacilli, or of preventing their domiciliation, when introduced under certain conditions; and that to effect these ends, artificial help is needed of every kind that science and art can devise. And this gives the most emphatic importance to the

identification and treatment of the pre-tubercular stage, and still more to the pre-bacillary stage.

We must not allow the microscopical finesse of the subject of microbes and the difficulties in observing their habits to cause us to forget that, throughout the vegetable and animal world, observations, made under circumstances less perplexing than those connected with molecular and atomic rearrangements of matter, have always taught that the first law which dominates habitat is the presence of appropriate food, and that the second law is the presence of appropriate temperature.

"Where the carcass is, there will the eagles be gathered together," is but the crude enunciation of the first vast law of habitat throughout the world. The converse is alike true, for where the carcass is not, the eagles will not be gathered together.

What, then, is the food and what the temperature which together determine the habitat of the bacillus tuberculosis?

We have now many reliable data from which to answer this question. Speaking of the artificial cultivation of bacteria out of the animal body, Klein says (op. cit.): "The materials which I now prefer for the cultivation of pathogenic and septic bacteria are: (1) Broth (beef) and peptone; (2) Solid nutritive gelatin; (3) Solid agar agar (Japanese isinglass), peptone meat extract; (4) Fluid and solid serum of blood; all faintly alkaline." "In 'fractional cultivations' (the attempt to isolate by successive cultivations the different organisms that have been growing previously in the same culture) . . . most probably only one species (of organism) in each tube, *i.e.*, the one that grows best in this particular medium and at this particular temperature, will be found to have increased to an enormous extent, while the others have made little or no progress." Speaking of the tubercle bacillus especially, he says: "The bacilli grow well at a temperature varying between 37° C. (98.6° F.) and 39° C. (102.2° F.) . . . Owing to the fact that tubercle bacilli require for their growth high temperatures, it is evident that, unlike some other pathogenic organisms, they do not thrive in the outside world in temperate climates." It is therefore "necessary, in order to study more accurately the life history of the septic as well as the

pathogenic organisms" out of the body, to provide an artificial incubator by which the requisite temperature may be maintained.

Now it most unfortunately happens that there is no "incubator" so perfect as the animal body itself; for in it a temperature of 98° to 99° F. is constantly maintained, and this temperature is readily raised to 102° or 103° F. by the disturbances of disease; while it is equally unfortunate, as it is equally true, that all the selected artificial "cultivation materials" above enumerated, are analogues of the far finer media provided in the blood, the tissues, and the secretions of living animals.

Another most important consideration in relation to habitat is the fact that "all bacteria, pathogenic and non-pathogenic, require for their growth and multiplication oxygen, which they either obtain from the media in which they grow, and which oxygen is dissolved in those media, or if this is consumed or absent it is obtained by the bacteria in the process of the chemical decomposition of the carbohydrates and proteids present." (Klein, op. cit.)

When treating of the disintegration of proteids by oxygen ("On Tuberculosis," 1866, and "On Loss of Weight," 1880), I showed that the relations of oxygen to the circulating media of the animal body are most similar in the lungs and in the lymphatic system—precisely those regions which are most liable to the incidence of bacillary infection. I pointed out that "there is no part of the body traversed by fluids essentially the same as those which traverse the lacteals, mesenteric glands, thoracic duct, and pulmonary artery, and there are no other parts of the body in which the relations between pancreatized fats, albuminoid tissues and oxygen are so similar as in the small intestines and mesenteric system and in the lungs. . . . In the lungs the oxygen is in the air of the alveoli, and the material to be oxidized is in the capillaries of the alveoli; in the small intestines and mesenteric system the oyxgen is in the capillaries of the glands, and the material to be oxidized traverses the glands. Hence we find that the disintegration of proteids by oxygen (or by bacilli?) which occurs first in the lungs, in both children and adults, is next most frequent in the small intestines and mesenteric system—except when it falls upon the bronchial glands of children, an excep-

tion susceptible of explanation consistently with the foregoing statements."

Now, if we regard the bacillus as the disintegrating factor we shall find that it may occupy the same relative positions with regard to the disintegration of proteids as the oxygen in the above description, *i.e.*, if admitted by the respiratory portal, it will be in the air of the alveoli; if admitted by the alimentary portal, it will be in the lacteal and lymphatic vessels; if it enters the general blood current, it will be in capillaries of the lungs and glands. In each case, therefore, the bacillus would have two separate directions from which to operate upon the proteids; but the symptoms would necessarily differ in the following manner:

If introduced by the respiratory portal, local lung disease would precede constitutional decline. If introduced by the alimentary portal into the lacteal and lymphatic system, local gland disease would be coincident with disturbed nutritive functions, and constitutional decline would be coetaneous with it, or might precede it by a short interval, in consequence of obstruction of the absorbents by the bacilli before tuberculization was set up. If introduced to the general current of the blood primarily, any or all of the organs of the body might be attacked simultaneously, or in the order of their mordant-like elective affinity for the bacilli. It seems reasonable to suppose that this general tuberculization might be preceded by a period of initial constitutional decline, due to the presence of the bacilli in the blood. But, as I have more fully pointed out, we have at present no definite evidence that such is the case, or indeed that any abnormal symptoms are produced by the bacillus till it has established tissue domiciliation.

In addition to the above, there is yet another position to be considered with regard to the bacillus in the blood, viz.: that, after it has effected a local domiciliation and has remained for a longer or shorter time as a purely local affection, it may make its way into the vascular system and overwhelm the patient with general tuberculization, as pointed out by Weigert, Wartmann, Barker and others. But as a rule, this will only occur after the local disease has sufficiently advanced to have set up a secondary constitutional disease; so that it does not come under the head of initial loss of weight and strength characteristic of the true first stage of consumption.

CHAPTER IV.

FROM all that has gone before we have learnt that bacillary tuberculosis may, in all probability, be:

1. A primary blood disease, bacilli entering the blood first.

2. A primary local disease, bacilli introduced into the tissues first.

3. A secondary blood disease, bacilli first domiciled in a part and then getting from it into the blood.

4. A secondary local disease, bacilli first entering the blood and then becoming domiciled in a tissue.

5. A combination of all the above conditions, with the addition of suppurative and septic processes.

In its position as a primary local disease (No. 2), and so long as it remains localized, it takes its place beside the other local irritants enumerated at pp. 394, 395, capable of setting up the formation of the heterogeneous foreign mass which we call "tubercle." But the bacillus can never be divested of a specific character which other irritants do not possess, viz., its vitality. This, at all times and under all conditions, adds to its dangers as a local irritant the far greater danger of its becoming generalized. In addition to this its vitality gives it an extraordinary virulence, over all other irritants, from its inherent capability of multiplying and gathering force as it advances. Doubtless, it is this quality which makes it set up cell-proliferation, etc., etc., so much more promptly, and in so much greater abundance than any of the lifeless irritants.

But in addition to these causes of virulence there is yet another item to be considered which is more important than all, viz., that in common with every other vitalized entity it must have sustenance.

Whether the bacillary army is bivouacking or advancing, it must have sustenance; and as it does not carry its rations with it, they must be obtained on the road. As the bacillary host is continually reinforcing itself by internal multiplication

the demand for this sustenance is ever multiplying at a compound rate.

The questions—(1) What is the nature of this sustenance? (2) By what modes is it obtained?—bring us to the very core of the subject of this treatise.

We have become so familiar with these minute vitalized entities (microbes) by the perfection of our chemical, microscopical, and spectroscopical appliances, that it is almost as difficult for the mind to formulate just ideas of their relation to the world in which both they and we "live and move and have our being," as it is to realize and weigh the conceptions that "the ponderable universe consists of detached stellar groups like that of our milky way, and like that of the nebula in Andromeda; . . . that the vast celestial spaces between these groups are beyond the confines of our universe—that they are, in fact, outside the universe of ponderable matter;"[12] "that electro-magnetic actions are due to a medium pervading all known space; that it is the same medium as the one by which light is propagated; that non-conductors can, and probably do, . . . transmit electro-magnetic energy, and that, by means of variable currents, energy is propagated into space with the velocity of light." (Hertz's Experiments.)[13]

The subject of Mr. Crookes's address to the Chemical Society (1888) is intimately related to that of the sustenance of microbes: "The possible existence of bodies, which though neither compounds nor mixtures, are not elements in the strictest sense of the word, but 'meta-elements,'—that what we are accustomed to consider an element is an aggregate of slightly dissimilar atoms of the same primordial material, an elementary group."[14]

The contemplation at once of the vast conceptions of the "detached stellar groups" of the astronomer, the "electro-magnetic ether waves" of the physicist, and of the infinitesimal "elementary groups" of the chemist, brings before the mind the extremes of subtlety which modern refinements in chemistry, spectroscopy, and electricity have imposed upon our investigations of force and matter.

It is evident that in considering so subtle a question as the sustenance of microscopic vitalized entities, the first thing we have to do is to divest our minds of all our customary ideas

of nutrient materials, as they occur in their coarse and ponderable forms. Sir Henry Roscoe[15] says, "Before chemists can answer as to the course of nature's action, they must find out the molecular constitution of the various bodies by synthesis in the laboratory"; and Professor Thiselton Dyer replies that "the building up of substances from simple elements (in the laboratory) does not throw any light upon their occurrence in the living body." Professor Gladstone, speaking of colloids and crystalloids, points out that "although proteids are colloids, the substances of which they are made up are crystalloids—which are believed to be very complex molecules." Professor Kühne[16] considers that "movement is an attribute of protoplasm both in animals and plants, and it is impossible to have a cell which does not preserve it at some time or other. This movement is a spontaneous activity which may go on in opposition to gravity, and which overcomes frictional resistance. Its cause can only be internal, and can only consist in chemical processes taking place within the protoplasm itself;" while Professor Gardner "has considerable difficulty in thinking of protoplasm as a substance, and is of opinion that the phenomena of life consist in an infinite arrangement and rearrangement of an exceedingly complex system of molecules, and that protoplasm will ultimately be found to be the development of energy, and nothing tangible"!

Now what I wish to suggest, as an almost inevitable conclusion from all that we at present know of the life history of the bacillus, and the conditions of its existence, is that, in seeking to find out the nature of its sustenance and the means by which this is obtained, it is to the subtle regions of elements, meta-elements, protoplasm, and complex arrangements and disarrangements of molecules, that we have to direct our search, not among such coarse materials as even the minutest cells that the microscope can detect or the ordinary formulated atomic compounds of the chemical laboratory.

A startling evidence of the necessity for this conclusion, in the present state of our knowledge, is found in the statement of Klein (op. cit.), that "what the chemical influence of pathogenic organisms on animal tissues may be is not yet known; and even when they grow outside the body, *i.e.*, in artificial cultures, it is not yet known what their chemical effect on the nourishing material is, except that, as is the case with all

other organisms, putrefactive and pathogenic, they continue to grow and multiply as long as there are present the necessary substances, *i.e.*, until the medium is exhausted." ("Pathogenic organisms cannot thrive if proteids or allied compounds and certain organic salts are absent."—Klein, op. cit.) I have already related the fact that when tubercle bacilli are inoculated into the normal tissues of a healthy animal, no detectable histological changes occur for several days. But that these days are secretly eventful in the highest degree, is shown by the rapidity and activity of the generation of microbes and of the histological changes which immediately follow. It is impossible to avoid the suggestion that these eventful days are occupied in the disarrangement of molecular matter—perhaps in the disassociation of the constituents of Crookes's "meta-elements or elementary groups;" perhaps in the disintegration of the complex crystalloid molecular constituents of proteids—in order to provide the necessary sustenance for the multiplying army of invading microbes before it commences its advance. It is only after it has been so provided with the necessary sustenance, and so enabled to reinforce its numbers, that its attack is recognized by the invaded tissues. And, then, in the attempts to bar the progress of the advancing foe, all those measures of organismal defence are set in motion which pile up the heterogeneous mass which we recognize under the name of tubercle.

When we find the bacilli swarming into the newly-formed cells, it is highly probable that they are attracted there in the pursuit of the molecular débris which constitute their means of sustenance, and which have been taken up by the cells in their attempts at the reconstitution of normal elements and at the construction of defences against the microbes. "It is curious to see," as Dr. T. Williams observes, "how the bacilli are attracted toward them, and are seen in large numbers both around the cells and within the cell walls."

According to Koch the bacilli, by-and-by, disappear again from the giant cells—probably when there is no more to be got out of them. Klein says (op. cit.), that "where in a tissue pathogenic bacteria find the suitable conditions for growth and multiplication, they can do successful battle against the amœboid cells, but where these conditions do not obtain, the bacteria linger and die, and, like other particles, can be swal-

lowed up by the amœboid cells. That the presence of bacteria in the protoplasm of amœboid cells does not indicate that the former are being removed or destroyed and their action neutralized by the latter, but exactly the contrary, is proved in Koch's septicæmia of mice, in bovine tuberculosis, in leprosy, and in other diseases."

Now, with reference to that short interval which I have called "eventful,"—which follows the experimental inoculation of bacilli and precedes observable histological changes, and of which I have suggested that it is employed in the important process of preparing sustenance for the bacilli in their new domicile—we must remember, as I pointed out, that in these experiments the bacilli are artificially and intentionally —that is, forcibly—inserted into healthy tissue in animals constitutionally healthy. It does not at all necessarily follow that this interval would occur, if the introduction of the bacilli was spontaneous. On the contrary, if we call to mind the first law that dominates habitat, viz., the presence of appropriate food, we shall conclude that the spontaneous domiciliation of the bacillus, *i.e.*, its selecting any special locality for its habitat, must be due to its finding there its appropriate means of sustenance.

If, then, we are to regard as essential to the appropriate sustenance of the bacillus, a condition of molecular disintegration of crystalloids and proteids and the like; we are confronted by the important question: How are we to account for the existence of such a condition in the organism, independent of the presence of the bacillus; that is to say, antecedent to its domiciliation? Are there any known circumstances favoring or inducing such a condition?

It is a singular coincidence, if no more, that the result of my long course of investigation into "the nature and cause of tuberculosis,"[17] (before the discovery of the bacillus), was to lead me to suggest as an hypothetical explanation of the loss of weight and of strength—the slow constitutional decline—antecedent to the formation of tubercle, and characteristic of the worst form of consumption—that a disintegration of proteids by oxidation *in situ* took place in consequence of abnormal conditions in the digestive and assimilative functions. And I showed that the points of special incidence of tuberculization, in these cases, were precisely those in which such dis-

integration of proteids would necessarily occur; and that the order of frequency in which these special sites would be affected, corresponded with the order of frequency in the incidence of tuberculization, as shown by clinical and pathological observations.

Supposing, then, for argument sake, that my hypothesis were correct, it would account for: (1) A period of constitutional decline, antecedent to bacillary tuberculization; (2) The establishment of feeding-grounds, provided with the proper sustenance for bacilli; (3) The consequent selection of these special sites by the bacilli for their habitat; (4) The mordant-like action of tissue faults in relation to bacillary domiciliation.

I may venture to recapitulate here the words in which I stated my hypothesis regarding the disintegration of proteids, etc., by oxidation *in situ:*

"The blood becomes deficiently and defectively supplied with fat elements from the food; is unable to afford those required for direct combustion; does not replace those taken up during interstitial nutrition; but, on the contrary, takes up more, to compensate the deficient supply from the food. This having gone on up to a certain point, the fat elements of the albuminoid tissues are seized upon, and these tissues are minutely disintegrated in the process. . . . The word tissues must be understood to include albuminoid materials employed in the construction and repair of tissues, for it is probable that the elements may be waylaid during the process of tissue formationn (oxidized *in situ*). . . . These débris of the combustion of tissues set up adenoid hyperplasia, angio-plastic, epithelial, and other cell proliferation, and all that follows in their track. In this relationship they fall into the ranks of other causes of similar changes. . . . My hypothesis, based as it originally was upon clinical data, framed to explain these data, and entirely consistent as it is with these data, must take precedence of all hypotheses in which they are disregarded. . . . No hypothesis but this attempts to explain how loss of flesh precedes all other changes. I am astonished that so palpable a clinical fact as the loss of flesh, premonitory of tuberculization, should have been entirely lost sight of by all modern pathologists. . . . All their ideas seem based upon the basic clinical fallacy that tubercle precedes constitutional decline instead of following it."

"According to my view, therefore, the order of events is as follows: (1) Deficiency of fat in the blood (this deficient supply of fat from the food is necessarily felt first in the blood of the pulmonary artery, presented for aëration in the lungs); (2) Oxidation and disintegration of albuminoid matters (proteids) *in situ*, with the consequent production of molecular débris. (3) Hyperplasia of adenoid (lymphatic) tissue, the effect of the irritation caused by the molecular débris of the disintegrated albuminoid matter."[1]

Now, it must be observed that this last item (3) is the position in which the discovery of the bacillus may find a place. Supposing, as I have said, for the sake of argument, we assume items 1 and 2 to be correct, and supposing that the special sustenance of the tubercle bacillus is the molecular débris of proteids and their constituent crystalloids—then we find the necessary food provided for the bacillus; and hence, instead of saying, in item 3, that the molecular débris sets up adenoid hyperplasia, etc., etc., and causes tubercle, we should have to say that it determines the habitat of the bacillus, and thus causes tubercle, the bacillus, and not the molecular débris, being the irritant.

I do not assert that this is correct; but it is only fair to point out in this place that it presents still, as I thought it did in 1880, a good solution of the problem: What causes the constitutional decline antecedent to "the worst form of tuberculization," or, as we must now say, to "bacillary consumption?"

But, instead of attributing this molecular débris—this hypothetical sustenance of the bacilli—to the defective absorption of fats, and consequent combustion of proteids *in situ*, as suggested in the foregoing hypothesis; in the light of our present knowledge, we may, possibly, find an explanation of the existence of some such materials, and a consequent "abnormal physiological state"—constituting the special form of constitutional decline antecedent to bacillary tuberculization, —in the spectroscopic discoveries of Dr. MacMunn, of Wolverhampton, referred to in my treatise on asthma (1886), which he has since further extended;[18] and in the discoveries of Selmi, Gautier, Peter, and others, elaborated in the exhaustive treatise of Dr. A. M. Brown,[19] and ably and lucidly epitomized and commented upon by Sir William Aitkin in his recent

brochure "On the Animal Alkaloids, the Ptomaïnes, Leucomaïnes, and Extractives, in their Pathological Relations" (1887). Sir William Aitkin says:

"Gautier has shown that in the dead animal tissues, processes of putrefactive decomposition set in by which certain alkaloids are elaborated from the proteid substances, which by the late Selmi, of Bologna, have been called 'ptomaïnes.' But Gautier has further shown that in the living animal tissues, and that by virtue of their vitality, certain other alkaloids are elaborated which are analogous to the 'ptomaïnes,' and these he has named 'leucomaïnes.' Still further . . . he has demonstrated that in the living animal economy there are elaborated certain azotized uncrystallizable substances, which are as yet undetermined, which we call 'extractives,' 'or extractive matters,' and which are quite as unknown as the *x*, *y*, *z*'s of an algebraical formula (Dr. H. L. Veale). The nature of these 'extractives' has, therefore, still to be found out; but this much we know of them: that while we are assured that the 'ptomaïnes' are toxic, and that the 'leucomaïnes' are also toxic, these unknown 'extractives' are more toxic or poisonous to the system than either. . . . It was at first supposed that these animal alkaloids differed in their nature from the organic alkaloids formed by vegetables, and various reactions had been given to distinguish them. More recent researches, however, especially those of Brieger, appear to show that this distinction can be maintained no longer; but that the animal and vegetable alkaloids are similar in their chemical constitution, and that they are both products of albuminous or proteid decomposition; and that some, at least, of the so-called 'ptomaïnes' are identical with vegetable alkaloids.

"'We may now indeed regard alkaloids,' writes Dr. Lauder Brunton[20] . . . 'as products of albuminous decomposition, whether their albuminous precursor be contained in the cells of plants and altered during the process of growth, or whether the albuminous substances undergo decomposition outside or inside the animal body, or by processes of digestion as by unorganized ferments.' . . . But Gautier has further shown by his researches, extending from 1881 to 1886, that the animal alkaloids are also a necessary product of vital physiological processes, he having obtained from the secretions of living

beings alkaloid bodies having poisonous properties—results which have been confirmed by M. Peter.

"They have shown that about four-fifths of our disassimilations are the result of transformations within the body, comparable with the oxidation of alcohol; and that the remaining one-fifth of the disassimilations are formed at the expense of the living tissues themselves, 'free of all demands on foreign oxygen.' In other words, a fifth part of our tissues live after the manner of ferments; that is, they are anaërobious or putrefactive as to their life. Hence the possibility of alkaloids being thus formed within the living organism independent of bacterial fermentation is quite within our conception."

Speaking of the clinical, pathological, and practical aspects, Dr. Aitkin continues: "According to the different sources of poisoning or intoxication (as it is technically called), there are correspondingly different indications, signs, or symptoms capable of classification as below: (1) Poisoning by the 'extractives' is attended by hyperthermia. (2) Poisoning by the 'animal alkaloids' is accompanied by 'hypothermia.' (3) A combination or succession of hyperthermic and hypothermic phenomena may become manifest, according to the combination or alternation of poisoning by the deleterious physiological products or their antagonistic action (Dr. A. M. Brown). . . .

"There is in these researches still further disclosed the fact that in this auto-infection, this spontaneous or self-infection of the living organism by the 'alkaloids' and 'extractives' of its own formation, there is no question of quality, but simply one of quantity to be considered, by reason of the essential physiological source and action of the poison. In other words, the healthy living organism may become poisoned (more or less slowly) by the accumulation within itself of deleterious substances normally elaborated, but imperfectly or defectively eliminated. Hence the slow and insidious onset of much ill-health, and from which recovery is correspondingly slow." (Op. cit.)

It is not probable that in "the normal physiological state" which we call "health" (see my lectures on "The Germs and Vestiges of Disease") any particle of the living body, even the most subtle molecule, element, or meta-element, can properly be said to be dead until the instant at which it is to be thrown

off by an excretory organ. A rough analogy of this may be drawn from the "fall of the leaf," which in a healthy tree is due to a vital act; the vitality of the leaf is not extinct until it is cast off.

Perfectly healthy histogenesis consists in the insensible, uninterrupted passage of molecules from one vitalized condition into another, up to the point of excretion as a non-vitalized excrement. The instant this continuity of vitalized transition gives place to molecular death, the physiological state ceases to be normal, and the first step of disease is taken. All sorts of contrivances are provided in the organism—and for the most part successfully—for stopping disease at this first step—this initial stage.

It is conceivable that the *raison d'être* of microbes is the removal of these initial steps of death, and that these devitalized molecules constitute one at least of those means of sustenance—those feeding grounds which determine their habitat.

These remarks and quotations regarding "Ptomaïnes" and "Leucomaïnes" were first written after reading Sir W. Aitkin's brochure in December, 1887. I am gratified to find, on reading Dr. Latham's Hunterian Oration ("Lancet," Oct. 20th, 1888), that he has arrived at conclusions in many respects so similar to mine, that had I read his before I wrote mine, or had mine been published before he wrote his, we might easily have been supposed to have copied each other. Thus Dr. Latham says: "I wish to draw the inference that, in the living animal organism, owing to slight departures from the normal nutrition of parts, arising probably through nervous agency—the trophic nerves—various substances, such as the extractives or alkaloids, will be produced, which, if not eliminated or neutralized, will lead to pathological changes in the system, absolutely and entirely independent of any bacterial action. Or it may be that these poisonous substances act especially as irritants of epithelial structure, causing an overgrowth of epithelial cells, just as we know will result when any other irritant is applied to these tissues, and so the soil is prepared for the lodgment of the bacillus." "It may be suggested that when the bacilli have gained a footing in . . . effete or enfeebled tissue, while absorbing from it the constituents of their own protoplasm, they may give rise to or secrete poisonous products or ferments capable of still further destroy-

ing or weakening the surrounding tissue or cellular elements, and so obtain fresh food on which they can thrive." "Perhaps by further investigations into the chemical changes produced by bacilli, the constitution of the proteid molecule, complex as its nature is now regarded, may be unravelled and its properties better understood; we may be able to discover, possibly, what poison it is which the bacillus secretes, or how it is formed, or from what constituents of the proteid molecule it is derived. The other constituents of the molecule may perhaps furnish the antidote, or a poison antagonistic to that produced by the bacillus; for we know that poisons are antagonistic to each other—atropine and muscarine for example."

Crookshank says ("Manual," Second Edition): "Though we may accept as a fact the existence of pathogenic organisms, we are not yet in a position to assert the means by which they produce their deleterious or fatal effects. Many theories have been propounded."

Klein (op. cit.) concludes that: "The most feasible assumption, and the one borne out by observation, is that owing to the multiplication of the (pathogenic) organisms, certain chemical changes are produced in the blood and tissues, or that a special ferment is created, which sets up the anatomical changes characteristic of the particular disease."

Dr. R. W. Philip, of Edinburgh, believes that he has separated and identified a toxic product from phthisical sputa.

Dr. Philip's conclusions[21] are as follows: "1. In view of the work of Koch, it is impossible to avoid admitting that a causal relationship exists between the tubercle bacillus and the phthisical process. 2. The mere predication of this relationship is not sufficient in explanation of the clinical facts and the generally fatal termination of such cases. 3. The usually received explanations of the *modus moriendi* in phthisis are insufficient. 4. It appears probable that the lethal influence of the bacillus is due to the production thereby of certain poisonous products. 5. Clinical and experimental evidence appears to indicate that the morbid secretions from the respiratory surfaces afford a good medium for the growth of the tubercle bacillus, and, presumably, for the elaboration of such products. 6. Such a product is separable from the carefully selected and prepared sputum. 7. This product is possessed of well-marked physiological properties, being emi-

nently toxic to frogs, mice, and other animals. 8. The toxic properties of the product are, speaking generally, depressant. 9. More particularly they include a marked depressant influence on the heart. 10. This depressant influence seems to be exerted through the medium of the cardio-inhibitory mechanism. 11. The toxic action of the product is more or less completely opposed by atropine. 12. The amount of the product which may be separated appears to bear a distinct relation to the abundance of the bacillar elements present. 13. Absorption of the poisonous products most probably occurs by way of the lymphatic circulation."

It will be observed that with the exception of the "substances, such as the extractives or alkaloids" (of Gautier and Peter), which "lead to pathological changes in the system absolutely and entirely independent of any bacillary action" (Latham), all these poisoning theories presuppose the domiciliation of the bacillus in the animal; and, therefore, although they may have value as accounting, as Dr. Philip suggests, for the *modus moriendi*, they in no way account for a prebacillary constitutional decline, or for the predisposing conditions which determine the habitat of the bacillus, which are the points in which we are now most especially interested, and which I am attempting to explain.

CHAPTER V.

If we reject the foregoing hypothetical explanations of the initial loss of weight and of strength—the constitutional decline—characteristic of the true first stage of bacillary consumption, we appear to have only the following alternative, (1) To reject as fallacious the well-known clinical fact that constitutional decline may precede bacillary tuberculization; (2) To assume that it is due to the presence of the bacilli in the organism before they have produced any local signs of their domiciliation.

Let us then carefully consider what probability there is of this assumption being true.

It has been amply demonstrated, both clinically and experimentally—

1. That if tubercle bacilli gain admission to a part, and are there imprisoned—all access to other parts and to the torrent of the circulation being barred—no constitutional decline will be produced while they so remain.

2. That such localized domiciliation of bacilli, having set up tuberculization in the part and subsequent destructive changes, may, if the affected part is one important to health, lead to a sort of constitutional decline of a secondary nature.

3. That if the bacilli escape from their local imprisonment and become disseminated in a number of localities, the tuberculization and consequent destructive changes, thus extended, will set up constitutional decline of a secondary nature even "unto death."

4. That if the bacilli, under either of the above conditions, get access to the torrent of the circulation, they overwhelm the vital power, and cause rapid death by acute miliary tuberculosis. "Weichselbaum ascertained that in acute miliary tuberculosis of man the blood contains the bacilli." (Klein, op. cit.)

But in all these cases the constitutional decline is a second-

ary state, not an initial one antecedent to the establishment of local disease.

In order to account for initial constitutional decline antecedent to the local domiciliation of the bacillus, we must revert to the idea of the existence of the bacilli in the general circulation as a primary condition.

It is true that such a condition has been assumed and stated by some observers. And in the enumeration of the different modes by which the animal organisms may be attacked by bacilli, I have mentioned "a primary blood disease —the bacilli having entered the blood first."

But when we come to inquire more closely into the evidences that such a state is possible—except as the result of the forcible introduction of bacilli in experiments—we shall find that they are almost nil. In the first place we have to find out a portal by which the bacilli can enter the blood direct. The existence of this has not yet been proved; but such discoveries as those of Lewis in 1872, and of Bancroft in 1876, with regard to the "Filaria sanguinis hominis," and the marvellous manner in which even so large a parasite may find its way into the blood, should make us hesitate to deny the possibility of the bacillus finding some easy means of access to the general circulation. At the Paris Congress, July, 1888, it was suggested that the bacillus can migrate through mucous membranes without hindrance by the organismal defences.

"M. Cornil communicated some interesting experiments on contagion of tuberculosis through mucous membranes. The cultures of the tubercle bacillus, introduced into the intestine, penetrate very quickly into the mucous parietes. In introducing into the œsophagus of guinea-pigs a few drops of tuberculous cultures, he always observed, at the end of four days, submucous tuberculous lesions, with rapid generalization without lesion of the epithelium." ("Lancet," August 4th, 1888.)

It has also been suggested that the bacillus, having been introduced with food into the alimentary tract, may find its way by the lacteals into the mesenteric glands, and, through some defect in the intercepting functions of these glands, may enter the thoracic duct and thus get into the general circulation.

These are suggestions which still wait for further evidence before they can be considered to be established as facts. But

even supposing that, by either of these suggested portals, the bacilli can gain primary admission to the blood current antecedent to localization; all that we as yet know, clinically or experimentally, of the effects of bacilli in the torrent of the circulation is against the idea that they can exist there without rapidly setting up symptoms of dangerous illness. The most reliable examples at our disposal, from which to judge of the effects of bacilli in the general circulation, are cases in which they have entered it by auto-infection, from the breaking-up of local bacillary centres, either by natural processes or by the intervention of the surgeon, as pointed out by Mr. Barker and Dr. Woodhead, and in the statistics of 837 resections of tuberculous joints collected by Wartmann.

It is true that "the bacilli, though found in the blood in such cases, do not become active until they come to some part of the circulation in which they can make their way into the surrounding tissue." (Woodhead, op. cit.) But it is also true that this opportunity very soon occurs, and that they lose no time in taking advantage of it to permeate all the tissues and organs. Thus Dr. Woodhead says (op. cit.): "Acute miliary tuberculosis must be looked upon as the result of spreading of the infective material directly by the blood-channels. The demonstration of this fact was first accomplished by Weigert, who, in a series of several cases of acute miliary tuberculosis, was able to determine the presence of ulceration of the pulmonary vein. The process being similar to that in or near the wall of a bronchus. . . . Ponfick had first supposed that the bacilli might pass from a tuberculous thoracic duct into the venous trunks, and thus into the general circulation. It is probable that both observers were correct, and that both forms may occur. Coats further points out that a limited distribution of tubercle by the blood may be due to the passage of bacilli into the minute venous radicles in the glands in which tuberculous changes are occurring. That bacilli are found in the blood has been now frequently demonstrated, and quite recently several cases have been recorded in which general tuberculosis has come on after hemorrhage in patients suffering from apical phthisis. This is a matter of greater interest when it is borne in mind that all these cases of acute tuberculosis were developed in from seventeen to twenty-five days—just the period given by Koch as

that required for the development of tuberculosis when produced experimentally."

It does not seem very probable then, that a slowly encroaching constitutional decline, unaccompanied by signs or symptoms of localized disease, can be due to the presence of bacilli in the general circulation.

I think it is more probable that bacilli, entering by the alimentary portal, and thence gaining admission to the lacteal and lymphatic systems, may so impede normal nutrition by these important channels as to lead to constitutional decline, which in this case would be antecedent to localized pulmonary tuberculization. The effect of such a condition would, in fact, be similar, as regards loss of weight and of strength, to the effects of arrested or perverted pancreatic digestion. In the case of the filaria sanguinis, such obstruction of lymphatic glands and vessels by the ova of the parasites does produce constitutional illness. But we know how obscure are the physical signs of abdominal tuberculization, even when accompanied by wasting to the extent of marasmus in children.[22] And it is conceivable that constitutional decline, due to this cause, might precede for a considerable time the domiciliation of the bacilli in the lungs and the development of the physical signs of pulmonary tuberculization.

Such a mode of attack would be accompanied by the symptoms of defective assimilation of fats; and would be relieved by cod-liver oil—absorbable by the portal veins, and hence independent of the mesenteric system—and by fats emulsified by pancreatization—so as to facilitate their absorption by the defective and choked lacteal system, or by those portions of it still capable of exercising their functions.

Up to the present time, no therapeutic remedies have been able to compete with pancreatic emulsion of fat and cod-liver oil in the power to restore loss of flesh, and to prolong life in abdominal and pulmonary tuberculosis. Dr. C. J. B. Williams says, "I have no hesitation in stating my conviction that this agent (cod-liver oil) has done more for the consumptive than all other means put together;" and the late Dr. J. Hughes Bennett said, "What then is really required is not oil added directly to the blood, but oil digested and emulsionized by the pancreatic and other intestinal fluids; a truth which has induced Dr. Dobell to recommend that before administration it should be mixed with pancreatic juice."[23]

But it is of the utmost importance to bear in mind that, while the hypothetical explanation which I have just suggested is consistent with known clinical, histological, and therapeutical data, it limits bacillary consumption with initial constitutional decline to cases in which the bacilli have gained admission to the organism by the alimentary portal.

It may be, then, that the mode of attack—the portal by which it is made—in this way rules the division of pulmonary consumption into two great classes; both of which may be due to bacillary infection, but one of which—the one characterized by initial loss of weight and of strength (constitutional decline)—can only be bacillary, whereas the other may be due either to bacilli or to one of a long list of other irritants.

The result of my investigations with reference to such a division into two great classes, published in 1880, before the discovery of the bacillus, holds good in the light of subsequent research, as we shall see by the following list of answers, which I then published, to the complicated set of questions which I had propounded, and which I have detailed in this treatise at page 368.

In quoting these answers I have now interpolated references to the bacillus at appropriate places. "It is found (see my tables), That:—1. In a large number of cases of pulmonary consumption, local disease is preceded by constitutional disease by an unequivocal interval.—Constituting Class 1.

"2. In a certain number of cases, local disease precedes constitutional disease by an unequivocal interval.—Constituting Class 2.

"3. These two classes must be considered as absolutely distinct throughout.

"4. There is no stage at which these two classes meet on common ground. Class 1 is always plus, as regards Class 2. (That is to say, the constitutional decline, which in Class 1 precedes the local disease, is always an extra factor to the factors in Class 2.)

"5. There is a stage in which the constitutional disease sets up the local disease in Class 1." This stage I described in 1880 as consisting in the setting in of the hypothetical oxidation of albuminoid matter. And supposing our more recent views to be correct, we must now describe it as consisting in the dissemination of the bacilli from the lacteal and lymphatic

systems, and the tuberculization caused by their presence in the lungs.

"6. The first stage of the local disease in Class 1 is not the same as the first stage of the local disease in Class 2." This answer will now only hold good for cases in which the local disease is not due to bacilli. Where the pulmonary disease is due to the introduction of bacilli by the respiratory portal, the first stage of local disease will be substantially the same in both classes.

"7. Starting from this point, the subsequent constitutional symptoms of decline are not alike due to the effects of the progress of the local disease. In Class 1 they are due to the progress of the constitutional disease plus the effects of the local disease, and in Class 2 they are due only to the effects of the local disease. The first will always, therefore, unless arrested, beat the second in the race of constitutional decline."

"8. The local disease in Class 2 does not *per se* set up a state of constitutional disease of the same nature . . . as Class 1."

"9. A new constitutional disease of special character may be set up by the local disease in Class 2, and the local disease of Class 1 is competent to set up a constitutional disease of the same special character as in Class 2. In both these cases the special constitutional disease is due to the absorption of the products of the local disease." We may now add that when both cases are bacillary, there will not only be "absorption of products," but dissemination of bacilli.

"Finally, as already stated, the symptoms of constitutional decline which follow the establishment of local disease in each set of cases (Class 1 and Class 2) are not 'devoid of special character, and only such as accompany the progress of any local disease proceeding to a fatal issue,' but are of a distinct and special character, due, in Class 1, to the defective supply of fat to the blood, etc., etc." Or, as now hypothetically suggested, to the choking of the mesenteric system with bacilli. "While in Class 2 the symptoms of constitutional decline are due to irritative hyperplasia and proliferation . . . exhausting discharges and absorption into the blood of morbid products." And we must now add, that when the local disease is in both classes due to bacilli, the special secondary constitu-

tional disease may also be due to the dissemination of the organisms.

"It is competent for the original cause of constitutional decline in Class 1, viz., arrested passage of fats into the blood"—either by defective pancreatization or by bacilli, to be overcome in the course of the disease, the bacilli and their effects being cleared from the mesenteric system, or the defective pancreatization cured; and thus the cause of loss of weight antecedent to local disease in the lungs, characteristic of Class 1, may be removed; in which case "Class 1 will approximate in its later stages to Class 2."[1]

It will be seen from the foregoing recapitulation that pulmonary phthisis preceded by constitutional decline is to be accounted for by either of the following ways:

1. By the direct admission of bacilli to the general circulation, and some unknown influence there, antecedent to their localization in the lungs. Of which we have no reliable evidence.

2. By a defective absorption of fats, due to choking, etc., of the lacteal and lymphatic system by bacilli, admitted by the alimentary portal, and afterward conveyed to the lungs—there to become domiciliated and produce tubercle.

3. To a defective supply of fats, due to defective pancreatization, leading to the molecular disintegration of proteids, the débris of which may constitute the special food for tubercle bacilli, determining their habitat in the parts where such débris are to be found—such parts, as we have seen, being especially and primarily the lungs and mesenteric glands.

Or, 4. To the production of such débris in the manner described by Selmi, Gautier, MacMunn, A. M. Brown, Aitkin, and others. "The possibility," says Brown, "of their being formed within the living tissues independent of bacterial fermentation is quite within our conception."

Now it will be observed that all the foregoing hypotheses attempt to explain, not only some form of constitutional decline antecedent to tuberculization, but a "special form." If we dispense with the conditions of speciality, and simply try to explain the occurrence of some or any form of constitutional decline, we may, of course, fall back upon the old, well-worn, often refurbished suggestion, of "lowered vitality." I have

long regarded this as "the refuge of the destitute hypothesis." But it may be that, even now—after all that modern discovery has disclosed—we are still so destitute of better explanations, that some may think it best to adopt the manœuvre of covering our retreat in the smoke of "lowered vitality"; which may be made to account for anything, anywhere, that tends toward death.

But what we want is an explanation of a special form of tendency toward death—an explanation of why the tubercle bacillus effects a domiciliation in this person and not in that, an explanation of what determines its incidence on this organ rather than on that.

If we cannot, at present, find an explanation which is entirely satisfactory, we had much better stand bravely by our guns and confess that we are waiting for fresh ammunition, than keep up a fire of blank cartridges under the delusive name of "lowered vitality." I am afraid we can detect a tendency to the firing of these blank cartridges—to a falling back upon "the refuge of the destitute hypothesis"—lurking among some, even of the most learned and scientific, recent disquisitions on tuberculosis—evidently unperceived by their accomplished authors.

It may be humiliating to our *amour propre* as medical men—but nevertheless it must be faced—to suggest a source of fallacy which involves our own incompetency, as possibly at the bottom of all our difficulties in accounting for "a special form of constitutional decline" antecedent to tuberculization. It must be regarded as still within the limits of possibility that constitutional decline special to bacillary tuberculosis is, after all, not antecedent to the first local inroads of the bacillus, but only antecedent to the evidences that it is present and at work discoverable by our present powers!

What if that "eventful interval" to which I have more than once referred, which, in our experimental inoculation of tubercle bacilli, elapses between the insertion of the bacillus and the first evidences of its work detectable by the most minute chemical and microscopical tests—that interval which, I have suggested, may be occupied in the molecular disintegration of proteids, crystalloids, etc., by the microbes, to provide sustenance for the gathering forces of their invading army—what if that interval should, under certain circumstances, be

capable of prolongation until a definite observable "special form of constitutional decline" is developed, no local indications of disease being detectable by our present means of examination during the life of the patient!

It must be remembered that we seldom have opportunities of post-mortem examination of patients in this pre-tubercular state, and that when, by some accident, such an opportunity occurs, it is only men with such special qualifications and appliances as are possessed by MacMunn, Watson Cheyne, Crookshank, Klein, and a few others, in this country, who would be competent to conduct such a subtle investigation as would be necessary for obtaining reliable results.

But even supposing the interval between the introduction of the bacillus and the commencement of tuberculization to be capable, under certain conditions, of sufficient prolongation to account for an observable constitutional decline, this will not provide us with the essential conditions for determining habitat; it will not account for the predisposition of one person and not of another, or of the same person at one time and not at another, to the domiciliation of the bacillus. On the contrary, the supposition necessarily involves the forcible introduction of the bacillus, as in the cases of experimental inoculations, not a spontaneous election or selection of a certain habitat. Therefore we must either throw up our belief in the existence of predispositions to bacillary domiciliation, or accept some hypothesis of a pre-bacillary state determining the habitat of the parasite, whether or not such a state is demonstrable by any appreciable diagnostic symptoms.

Although I have suggested the possibility that the special form of constitutional decline antecedent to our detection of any local domiciliation of bacilli may, nevertheless, be due to the bacilli, and that the secret of the non-detection of their work may be found in the incompetence of our present means of diagnosis, it will be seen, by all that has gone before, that this is not the opinion to which I myself incline.

I still think it most probable that the "special form of constitutional decline" is due to pre-bacillary conditions—conditions by which the necessary sustenance of the bacilli is prepared, and hence their habitat in the animal organism determined, in accordance with the laws which determine habitat throughout the vegetable and animal kingdoms.

This brings us back, at last, to the practical object with which I commenced this disquisition, viz., To attempt some sort of articulate answer to the call of the medical practitioner: "Tell me, in the state of our knowledge this day, what is the best thing I can do to repel this enemy at our gates? What ought I to prescribe for this patient suffering from that 'special form of constitutional decline' constituting the true first stage of bacillary consumption?"

It is said that an old stage coachman, expatiating on the advantages of the road over the rail, exclaimed, "If you have an accident on the road, why there you are! but if you have one on the rail, why, where are you?"

If we adopt this graphic form of comparison in the case of consumption, we cannot find even the coachman's comfort. For I fear we must say, "If you find the bacillus, why where are you? for you do not know how to expel it; and if you don't find the bacillus, why, still, where are you? for it may be there!"

Speaking of the detection of bacilli in phthisical sputum, Dr. Troup says (op. cit.): "Naturally it is only where the breaking up of the original reaction products has begun and opened a way outward for the bacillus that it will be found. . . . In acute miliary tuberculosis it will not be seen in the beginning, and it is here and in all suspicious cases that a thorough quest for elastic tissue should be instituted." But elastic tissue cannot be present till the disintegration of formed tissue has taken place, and this is far too late for the diagnosis of "the true first stage," the stage which precedes any such tissue changes. Besides, as Dr. Troup is obliged to admit, "one cannot say" that the elastic tissue "is only present in the spit of phthisis—other destructive changes cause its appearance."

In the discussion on Dr. Percy Kidd's paper, at the Medical and Chirurgical Society,[24] Dr. Heron said, "I wish to emphasize, with Dr. Kidd, the importance of examining cases again and again. Very often the bacilli will remain undetected for weeks, and after examining the sputa during four, five, or six weeks, they will be found in large numbers."

To which Dr. Douglas Powell very aptly replied: "While the discovery of bacilli in the sputum is of positive value as a qualitative test, showing the existence of tubercle, the absence of bacilli is not by any means of equal negative value, unless,

as Dr. Kidd has said, the case be watched for many weeks; and in the course of these many weeks most of the physical signs will have developed."

Why! in the experimental inoculation of bacilli into healthy animals which I have recounted, we have seen that "four or five weeks" is time sufficient to allow the bacilli to become domiciled in every organ of the body!

Klein says (op. cit.): "After the introduction of the tubercle bacillus into the subcutaneous tissue, the nearest lymph glands show the first signs of swelling and inflammation, after one week, or even later, and the general disease of the internal viscera does not follow until one, two, or more weeks have elapsed. This is also borne out by the behavior of these bacilli in artificial cultures."

It is evident that the detection of elastic tissue and of bacilli in the sputum is, like the detection of physical chest signs, all too late. We must be far in advance of all this if we wish to anticipate the first steps toward death—if we wish to prevent or abolish the molecular feeding grounds, before they have led to the domiciliation of the bacillus.

Thirty years ago—at a time when the physical signs of auscultation and percussion were expected to detect everything connected with diseases in the chest—I published the following warning. And if we now add to our list of "physical signs" the presence of elastic tissue and bacilli in the sputum, the warning is almost as much needed to-day as it was then. Concluding a chapter on the "Diagnosis of Early Phthisis," I wrote:

"This absence of reliable signs of the earliest stage of tubercular deposit cannot be too forcibly impressed upon the young practitioner, who, with creditable zeal, is too apt to think, and naturally prone to hope, that by sufficient diligence, experience, and care, he may insure that no tubercle shall escape his searching examination. In this belief he will be often led to fancy that he has detected the presence of tubercle where it does not exist, and to assume its absence while it really lies concealed.

"This is the great disappointment which every man has to encounter who studies and practises physical diagnosis. That upon which he had set his heart—to detect the first shadow of consumption, scarcely yet resting upon a life, before it is too

late to drive it back—is the very dream which experience will most surely dissipate.

"If physical diagnosis could detect consumption as soon as the first few spots of tuberculous matter were deposited in the lung, with the same certainty that it detects pneumonia or a cavity, we might well be content to sacrifice for this all that it could do besides. But that it cannot, in its present state, accomplish this, and that there is no good reason to suppose that it ever will accomplish it, need surprise no one who will think calmly on the subject. It is not probable that the physical properties or the functions of a portion of lung tissue should be sufficiently affected by a few scattered gelatiniform granules, to produce any recognizable signs of their existence. There may not be a dozen tubercles deposited in the whole lung; nay more, there may not be one single microscopic speck deposited, and yet the disease may be working its stealthy inroads on vitality. We shall not, then, place implicit confidence in the physical diagnosis of early phthisis, but—while not neglecting this, while seeking from it all the aid it can give—we should exert our most acute observation of symptoms and diligently search into the histories of cases, in the hope that we may thus encounter some herald of the coming foe." [25]

When writing upon this subject again, in 1867, I insisted, with all the force I could put into words, that what has heretofore been called the "premonitory" stage of consumption ought to be at once recognized as "the true first stage."

"It is utterly absurd, when applied to practical medicine, to make a disease begin at a point at which serious structural change takes place, but which it is impossible to identify with certainty during life. The truth is that these divisions in the course of the disease have been made by the morbid anatomist in the post-mortem room, not by practical pathologists and physicians in the chambers of the sick. It is not surprising, therefore, that they are worse than useless in practice. . . . It is evident that the use of the terms 'premonitory stage' and 'first stage,' as now employed, can have no sense except on the assumption that the formation of tubercle constitutes the disease; and thus a false pathology is directly impressed on the mind of both physician and patient." [26]

Speaking of the difficulties which the pathologist has to

encounter in determining the histological origin and course of tuberculization, Dr. Woodhead says (op. cit.), "It would seem at first sight to be an easy matter to determine at once in what tissue the tubercle has originated in any special case. In the lung, however, where the tissues are so delicate and so complicated, and where, in consequence, the changes are so rapid, this is not the case; and it is only in exceptionally favorable cases that the mode of origin and spread can be at all satisfactorily demonstrated. Further, the variety in the life histories of individual tubercular growths at one time rendered it a matter of considerable difficulty to arrive at any definite understanding of tubercular processes, especially of those associated with pulmonary phthisis. The anatomical structure in the various forms being so absolutely defined in the earlier stages of the growth, it was difficult to bring into a common group forms which differed so widely from one another, not only in naked eye, but in microscopic appearances."

But if difficulties of diagnostic differentiation are so great when the clinical histories of the cases have been played out, and when the affected tissues are subjected to examination in the laboratory; how incomparably greater are those difficulties when the cases are in their "initial stage," before the microscopist or the chemist has any materials for examination, and when local and general clinical phenomena have scarcely dawned out of the regions of imagination!

This is a period so eminently critical to the patient, that the turning of the balance for life or death may depend upon the placing of a few grains of evidence in the right or the wrong side of the judicial scales. Yet this is the period when the physician ought definitely to begin his treatment of the true first stage of bacillary consumption.

CHAPTER VI.

THERE can be no doubt that we are right in having come to the conclusion in recent years that a medical man should be a scientific man. But we shall have lost more than we have gained if, in becoming scientific, he ceases to be practical. The fascinations of scientific research must never lead us to forget that the object of correct pathology and diagnosis is not diagnosis, but treatment. Our object in diagnosing the true first stage of bacillary consumption in its earliest dawn, is not merely to signalize a scientific triumph, but to enable us to quash it in its dawn by proper treatment, and thus to discharge the highest functions of the medical man—to prevent human suffering, and to save human life.

It is of no use to deny that, to the simply practical man, there is a greater charm in dealing heroically with the acute stages of disease, and, as it were, confronting death face to face, so that the victor's wreath can be unhesitatingly claimed for each success, than in calmly and considerately devising and applying the unexciting measures of protection and prevention which shall secure a far wider, grander, and more enduring—though less easily recognized—success. But, to the man who has cultured his practical faculties with scientific training and research, the greatest charm will lie in finding out the hidden secrets of nature's harvests, and winnowing out from her rich granaries every germ and vestige of disease. This is our special duty in the treatment of such abnormal physiological states as the true first stage of bacillary consumption—

> "A pebble in the streamlet scant
> Has turned the course of many a river;
> A dewdrop on the baby plant
> Has dwarfed the giant oak forever."

In the diagnosis and treatment of such subtle stages of disease, our first rule should be "to keep on the safe side"—the side of precaution. Better place fifty cases under safe

precautionary treatment that might possibly have righted themselves, than run the chance of leaving one case untreated that is thus allowed to pass beyond the regions of cure. Unfortunately, our greatest obstacles to such a wise course of preventive medicine are raised by the patients, and by the friends who surround them with solicitous yearnings for their good. They think we are "making too much fuss" about what appears to them to be a comparatively trivial ailment —a little obstinately progressive weakness, loss of weight, and disturbance of temperature. Both urge us to wait for more conspicuous calls for action—that is, till it is too late!

"They dawdle over physics, quackeries, and intended changes of occupation, till the day for cure has passed. Promptitude in this case is everything. I cannot too strongly describe the value of every day wasted—for every day brings nearer, at a rapidly increasing rate, the hour when successful treatment will have lost all its simplicity, and become beset with innumerable difficulties." Whereas, if taken in time the treatment is simple enough, although it must be uncompromising. I have said that our first rule in such treatment should be "to keep on the safe side." We have to decide what to do for the best "in the present state of our knowledge," which is admitted to be incomplete. We must, therefore, make our scheme of treatment wide enough in its scope to include the possibility that either of several hypothetical explanations, of the nature and cause of the patient's "abnormal physiological state,"[27] is the true one.

In order to be safe that we are doing our best for the patient, we must make the scope of our treatment include the possibility that his state may be either ante-bacillary or post-bacillary. It must be admitted that this margin appears to give more than "room and verge enough" to be satisfactory; but this cannot be helped, if we would be safe, until our knowledge has had time enough to arrive at greater precision. Of one thing, however, we may be certain, viz., that, if the condition we have to deal with is still ante-bacillary, the bacillary stage is so imminent and its advent so insidious that our treatment ought to include the possibility that both conditions exist. We shall do no harm by fighting against the bacillus, even though it be not there, so that this treatment is only supplementary to that which is directed to keeping it out, *i.e.*,

to curing the state of predisposition, which invites its domiciliation.

But, unfortunately, as I have pointed out, although we may not have been able to discover the bacillus, nevertheless, it may be there in some secret hiding-place, and our search must be renewed from time to time, so as to catch the first demonstrable evidence of its presence.

In order to make our treatment as comprehensively safe as I have suggested, it is necessary to bear in mind the principal portals by which the bacillus may enter the organisms, viz., the alimentary, the respiratory, the cutaneous, and the generative tracts. And, in reference to these portals, we must also remember that, in these earliest stages of disease of which we are treating—the bacillus, although not yet domiciled in the tissues, may be on their threshold, sustaining itself on the secretions to be found at these portals preparatory to its advance into the tissues. "Having regard to the conditions of growth of the tubercle bacillus, it seems likely that the muco-purulent secretion is a peculiarly good medium for its cultivation." (Dr. Philip, op. cit.) Therefore we must lose no time in purifying all these portals by bactericides, and encouraging them by the best remedies at our command to clear themselves of their secretions and throw them out of the organism. Secretion and excretion must be stimulated at all the emunctories.

"The stomach, liver, intestines, and kidneys must be unloaded of their contents, and their secreting glands and cells freed from accumulations, and stimulated to fresh action; this is especially necessary with the liver and mucous glands of the intestines. The portal system of veins must be thoroughly relieved from congestion and obstruction, so that absorption may be accelerated. The skin must be stimulated to action. The pulmonary circulation and the right heart must be freed from impediments; respiration must be set vigorously to work; and the diet must be regulated. In fact, every function of the body must be looked to and called to its account; and, above all, everything must be done promptly"[26] and with a jealous regard for the strength of the patient. Such remedies must always be selected as combine restorative with eliminative properties. Much knowledge, skill, and tact must be used in making this selection, and in adapting it to the

specialitiees of each case. It cannot be done on any "rough and ready" lines.

Both during and after this general purification and elimination, we must take every possible precaution to keep the patient out of reach of the enemy. We must never forget that if the bacilli are not already there, the conditions exist which invite their domiciliation—which determine their habitat, if by chance they should be brought within any of the portals.

In the light of our present knowledge, as set forth in this treatise, we must regard as nothing less than criminal the neglect, which is familiar to every medical man—the astonishing neglect, as we must now feel it to be—to remove the patient threatened with consumption from dangerous surroundings. There can no longer be any excuse for the doctor who does not impress this upon the friends of the patient, or for the friends who persist in the neglect after it is pointed out. It is appalling to remember how often we have seen one member after another—in consumptive families—told off to nurse the one dying of phthisis, each falling in turn, as soldiers succeed one another in the breach under the killing fire of the enemy. It has hitherto been looked upon as heroism in the victims. It must henceforward be regarded as murderous on the part of those by whom it is permitted. Those only should be allowed to be most intimately associated with a patient in whom bacilli are known to exist, who have no special susceptibility to its domiciliation.

The practical question at once arises: What are we to do to establish these essential precautions for keeping out of reach of the enemy till the conditions of receptivity have passed?

Every case must stand upon its own merits, and common sense should be the best guide as to how to accommodate special circumstances to special cases. But, unfortunately, common sense has always been the rarest of human possessions, and it gets rarer and rarer every day. The curse of the present generation is want of thought—the hurry to do something—instead of first thinking what is best to do. "Pause, and think before you speak," was a favorite school-boy lesson of the past. "Pause and think before you act," should be the daily lesson of the present day, for both the young and the old.

If common sense were allowed to rule in the present matter, we certainly should not send patients, specially susceptible to bacillary infection, to private or public institutions where people are allowed to congregate who are suffering from bacillary tuberculization—to breathe the air polluted by the breaths and sputa from lungs teeming with bacilli and their spores. We should not allow predisposed children to be tended by tuberculous maids, or suckled by tuberculous nurses.

If common sense were allowed to rule, we should not let persons, specially susceptible to bacillary infection, drink unboiled milk without first having made sure that it did not come from an udder affected with bacillary mammitis; and we should not let such persons eat at tables supplied with tinned meats, the history of which is only known to those who have thus disposed of their tuberculous cattle. ("The precaution of having all milk boiled before consumption will, there is good reason to believe, insure absolute safety against all the ordinary contagions of which milk may be the carrier; and, as we do not consume other kinds of animal food without cookery, there is nothing remarkable in cooking milk as well as flesh. There is, moreover, one point of Professor Brown's report with which we are in complete agreement—that, namely, in which he refers to the filthiness of many dairies."—"Times," Dec. 6th, 1888.

"*Tuberculosis and Milk.*—Although in the light of recent researches into the bacillary nature and communicability of tubercle by direct experiment, the consumption of milk from tuberculous cows cannot but be looked on as fraught with danger, instances in which such a mode of communication can be absolutely demonstrated are, from the circumstances of the case, not frequently met with. The fact that, even in advanced stages of the disease, the bacilli are often not to be detected in the milk, the generally long incubation period, and the probability that the milk supply has been changed or the animals slaughtered before palpable mischief has been done, as well as the frequency of tuberculosis in man arising from other causes, surround with almost insuperable difficulties all observations on the human subject. But an instance has lately come under our notice which admits of no doubt, and consequently deserves record. The owner of a valuable herd of cows, finding that a large proportion of them were tubercu-

lous, so large a proportion indeed as strongly to suggest infection by association in the sheds, withdrew his milk from the market, and used it, unfortunately without boiling, for fattening his pigs, of which he has a large number and on which he prides himself not less than on his cows. The result has been that the pigs have, almost without exception, been affected with the disease to an extent that has necessitated the slaughter of the whole stock. Another point of practical interest is that he has not been able to discover nodules or other indications of localized tubercle in the cows' udders, a condition still held by some to be necessary to render the milk capable of transmitting the disease. It is much to be regretted, too, that the legal definition of disease in the cow, as laid down in the Dairy and Cowshed Orders, does not include tuberculosis, but is limited to cattle plague, pleuro-pneumonia, and foot-and-mouth disease. This definition should be made to include tuberculosis and all eruptions of the udder."—"British Medical Journal," Jan. 5th, 1889.

"At the meeting of the Paris Congress on the 26th July, the question as to the dangers to which people are exposed by the use of the flesh and milk of tuberculous animals, and the means to prevent them was discussed. . . . All agreed that the use of the meat, and particularly the milk, of tuberculous animals, should be regarded as dangerous. . . . The Congress voted, in principle, that the flesh of a tuberculous animal should be seized in totality."—"Lancet" report of Congress, Aug. 4th, 1888. Drs. Hubermaas, Gerlach, Albert, Klebs, Bollinger, Stein, Johne, Baug, Toussaint, Chauveau, and many others, besides Koch, concur in stating that if milk from cattle with tuberculous udders is given for any lengthened period, tuberculosis will be developed.

In Copenhagen "The Control Committee of the Milk Supply Company ordered a fortnightly examination by a veterinary surgeon of all cows supplying milk to their company. This examination to include a most careful inspection of tuberculous udders."—Dr. Woodhead, op. cit.)

If common sense prevailed we should not let those suffering from bacillary consumption cohabit with those already predisposed to the same disease.

Knowing as we do that the tissues and organs of the young are especially unstable and vulnerable in the presence

of the bacillus; that their glandular systems are especially apt to break down; that their alimentary and respiratory portals are in special need of protection, and that their cerebro-spinal systems are especially liable to disease; if common sense were allowed to rule, we should not let the children of all sorts and conditions of men be passed through the same educational mill, at schools where they are fed alike—or left unfed if they are unwilling to eat—exercised alike, exposed alike, worked alike, punished alike, worried alike—leaving it to the rough laws of natural selection and the survival of the fittest to kill off the feeble and susceptible from the crowd. If common sense prevailed, men and women would interpose the selection of intelligence to prevent the specially susceptible of their progeny from falling under those rough laws of natural selection which decimate the rising generation.

The practice of medicine was once described as the "Practice of Educated Common Sense." Let us pray that it may never lose this high character! The more we see the common sense of the general population obscured by the feverish heat and fickle glamor of the rage for superficial education, the more necessary it becomes that medical men should keep aloof from the dust and hurry of the crowd, and calmly hold and cultivate their common sense as a sacred charge for the guidance of those in whom it is obscured. There are few more difficult fields in which to exercise "educated common sense" than in attempting to guide the public in the management of the true first stage of bacillary consumption; because it needs to be accommodated to a number of concurrent and often conflicting circumstances; and must be based, as I have said, upon a plan wide enough in its scope to embrace the possibility that one of several conditions may exist alone, or that several conditions may co-exist.

This is exactly what it is most difficult to make the majority of persons understand. The "highly educated" narrow-minded conceit of the present day makes people think that they are bound to understand everything, however much it may be beyond their comprehension; and when they find themselves in a position which they cannot understand, instead of trusting to the authority of a wise and instructed counsellor, who has devoted his life to the subject, they rush at the first chance of some simple formula of belief—some striking flag—on which to pin their faith.

In medical matters this is conspicuously exemplified. At one time they "believe in" cod-liver oil, at another in arsenic, at another in pancreatic emulsion, at another in koumiss, at another in mutton chops and porter, at another in grapes, at another in open windows, at another in hot inhalation halls, at another in cold sponging, at another in Turkish baths; at one time they "believe in" compressed air, at another in rarefied air, at another in sea voyages, at another in pine woods, at another in altitudes; at one time they "believe in" hot places, at another in cold; at one time in sea coasts, at another in inland continents; at one time in constant exercise, at another in constant rest, at another in cotton, at another in wool, at another in peat smoke, at another in no smoke; at one time in inhalants, at another in no inhalants; at one time in electricity, at another in massage; sometimes it is "throw physic to the dogs" sometimes it is physic, physic forever, on which they pin their faith; and so on *ad infinitum.* It is always all one thing or all another. In the course of a few weeks of consulting practice a physician will be asked by different patients, and sometimes I am sorry to say by their doctors, too, whether he "believes in" this or that of many of the items in this list of incongruous elements of treatment. The inquirers seem to suppose that the doctor can answer "yes" or "no" to the idiotically-conceived questions as glibly as they are asked.

It seems to be out of the reckoning of these questioners that more than one thing can be "believed in" at once; that any reasonable combination of such important elements can be the proper thing to "believe in"; that one or more of these means of treatment may be necessary at one stage of a case, and one or more at another stage; or that none of them can be wisely applied to the case in hand under present conditions.

My usual answer is, "I don't 'believe in' anything in the sense in which you ask the question. Medical treatment is not to be so thought of or administered. It must always comprise a group of ideas as to the nature of the case, and a group of ideas as to the means of treatment. It is never 'all this,' or 'all that.'"

It is this superficial haste to have everything put into a single formula, needing no thought or consideration during action—ruining all prospect that wise advice will be either

comprehended or acted upon, that discourages doctors from taking the pains to give it. So patients run hither and thither, first pinning their faith on this flag and then on that, till all chance of being cured is lost.

I warmly agree with Dr. Clifford Allbutt when he says—speaking of the climatic treatment of consumption in the altitudes ("Lancet," Oct. 13th, 1888):

"Let us, then, bring the friends always, and the patients generally, to realize that recovery from phthisis, however incipient, probably means a very costly and prolonged system of treatment, and, what is more, a steady, clear-eyed, persevering walk on the part of the patients and friends, if success is to be attained. . . . Now, when a man has had it put straight before him what phthisis means, even in its small beginnings, he will learn that a serious, an unflinching and vigilant attitude is his one way of safety; and a little home sickness, some sense of tedium, and some love of change, must not be allowed to turn him from his long and arduous course. . . . Once more, I would urge upon all phthisical patients the importance of incessant medical supervision. Apart as I am from practice in alpine health resorts, I may brush aside all scruples, all suspicions of self or class service, in saying this and repeating it. For those medical men who do practise in these health resorts there must be a fear of misconstruction of their motives when constant supervision seems to them more necessary than it may seem to the patient or his friends. But scruples of this kind, honorable as they are to the physicians, must give way to a clear view of the need of such supervision. The patient must be kept at his best—at his best of digestion, as well as at his best of pulmonary disorder. His temperature should again be scheduled whenever any sense of lassitude is felt, and the catarrhal and other varying conditions of the lungs should be systematically recorded. Not only so, but the regular visits of his doctor keep up the patient's serious resolve, strengthen his will, inform his judgment and discipline his habits. And in all this lies most of the battle!"

These are the wise and considerate words of an unusually wise, experienced, and independent practical physician. Dr. Allbutt has applied them to cases of phthisis even "in its small beginnings." They apply to it more and more strongly

as we go further and further back, "in its small beginnings," to what I wish to be emphatically recognized as the smallest of all beginnings — its true first stage, its pre-tubercular stage—its stage of comparative simplicity, and of comparatively easy cure.

If, at a later stage, "his one way of safety," as Dr. Allbutt says, "is an unflinching and vigilant attitude," "a steady, clear-eyed, persevering walk on the part of the patient and friends, if success is to be attained,"—this is still more important when we are dealing with the earliest stage of all; when everything depends upon our success in keeping the patient from sliding on into the next stage while we are endeavoring to eradicate the first. He is often so close upon the confines of this next stage that it may be impossible to prevent its invasion in the time at our disposal. But the immediate detection of its onset may enable us to cut it short by a prompt and resolute change of treatment. "Let well alone," is a wretched axiom for the physician. He should always be on the watch to do better.

If the disease runs on, such prompt and resolute changes of treatment may be required for each new phase.

But, if the patient is to have the best chances of cure, these changes must be made with a constant recollection of the "main lines" of treatment—treatment founded on a full appreciation of the basic nature of the disease—and it must be subservient to them. It is distressing to see in daily practice how often this is lost sight of—how often a patient, doing well on a wisely-arranged line of curative treatment, is taken off it, and "shunted on to a siding" because of some accidental intercurrent complaint, and is not restored to the main line till it is too late to continue the curative journey with success. But it is not my purpose in this treatise to discuss the treatment of any stage subsequent to the true first stage.

Supposing, then, that we have taken the patient out of reach of the enemy—that is to say, so far as it is practicable, out of the chances of bacillary infection by any of its usual portals; that we have cleansed these portals with bactericides; that we have "called to their account" all the secreting and excreting functions; that we have put him upon an easily assimilable unirritating diet, including carbo-hydrates and pre-pancreatized hydro-carbons; and that we have removed

any accidental intercurrent complaints that happened to be present—"let us never forget that when the patient is thus placed in a position of safety, he is scarcely better off than a leaking ship, when by means of constant working at the pumps she has been brought safely into a harbor of refuge. Unless advantage is taken of this temporary security effectually to mend the leak, the ship will go down the first time the pumps are stopped. . . . It is through ignorance or neglect of this vital object of treatment that cases of early consumption, apparently progressing steadily toward recovery, so often relapse just when the improvement ought to have been made permanent, and thus the best chance of a radical cure is lost." [26]

The question is—What next should be done? What are the best means of restoring "the normal physiological state"? What are the best means of setting right that abnormal metabolism and histogenesis which have allowed the formation and accumulation in the blood or in the tissues,—perhaps of MacMunn's respiratory histohæmatin, adrenal hæmochromogen, urohæmatin, enterochlorophyl, hæmatoporphyrin, and other occult products revealed by spectrum analysis;—perhaps of Selmi's ptomaïnes;—perhaps of Gautier's leucomaïnes, alkaloids, and extractives;—perhaps of Crookes' meta-elements;—perhaps of disintegrated proteids and crystalloids, and other débris of devitalized molecules which supply the "happy hunting grounds" of the bacilli with those special means of sustenance which determine their habitat?

How are we to find means of treatment which shall permanently arrest these earlier degradations of the normal physiological state?

In answer to this question the first ideas which naturally come into the mind are meteorological—the "*Τὰ μετέωρα*" of Aristotle—some change of climatic surroundings which shall radically alter the conditions of existence.

But before discussing or prescribing remedies not obtainable by "all sorts and conditions of men" it is well to turn to certain valuable medicinal substances, well worthy of consideration, which are within the reach of all. I have taken for granted, that hydro-carbons have been at once added to the diet in their most assimilable forms—oleinous fats, like cod-liver oil, to reach the blood by venous absorption, independent of lacteals and lymphatics which may be blocked, overtaxed,

or diseased; solid fats (stearin, margarin, and palmatin) emulsified by pancreatization, to assist them in reaching the blood by the lacteal route in spite of its damaged or obstructed state. (The practical experience of a quarter of a century, quite independent of any hypotheses as to the nature and cause of tuberculosis, has yearly added accumulated evidence from all parts of the world of the value of "Pancreatized, Predigested or Peptonized" foods originated by the Author in the form of Pancreatic Emulsion in 1863. In addition to its value in consumption and in tabes mesenterica, the "British Medical Journal," August 15th, 1885, speaking of predigested food, said, "It has proved useful in many hands, in uræmic vomiting, gastric catarrh, pernicious anæmia, gastric ulcer, and in pyloric and intestinal obstructions. Its introduction has probably done more than any other therapeutic measure of recent times to lessen infant mortality." See also a Leader on "Pre-digested Food" in "The Chemist and Druggist," January 14th, 1888.)

When we remember that fat is essential to the formation of healthy cells; that there is reason to believe that the devitalized molecular débris, of which we have spoken, are taken up and metamorphosed by cells; and that there may be a "battle of the cells" to resist the bacilli; we shall understand that there are much deeper reasons for this kind of treatment than appear, *primâ facie*, when we merely think of supplying fat to stop waste. I have also taken for granted that carbohydrates (farinaceous foods) have been at once added to the diet—to supply materials for heat and mechanical force, and thus to economize the fats. For the same reason it may often be necessary to add alcohol, when there is urgent need to save time by supplying materials for rapid oxidation until the organism is charged with materials for tissue formation; but in whatever form alcohol is given, it should always be associated and supplemented with more permanent and substantial forms of food.[28]

Next in importance, and in the order of therapeutic sequence, come iodoform, chloride of calcium, arsenic—and many would add the hypophosphites; but as to these, although they have a certain usefulness, my own experience is disappointing. I believe that much valuable time is often wasted by too great a dependence upon them, and especially upon certain popular

preparations which, while bearing their name, contain them in ridiculously small quantities. But iodoform is a potent remedy, although its most important place is in cases of advanced tuberculization and disintegration, where the sputa are unavoidably swallowed in considerable quantities. It is also of much value in the earlier stages. When there is a suspicion that bacilli may possibly be lurking in the secretions, ready for an advance into the tissues, or that they may have gained admission to the blood, iodoform should always be administered as a precautionary measure. Its value was first brought to my notice by Dr. Stiles Kennedy, of Newark, Delaware, in a brochure which he sent me when I was editing my Reports on Medicine in 1869. Speaking of his own experience he said, "Any medicine that will assuage the sufferings of the phthisical patient is indeed a boon, but iodoform offers more; there is every reason to believe that it exerts a most potent influence in discussing and removing the disease in its early stages. . . . I do not know that iodoform has been used in any case of open scrofula, *i.e.*, ulceration of the lymphatic glands; but several cases bearing that diathesis, and springing from that cause alone, so far as could be judged by the senses, have been treated successfully." But we owe the present revival of the remedy in consumption to the careful experiments and reports of Dr. Shingleton Smith, of Clifton. There is very seldom any difficulty in getting it taken and well borne if prescribed in a pill—which I contrived after reading Dr. Kennedy's paper in 1869—containing gr. ij. iodoform, and made up with sugar of milk and glycerin of tragacanth. This should be varnished and scented with coumarin or coffee, and taken twice or thrice a day half way through a meal. If it causes any sensations of nervous perturbation, which is rarely the case, the dose should be diminished, and if there is diarrhœa or sickness, it should be discontinued till they are allayed, and during this interval, sulpho-carbolate of soda (a valuable remedy which we owe to Dr. Sansom) should be given instead.

The next remedy of importance is chloride of calcium. It deserves to be considered a curative medicine; not from any direct action upon bacilli or their food, but from its specific action upon lymphatic glands. Its action in relieving and reducing enlargement of superficial lymphatic glands is well known; and there is every reason to believe that it is no less

potent in its action upon adenoid tissue generally, and upon the deeper glands, especially the mesenteric system, to which it is more directly applied through the alimentary portal. It should be given in doses of ten to fifteen grains twice or thrice a day, well diluted, and it may be advantageously combined with bark or quinine. With our present knowledge of the connection between bacillary consumption and affections of the lacteal and mesenteric system, we cannot be surprised that clinical experience has proved the value of chloride of calcium in the earliest stages of consumption, and that this is especially the case in early life.

Arseniate of soda comes next in importance, especially if loss of flesh is rapid. I think it must be regarded rather as a "skid" to "brake" the wheels of a descending vitality while restorative measures are being applied, than as a strictly curative remedy; but as a tonic, antiseptic and anti-dyspeptic, it does much to help the cure in a large number of cases—when tuberculization is imminent. But it is most valuable in developed phthisis after tuberculization has occurred. One of the best means of administering it is in the form of La Bourboule water, mixed with one-third of boiling milk.

CHAPTER VII.

BEFORE leaving the subject of food and medicine, I must insist upon the great importance of keeping up the habit of taking regular solid meals of the old-fashioned family type; consisting of a fair admixture of fish, meat, poultry, game, eggs, vegetables, puddings, bread, and fruit, with a moderate quantity of beer or wine, and of coffee, tea, or cocoa, taken at regular intervals of three to five hours,—the intervals being shorter in the first half of the day and longer in the second half. When it has been necessary—as it often must be when cases first come under treatment—to adopt smaller, more fluid, and more frequent meals, we should never forget that "strong meat belongeth to them that are of full age," and that the stomach very readily loses the habit of taking and disposing of "strong meat"—if it is kept too long and too frequently supplied with food that does not call upon the digestive apparatus for the full performance of its functions. (See some excellent papers by Dr. D. J. Brakenridge, of Edinburgh, "On the Influence of Digestive Habit."—"Medical Times and Gazette," June, 1868.) Therefore we should always keep in view, as an important feature of dietetic management, the return to solid and less frequent meals, as soon as it is possible to do so with safety. This possibility may often be hastened by giving pancreatin, pepsin, and diastase, either with or soon after meals, to assist and accelerate digestion and assimilation. I may mention here—as it does not seem to be always understood even now—that when pancreatin is given it should be the powdered pancreatin, as none of the fluid pancreatins have any considerable power of emulsifying fat, and that as this preparation has the properties of pepsin and of diastase—as well as the emulsifying power—there is no need to give malt extracts as well as pancreatin, and that pepsin is only needed when the power of the pancreatin to digest albuminoids requires to be supplemented.[29]

The importance of wool-clothing next the skin, both night and day, and of so regulating the clothing as to protect from chill and cold, but not to oppress with its weight, or to keep the body perspiring when at rest, are so well known now that I need not dwell upon them here, further than to add that "during sleep it is equally important to keep the body pleasantly warm, and to avoid keeping it over-heated; and that as serious changes in the weather may happen in the night, and the lowest temperature in the twenty-four hours naturally occurs between two and six o'clock A.M.—the ventilation and clothing must be prepared for these contingencies. Some extra article of clothing should always be at hand to put on the bed if necessary. . . . It is most important to keep the arms warm both day and night. Cold arms act as refrigerators to the blood immediately before it is discharged into the heart and lungs. . . . No sitting or sleeping-room should be left long without a fire; and every room in which persons live, either by day or by night, should have some opening by which it communicates with the outer air; but this should be so arranged that no draught can fall upon the persons in the room. If several rooms are occupied by turns during the twenty-four hours, the temperature of any one should not differ greatly from that of the rest. No draught should blow upon a bed, and during sleep the whole body should have one covering at least of woollen material; for while it is very important to keep the air of sleeping rooms fresh, it must be remembered that the body is more susceptible of chills during sleeping than waking, and that changes of temperature in the outer air are especially apt to occur during the night, and are therefore in danger of producing chills before they are observed." [30]

Recent observations have proved that the catarrhal secretions of mucous membranes form tempting hiding-places for bacilli while waiting for the chance of more dangerous domiciles in the tissues; and this should be a warning to those who, in their zeal for open windows and open carriages, set at defiance the experience of the past as to the dangers of thus giving colds to those constitutionally disposed to catarrh.

Great follies are committed in the matter of the daily bath, some going to one extreme and some to another. As it is of great importance to adopt a plan by which the body may be invigorated without the danger of inducing chills and catarrhs,

which are so dangerous to the consumptive, I should like to repeat here the directions given with this view in my work on "Diet and Regimen": "Warm baths, Turkish baths, vapor baths, shower baths, cold plunges, and sea baths should only be used under special medical orders. During ordinary health the skin of the body and limbs should be smartly rubbed once in twenty-four hours, first with a rough towel (or large sponge) wet with cold water, and then with a dry towel till in a glow. The bather should stand on a dry rug while using this 'cold friction bath,' and it should not last more than one or two minutes, including both the wet and dry rub. Salt may be advantageously added to the water, and the bath may be used either on rising or going to bed, according to the feelings and convenience of each individual, the morning being preferable as a rule.

"When water cannot be borne cold, it must not be used tepid, but scalding hot. The momentary application of scalding hot water to the skin, immediately followed by a brisk friction ('hot friction bath') will produce a direct-action glow much like the reaction glow following the application of cold.

"If the weather is cold, or the person delicate and chilly, the upper half of the body should be uncovered and bathed and rubbed first, and then a woollen vest should be put on, and the lower half uncovered and bathed and rubbed. . . . Except under definite medical orders, it is not advisable to sit or stand in cold water."

On the important subject of exercise, the foolish extremes advised for patients are even more dangerous than in the case of baths. It is necessary, therefore, to give a few plain directions under this head. "During ordinary health, some part of every day ought to be spent out of doors; and in ill health it is of great importance not to discontinue the observance of this rule without good reason, for although in certain states of disease it may be very important to remain in-doors, it must not be forgotten that proper clothing, goloshes, respirators, waterproofs, and umbrellas, may make it not only safe but advantageous to go out of doors for exercise, when without these it would be very injurious. Out of door exercise should be as active as the strength will allow, and should always be continued up to the point of slight, but not over-fatigue. This will be the best measure of the proper amount for both

the weak and the strong. Unless the air is pure and the person strong, exercise before breakfast is more likely to do harm than good." Mr. Maclaren has well observed in his excellent work on "Physical Education,"[31] that "a most important principle in exercise, and one that should ever be borne in mind, is that it should be regulated by individual fitness; for the exercise that scarcely amounts to exertion in one person, will be injurious and dangerous to another; and not only is this inequality observable among different individuals, but the same individual may have parts of his body possessing special power or presenting special weakness. A man may have limbs capable of transporting him at the rate of four miles an hour, throughout the day and for many days in succession, but the heart or lungs all unequal to the effort; or he may have an organization so fine and a temperament so susceptible to stimulation or excitement that the one is an abiding danger to the other."

Over-fatigue, by which I especially mean the effects of too protracted exercise, is singularly disastrous in consumption in all its stages. In the early stage I have often seen a patient —who was going on so well as to believe he might indulge in as much exercise as he pleased—make a disastrous descent, from which he never recovered, after foolishly prolonged fatiguing exercise. And no form of exercise is worse in this respect than walking.

These cases are so appalling that I have often endeavored, in vain, to find any adequate physiological explanation of such a tremendous downfall from a cause that, *primâ facie*, ought at most to have been temporary in its effects. But I think I can now explain the matter.

Suppose the patient to have a few bacilli domiciled in his system—the propagation and advance of which is being successfully kept back by the organismal defences, fortified by the influence of judiciously directed diet, regimen, and other medical treatment. From what we have already fully discussed, we may assume that this improved state signifies that, while maintaining his formative and reparative powers, he is starving out the bacilli, as it were, by keeping his organization clear of those débris of molecular disintegration, etc., which constitute their sustenance. Now, if, under these circumstances, he not only exhausts his formative and repara-

tive powers and calls upon them for unusual activity while so exhausted, but floods his system with the materials which supply sustenance to the bacilli, and at the same time raises his temperature a few degrees by fever; he establishes in special force the two first laws which dominate habitat, viz., the presence of appropriate food and the presence of appropriate temperature—which are at once taken advantage of by the bacilli to multiply their host and overwhelm the patient.

That over-fatigue is competent to produce these effects has been practically demonstrated in the too protracted marches of armies, and by experiments. "Normal health comes to be conditional on an incessant formation, transformation, and elimination of the effete or old organic materials, which must give place to the new. It is this effete material (in whatsoever form it is found) which, therefore, represents a series of partial deaths; and which, as the result of organic functional operations, constitutes life, during which the tissues and organs in the processes of their metabolic changes, perform a constant function of disintegration—fabricating during these processes those 'alkaloids' and extractives—'those *x*, *y*, *z*'s of pathology' which must be regarded as veritable scoria (Dr. A. M. Brown), or physiological ashes (Dr. Lauder Brunton) resulting from the processes of combustion of the elements of organic tissues. . . . Their accumulation may take place under two widely different conditions: as when there is an excess of 'extractive matters' and 'alkaloids,' with normal but inadequate elimination by the emunctories: or, the production of deleterious materials being normal, their elimination is inadequate from disease or derangement of the emunctories. Hence, auto-infection may result from excessive production, and inefficient (*i.e.*, inadequate) elimination, the emunctories remaining sound, a condition which is constantly seen in all forms of physical over-taxation or over-exertion, as in a prolonged march, or by excessive drill, especially in young and adolescent soldiers. Of such examples army medical officers acquire considerable experience.

"In a paper recently submitted to the Academia dei Lincei (to be printed in its Transactions) the physiology of fatigue has been carefully worked out by Professor Angelo Mosso, of Turin, with a view to the determination of the pathological manifestations which accompany that physical condition. He

has found that when fatigue is carried beyond the moderate stage at which it is decidedly beneficial, the blood is subjected to a decomposing process through the infiltration into it of substances which act as poisons—substances which, when injected into the circulation of healthy animals induce malaise, and all the signs of excessive exhaustion. It was on the soldiers of the Italian army that Mosso's experiments were made. . . . This 'fever of prostration' engendered by over-exertion is a very characteristic one, due to the proteid embarrassment which results from the functional disturbance of the tissues. The changes which take place (chemical and functional), although they cannot be actually seen, yet to some extent they can be measured, judged of and made out, or realized from their effects." (Sir W. Aitkin, op. cit.)

Some kinds of exercise are to be greatly preferred to others for consumptive patients. For a variety of reasons, and especially on account of economy, exercise is too apt to take the form of walking, which is by no means the most desirable for delicate persons; it cannot compare with horse-riding. I pointed this out in my work "On the Medical Aspects of Bournemouth": "In speaking of air and exercise I wish to impress the importance of horse, pony, or donkey riding (in proper weather, according to the strength, etc., of the patients). . . . Neither walking nor driving can compare with riding for consumptive invalids. It has even been regarded by very eminent authorities as a 'cure' for incipient consumption. . . . Consumptive patients are often accompanied (to health resorts) by relatives who have the same hereditary and constitutional tendencies as the patients themselves, and who, frequently, are only one or two stages behind the so-called 'patient' in the progress of the same family scourge. To these persons horse-riding and pony-riding would be most potent aids to developing in their favor the curative influences of climate. . . . I am endeavoring to induce the riding establishments at Bournemouth to introduce troops of sturdy and quiet ponies trained for invalids to ride."

I am glad to find that Dr. Theodore Williams agrees with me in this. "Riding exercise, from the time of Sydenham, has been generally acknowledged to be peculiarly beneficial to consumptive patients who are strong enough to bear it, and it is difficult," he says, "to find a form of exercise which so

admirably answers the purpose of giving plenty of fresh air and thoroughly warming both body and extremities with so small an amount of fatigue. Bicycling and tricycling combine the advantage of fresh air in abundance and a great amount of exercise, chiefly of the legs, but there is no doubt that the whole muscular system is brought into play, though often too strongly. In cases of weakness, and great loss of appetite and flesh, when at the same time the tubercular disease is not very active, massage of the muscles of the body after the Weir-Mitchell method can be practised with considerable benefit and with decided gain in weight. Under its influence appetite and digestion improve, and the circulation becomes more vigorous, color returns, and the quantity of food consumed is sometimes astonishing."[6]

When the true first stage of bacillary consumption is first detected, and while preparations are being made for change of occupation and surroundings, I have found massage of great use. It should be moderate in severity and always centripetal in direction. In these cases, as we have seen, the ptomaïnes, leucomaïnes, alkaloids, extractives, myohæmatins, débris of molecular disintegration, etc., have been allowed to accumulate in the tissues and organs. It is probable that centripetal massage drives them into the current of the circulation and facilitates their destruction or excretion. Hence two or three weeks of skilled massage is an excellent prelude to out-of-door exercise.

With regard to cycling, I do not think these patients should be allowed bicycling; and if tricycling is allowed it should be on a double tricycle accompanied by a friend stronger than the patient—who can take the stress of the work if the patient is fatigued, push the tricycle up hill for him, and protect him from accidental bad weather, and, if on a tour, regulate the amount of his daily exercise according to circumstances, and superintend his hotel and other management; otherwise the risks of cycling are apt to be greater than the advantages. This caution also applies to tours on horseback; they should not be made alone.

Some years ago I sketched out a scheme for a riding tour round the United Kingdom—varied, at special places and seasons, by boating and sailing excursions—bringing the tourist to the various districts at their most favorable seasons, and

arranging halting places for rest and for medical examinations at stated intervals. I submitted the plan to Messrs. Cook, the tourists, and tried to induce them to organize parties provided with tickets, etc., to secure proper supervision and accommodation, as in their holiday tours abroad. But at that time they did not think it would pay, and the matter was dropped. For a large number of the most incipient cases such tours would answer every purpose of change of scene, change of air, out-of-door exercise, etc., with these great advantages: —that they could be started on very short notice, that the tourist would always be within reach of home and of friends in case of need, would be secure of reliable medical aid in case of intercurrent illness, and of periodic medical examination—to see whether he was still on the right track, and if not to alter it—and he would be able to get that very important desideratum, wholesome English fare, and to avoid that apparently inevitable accompaniment of foreign travel, the risk of being poisoned by bad water and stinks.

Such tours would also have the very important recommendation that they would be available for girls and women, who, while unhappily more liable to consumption than men, are, by numerous inevitable difficulties, unable to undertake the more enterprising schemes of climatic change which are open to men. Fortunately, these disabilities are being every day reduced in number; and even the journey from London to the Adirondack Mountains has been made comparatively easy. The Atlantic voyage is now almost a holiday trip, and the extension of the Chateaugay Railroad enables the traveller to leave New York by a night express, to breakfast at Plattsburgh, and reach Saranac by noon next day without further change. But I advise any patient going on this excursion to stay in New York long enough to consult Dr. Loomis—who has done so much to develop the Adirondack cure,—and take full instructions from him as to the management of "life in the mountains." [32]

Nothing in the way of travel can be more suited for the treatment of the true first stage of bacillary consumption than the tour of the Rocky Mountains,[33] the Adirondacks, the Swiss Alps, the high grounds of South Africa, the northern parts of New Zealand, and the like. The questions of more land or more sea, more heat or more cold, higher altitudes or

lower altitudes, must be settled after a full consideration of the specialties of each case.

However advantageous it may be in the later stages of developed phthisis to settle patients for a season at some selected health resort, this is not the best thing for the majority of persons in the true first stage. Except in the case of children, who can find sufficiently invigorating and amusing occupations within a limited area, and who, as a rule, should be exposed to as little travelling as possible.[34]

It is comparatively easy to surround the very young with all the conditions of existence essential to health, without going far afield if people will only be guided by common sense. "Happily the day has passed when it was believed that there must be a specific climatic cure for every disease. No professional or popular delusion has led to the sacrifice of more lives, or to more domestic disruptions, blasted hopes, and weary disappointed longings for that which, while impossible, was still believed to exist. The conviction of to-day that even the most typical climate is only a more or less powerful adjuvant to other medical treatment—a more or less favorable condition of existence—brings us within the regions of reason and common sense. . . . It easily occurs to the mind of the invalid that, by rushing after the region of lowest mortality for each season, he may defeat death. But too often this idea leads to almost as mad a dance as that of a moth around a candle."[35]

In prescribing the exact form which the change in the surroundings and in the manner of living is to assume, we must always take into account the temperament and possibilities of the individual, and also any strong longings for this or that, the indulgence of which may give the chief stimulus to restorative treatment.

"Let him who, while yet young and in health, only fears the future inroads of some constitutional or hereditary taint, and whose temperament rebels against the trammels of a cautious life at home,—let him, if he has no ties or responsibilities to prevent, take his life in his hands and his knapsack on his back, and go forth into this beautiful world of land and sea, mountain and river, valley, forest, and plain, to explore the grand battlefields of nature, and run the risks which such explorations inevitably incur, for the sake of the great harvest which they sometimes bring—saying to himself, 'Nothing was

ever done without risk, and nothing great without danger, therefore I have taken my determination.' To shut up such spirits for months in Davos, Bournemouth, the Riviera, Arcachon or any other health station in the world, is like attempting to keep swallows in a cage" (op. cit.)

But such stations may with advantage be utilized as resting places during certain phases of a health tour, as centres to and from which to travel, and especially as harbors of refuge during temporary illness or stress of weather, where domestic comforts and good medical advice can be secured.

For these and many other reasons it is advisable that patients who have passed the true first stage—in whom tuberculization is advancing, or who are in still later stages of phthisis — should be quietly settled in judiciously selected health stations.

But it is not for such cases that I have written this treatise. My object has been to point the way by which such fates may be avoided, by nipping "in the bud" the "worm" that would have fed upon the "damask cheek."

CHAPTER VIII.

Essentials of a Normal Diet.—A healthy adult man of average stature, taking moderate exercise, will require and can consume, daily, from 32 to 40 ounces avoirdupois of dry nutritious food, which should have the following characters:

1. About $\frac{1}{200}$ must be mineral matter.

2. From $\frac{2}{5}$ to $\frac{1}{2}$ may be water, leaving $\frac{3}{5}$—or not less than $\frac{1}{2}$—or from 15 to 20 ounces of anhydrous solid alimentary material.

3. Three or four ounces of plastic matter must be combined with three or four times that quantity of heat-giving material.

4. The heat-giving constituents must contain a mixture of fats (hydro-carbons) with saccharine materials (carbo-hydrates), in the proportion of about one of the former to three of the latter.

5. These heat-giving constituents should supply from 6 to 10 ounces of carbon, the exact amount required varying with season, exercise, etc.

6. The articles of food must be sufficiently varied to meet the requirements of the taste and of the appetite, and their mechanical and other conditions must be suited to the digestive powers of the stomach.

In addition to these characters, every complete diet must contain some potash-vegetable or fruit; and the total amount of water taken in 24 hours, including that contained in the dry food, must not be less than 70 ounces avoirdupois.

Diet Tables—For the maintenance of health in adults living in the climate of the United Kingdom. Each diet table contains all the essential elements of nutrition, in forms, quantities, and proportions necessary to the maintenance of health. The tables have been made complete without alcohol, leaving this to be ordered or not, according to circumstances. But,

if fermented liquors are added to any diet table, the quantity of carbon which they contain must be borne in mind.

It will be observed that the totals of the corresponding columns of the analyses are nearly the same in all the diet tables, showing the important fact that all the essentials of a normal diet may be equally secured in a diet that is simple and cheap and in one that is complicated and expensive. In all diets salt must be used, and in those which do not include potatoes, some other potash-vegetable, or fruit, or cresses, or lime or lemon juice must be taken. (See Table V.)

No. 1.

Liquid. Fluid Ozs.	Dry. Ozs.	Food for 24 hours.	Water. Ozs.	Plastic. Ozs.	Fat. Ozs.	Saccharine. Ozs.	Carbon. Ozs.
	6	Meat Poultry or Game (cooked).	3.97	1.35	.53	....	.42
	4	Fish	3.19	.66	.03	....	.02
	10	Bread	4.20	1.00	.07	4.53	1.93
	8	Potatoes	5.81	.14	....	1.84	.76
	2	Rice	.18	.10	.01	1.63	.73
	2½	Sugar	....	....	...	2.50	1.05
	2½	Butter	....	....	2.50	....	1.85
5		Milk	4.34	.25	.17	.21	.22
16		Coffee	15.77	....	....	.22	.10
16		Tea	15.05	....	....	.05	.02
17		Water	17.00	...	....	....	....
54	36		70.41	3.50	3.31	10.98	7.01
		In Plastic matter					1.89
		Total					8.90

No. 2.

Liquid. Fluid Ozs.	Dry. Ozs.	Food for 24 hours.	Water. Ozs.	Plastic. Ozs.	Fat. Ozs.	Saccharine. Ozs.	Carbon. Ozs.
	8	Meat, Poultry or Game (cooked).	5.29	1.80	.71	....	.56
	12	Bread	5.04	1.20	.08	5.44	2.32
	12	Potatoes	8.71	.20	....	2.76	1.14
	2	Butter	....	....	2.00	....	1.48
	2	Sugar	....	....	....	2.00	.84
5		Milk	4.34	.25	.18	.21	.23
16		Chocolate	15.20	.07	.32	.38	.42
16		Tea	15.95	....	....	.05	.02
16		Water	16.00	....	....	....	...
53	36		70.53	3.52	3.29	10.84	7.01
		In Plastic matter					1.90
		Total					8.91

No. 3.

Liquid. Fluid Ozs.	Dry. Ozs.	Food for 24 hours.	Water. Ozs.	Plastic. Ozs.	Fat. Ozs.	Saccharine. Ozs.	Carbon. Ozs.
	18	Bread	7.56	1.80	.12	8.15	3.47
	3½	Cheese	1.28	1.07	.89	.08	.70
	3	Bacon	.76	.25	1.88	....	1.48
	1½	Sugar	....	....	....	1.50	.63
5		Milk	4.33	.25	.17	.21	.22
20		Chocolate	19.00	.10	.40	.50	.50
20		Tea	19.93	....	....	.07	.04
20		Water	20.00	....	....	....	....
65	26		72.86	3.47	3.55	10.51	7.04
		In Plastic matter					1.87
		Total					8.91

No. 4.

Liquid. Fluid Ozs.	Dry. Ozs.	Food for 24 hours.	Water. Ozs.	Plastic. Ozs.	Fat. Ozs.	Saccharine. Ozs.	Carbon. Ozs.
	8	Fish	6.38	1.33	.06	....	.05
	16	Bread	6.72	1.60	.11	7.25	3.99
	8	Potatoes	5.81	.14	....	1.84	.76
	2½	Butter	....	....	2.50	....	1.85
10		Milk	6.93	.40	.28	.34	.36
20		Cocoa	19.10	.10	.60	.20	.56
	1½	Sugar	....	....	....	1.50	.63
25		Water	25.00	....	...	....	....
55	36		69.94	3.57	3.55	11.13	7.30
		In Plastic matter					1.93
		Total					9.23

No. 5.

Liquid. Fluid Ozs.	Dry. Ozs.	Food for 24 hours.	Water. Ozs.	Plastic. Ozs.	Fat. Ozs.	Saccharine. Ozs.	Carbon. Ozs.
	16	Bread	6.72	1.60	.11	7.25	3.09
	3	Peas	.41	.65	.04	1.40	.65
	4	Bacon	1.14	.33	2.50	....	1.98
	2	Cheese	.73	.61	.51	.05	.40
10		Milk	6.94	.40	.28	.34	.36
20		Coffee	19.71	....	....	.28	.13
	1	Sugar	....	....	....	1.00	.42
35		Water	35.00	....	....	....	....
65	26		70.65	3.59	3.44	10.32	7.03
		In Plastic matter					1.94
		Total					8.97

No. 6.

Liquid. Fluid Ozs.	Dry. Ozs.	Food for 24 hours.	Water. Ozs.	Plastic. Ozs.	Fat. Ozs.	Saccharine. Ozs.	Carbon. Ozs.
40		Milk	34.68	2.00	1.40	1.68	1.80
	4	Rice	.36	.20	.02	3.26	1.46
	3	Eggs (two)	2.15	.45	.32	....	.26
	2½	Sugar	....	....	....	2.50	1.05
	1	Butter	....	....	1.00	....	.74
	9	Bread	3.78	.90	.06	4.08	1.74
30		Water	30.00	....	....	....	....
70	19½		70.97	3.55	2.80	11.52	7.05
		In Plastic matter					1.92
		Total					8.97

Dietetic Directions for Consumptives.— Dr. Theodore Williams gives the following judicious dietetic directions for consumptives: "When the appetite is good, and a fair amount of open-air exercise can be taken, a dietary like the subjoined generally results in gain of flesh:

"8 to 9 A.M. Breakfast.—Bread (whole-meal if possible) and milk (¾ pint), or porridge and milk, rendered a little more digestible by the addition of a little ground malt, or A. B. C. cereals, white wheat, or Durber's wheat or hominy with milk; fried bacon, egg, or fish, or poultry; a cup of coffee or cocoa and bread and butter.

"1 to 2 P.M. Luncheon or Early Dinner.—Plenty of tenderly cooked meat, with potatoes and fresh vegetables; light farinaceous puddings, a little ripe or stewed fruit, and a glass of sound sherry, or a larger one of claret, or ½ pint of bitter ale.

"4 to 5 P.M.—½ pint of milk with a rusk or biscuit.

"7 to 8 P.M. Dinner or Supper.—Plainly-dressed white fish, to be followed by meat, mutton or beef, alternated, to give variety, with poultry and game, vegetables; sweets as at luncheon, and a glass of sherry with water, or a larger one of claret, hock, or Chablis, followed by a cup of hot coffee or a glass of hot water, if needed to assist digestion.

"In cases where the appetite is very capricious, as in weakly women, less food can be taken at a time, and more frequent feeding is requisite, so that a dietary something like the following is to be preferred:

"7 A.M.—½ pint of warm milk with a dessert-spoonful of brandy or rum.

"9 A.M. Breakfast.—Milk with cocoa or coffee, bread and butter, bacon, fish, or poultry.

"11:30 A.M.—Eggflip, *i.e.*, one egg beaten up with a dessertspoonful of brandy; or ½ pint of milk or a glass of koumiss or kéfir.

"1 to 2 P.M. Luncheon.—As in first dietary.

"4 P.M.—Same as at 11:30, or, if desired, a cup of tea with milk and biscuit, or slice of bread and butter.

"7 P.M.—Beef tea with toast and a glass of wine.

"10 P.M.—Some farinaceous food, such as milk, gruel, arrowroot, etc.

"These forms may, and should, be endlessly varied according to the digestive capacity of the individual, provided always that the relative proportions of the food-stuffs are maintained, and the stimulants introduced in amount and kind to assist digestion and prevent waste."

Special Nutritive.—In my work on "Loss of Weight," I said, in 1880, what I may repeat, after nine years' further experience, "I cannot speak too highly of the following 'special nutritive' (pancreatized milk, egg, and arrowroot, with wine or brandy), as a means of feeding and fattening patients unable to take or to digest a sufficiency of solid food. I have used it very largely in practice (and so have numerous medical friends), and I have seen emaciated patients grow fat upon this food alone. This need not surprise us, for its analysis shows it to contain all the essentials of a normal diet in proper quantities and proportions in the forms most easy of digestion and assimilation.

"Beat up an egg, both white and yelk, quite smooth and free from stringy particles; stir it well into half a pint of hot milk in which enough arrowroot has been boiled to make it about as thick as cream; add a wineglassful of sherry or a tablespoonful of pale brandy, and some fresh nutmeg and sugar; mix all thoroughly by pouring from cup to cup, and when it is just cool enough to be borne by the mouth (about 140° F.), stir in ten grains of powdered pancreatin.—A person living entirely on this diet should drink fresh-made lemonade when thirsty."

Invalid Soup.—The Special Nutritive may be adantageously alternated with the following invalid soup, which may be taken cold as a jelly, or warm as a soup. Since I first published the recipe in 1864 it has proved invaluable in a large number of cases. It paves the way for solid food.

"Gravy beef 1 lb., scrag of mutton 1 lb., isinglass 2 oz.,

vermicelli 3 oz., mushroom ketchup three tablespoonfuls, corns of allspice 24, sage a sprig, cold water three quarts.

"Put the isinglass and the meat, cut small, into the cold water, gradually boil, skim well, and then add the other ingredients; simmer four or five hours till reduced to one quart. Strain through a fine hair sieve, and carefully remove all fat; add salt to the taste. Calfsfoot may be used instead of isinglass when procurable; and when not contra-indicated a little solution of cayenne pepper may be added; and the flavor may be varied by the substitution of Worcester or some other wholesome sauce for the ketchup. A full dose of powdered pancreatin should be taken directly after the soup.

Koumiss.—The following forms a sort of Koumiss, and is not only pleasing to many palates, but to stomachs that cannot bear milk in the usual forms:

New milk well curdled, ℥ viij. Remove half the curds and put the rest of the curds and whey into a wine bottle; add ℥ iij. of "Hungarian sparkling" briskly effervescing (to keep the wine well "up" for further use, stop the bottle with an elastic cork), shake the whole sharply, and pour it frothing into a tumbler.

See other recipes for the sick-room in "Diet and Regimen," 7th Ed.

Bacteriology of Snow.—"While the bacteriology of ice and hailstones has been studied with considerable success by Drs. Fränkel, Bischoff, Mitchell Prudden, Pumpley, Hills, Stoben, A. V. Poehl, Bordone-Ufreduzzi, Bujwid, etc., that of snow has been, up to the present, almost wholly neglected. Even in Russia the subject has been touched only in a cursory way by Professor A. V. Poehl in a paper on the water-supply of St. Petersburg, in the "Vratch," Nos. 8 and 9, 1884, p. 119. In it he points out: 1, that snow always contains viable microbes liquefying gelatin; 2, that, when snow falls, the first portions invariably contain greater numbers of bacteria than the subsequent ones (for example, 8,324 per 1 cubic centimetre of snow-water against 3,380 several hours later); 3, that, when snow lies on the ground, the superficial layers become richer in microbes (for example, 780 just after the fall, against 962 about three hours later). The fact is of interest from a sanitary point of view, as Dr. Poehl's researches furnish an additional proof that exposure of microbes to low

temperatures does not destroy their vitality; at least, in certain species of micro-organisms. In many countries, such as Russia or Sweden, snow forms, so to speak, a natural ground or soil during several months of the year, receiving excrementitious matter and every possible kind of refuse and filth. In spring, when the snow melts, it is imbibed by the soil, carrying with it all the polluting matters referred to. Hence an interesting question arises: Are such microbes as happen to be present in these matters in any way changed by their contact with snow, or not? This point can be determined only by further bacterioscopic researches. A contribution to the subject has just been published in the "Vratch," No. 37, 1888, p. 727, by Dr. F. G. Ianovsky, of Kiev, who has examined bacterioscopically, under Professor K. G. Tritshel's guidance, a February snow in its purest state, collected both immediately and from one to three days after its fall. This observer has found: 1, that even when collected during its fall, snow is invariably found to contain living bacteria in considerable numbers, varying from 34 to 463 per 1 cubic centimetre of snow-water; 2, that their number does not decrease from exposure of snow to low temperatures (—16° C.=3.2 Fahr.) for several days; 3, that the following three species of microbes are met with constantly in great numbers: *a*, a large diplococcus composed of ovoid cocci, endowed with energetic motion, and characterized by its rapidly liquefying jelly; the test-tube culture, on the third day, forming greenish colonies along the track of the needle, assumes the shape of a funnel-like sac with a whitish flocculent deposit, while on the fifth the whole medium becomes liquefied, the precipitate sinking to the bottom; on agar a pale grayish-white streak is formed at the site of inoculation; on potato a fairly thick white film; *b*, small-sized cocci, often arranged two and two, energetically mobile, and slowly growing on jelly without liquefying the medium, the growth proceeding solely along the track of the needle in the shape of a narrow stripe consisting of non-coalescing minute points of a yellow color, while on the surface the colony is seen as a grayish-white, circular, slightly prominent patch with somewhat fringed edges; on agar the coccus forms a white streak with sinuous edges; on potato a gray film with a brownish tint; *c*, very large cocci liquefying jelly as late as three weeks after inoculation, and growing along

the track of the needle in the form of a sharply-defined streak of a beautiful pink color, with a slightly elevated pink circular patch or "cap" on the surface; on agar the microbe forms a freely-spreading white film with a rosy tint; on potato a thick, tallow-like, pink coat with sharply-defined fringed contours. 4. That the first two species (*a* and *b*) are also met with commonly in the water of the river Dnepr, which flows through the town (vide Dr. Ianovsky's bacterioscopic examination of the water, published in the "Meditzinskoië Obozrenië," Nos. 9 and 10, 1888, p. 975), while the peculiar pink micrococcus seems to occur only in snow. 5. That, generally speaking, the microbes liquefying jelly, in falling or recently fallen snow are met with invariably in far greater numbers than in snow which has been on the ground for some time; this, in fact, very often contains only such bacteria as do not liquefy gelatin. 6. That the bacteria of snow originate partly from aqueous vapors which are transformed into snow, partly and chiefly from the air, that is, they are carried away by the snow-flakes on their passage through the atmosphere."—"British Medical Journal," December 15th, 1888.

The Comparative Antiseptic Value of Various Chemical Substances.—"At a recent meeting of the London section of the Society of Chemical Industry, Mr. C. T. Kingzett, F.I.C., F.C.S., read a paper on the above subject. The author began by expressing his belief that 'in all probability' various antiseptic chemical agents act in a similar way upon all sorts of micro-organisms, either (1) by killing the organisms directly, or (2) by causing their death indirectly by altering the composition of the medium in which they live. He considers that this view receives support from a careful study of the chemical changes which are produced by various micro-organisms, all of which may be classed, according to Mr. Kingzett, under two heads—namely, hydration and oxidation. Moreover, he believes that the most important chemical poisons (ptomaïnes, etc.), which are produced by certain pathogenic micro-organisms, are formed by a process of hydrolysis, set up by the organisms themselves.

"Having thus stated his views as to the practical importance of studying the action of various 'antiseptics' (germicides), he proceeds to describe three series of experiments bearing on this point.

"In Series 1 the action of chlorides, nitrates, and sulphates of various metals was tried, with the general result that these salts of the metals of the alkalies and alkaline earths were found to be very deficient in antiseptic power, whilst the corresponding salts of manganese, zinc, tin, iron, lead, and aluminium have a decided value, whilst the metals copper and mercury are superior to all others in this respect, mercury being better than copper.

"In Series 2 the antiseptic action of 'sanitas,' 'salufer,' and 'bactericide' (three antiseptic solutions patented by Mr. Kingzett) are compared with solutions of the more potent antiseptic metallic solution of Series 1, with results which are favorable to the new solutions. Indeed, the author asserts that his solutions, which contain peroxide of hydrogen, possess an advantage over mercuric chloride, in that they act as oxidants, which the latter does not and cannot do.

"In Series 3 a number of experiments are described in which mixed solutions were employed, some of the results being highly interesting; for example, chloroform is found by Kingzett an inferior antiseptic to chloral; boric acid he finds superior to borax, though borax is said to be superior to a mixture of borax with boric acid added to exact neutralization; and, as a rule, acid solutions are found to be better antiseptics than alkaline ones.

"The author has tabulated his results, and proposes that his tables shall be used for purposes of comparison in case of the introduction of any new antiseptic solutions."—"British Medical Journal," November 24th, 1888. On the subject of antiseptic medication reference should be made to the valuable and practical contribution of Dr. Shingleton Smith, which appeared while these pages were in the press, entitled "Some Recent Developments of the Germ Theory," etc.

BIBLIOGRAPHPY.

1. On Loss of Weight, Blood-spitting, and Lung Disease, By Dr. Dobell, Second Edition, 1880.—2. Tubercular Joint-Disease, etc. Lectures delivered at the Royal College of Surgeons, England, June, 1888. By Arthus E. J. Barker, F.R.C.S., British Medical Journal.—3. The Diagnosis of Early Phthisis by the Microscope. By Francis Troup, M.D. Edinburgh Medical Journal, July, 1888.—4. Tuberculosis and Tabes Mesenterica. Lectures delivered before the Honorable the Grocers' Company in the University of London, July, 1888. By G. Sims

Woodhead, M.D., etc. Condensed report in The Lancet.—5. Micro-Organisms and Disease. Third Edition. By E. Klein, M.D., F.R.S., etc., 1886.—6. Pulmonary Consumption. By C. J. B. Williams, M.D., etc., and Charles Theodore Williams, M.D., etc. Second Edition, 1887. —7. Anatomy of the Lymphatic System.—8. Paper by the Author, On Fat and Starch in Nutrition of Children. Reprinted in Diet and Regimen, Seventh Edition.—9. Dr. Dobell's Report on the Progress of Medicine, 1869, page 105.—10. The Medical Aspects of Bournemouth, page 205. By Dr. Dobell.—11. Nocard's Experiments on Cats, page 393, and Alimentary Portal, page 384.—12. G. S. Storey, Chemical Society, 1888.—13. Prof. G. F. Fitzgerald, Bath Meeting of British Medical Association, 1888.—14. Report in Chemist and Druggist, April 7th, 1888.—15. British Association Meeting, 1888.—16. Croonian Lecture, Royal Society, 1888. —17. British Medical Journal, January and February, 1866.—18. Researches on Myohæmatin and Histohæmatin. By C. A. MacMunn, M.A., M.D. Proceedings of Royal Society, No. 240. Read Oct. 19th, 1885. Ditto, Transactions of Royal Society, 1885. Read Nov., 1885. Observations on the Chromatology of Actiniæ. Transactions of Royal Society. Read January, 1885. Further Observations on Enterochlorophy and allied Pigments. Transactions of Royal Society. Read April 30th, 1885. Further Observations on some of the Applications of the Spectroscope in Biology, etc. Proceedings of Birmingham Philosophical Society. Read April 8th, 1886.—19. Contributions to the Study of the Cadaveric Alkaloids, the Ptomaïnes and Leucomaïnes; their Physiological and Pathological Significance in relation to Scientific Medicine. By Dr. A. M. Brown, 1886.—20. Pharmacology and Therapeutics. By Dr. Lauder Brunton. Third Edition.—21. British Medical Journal, January 28th, 1888.—22. On Fat and Starch in the Nutrition of Children, A paper by Dr. Dobell in Seventh Edition of Diet and Regimen.—23. Reynolds' System of Medicine, 1871.—24. On the Value of the Tubercle Bacillus in Clinical Diagnosis. By Percy Kidd, M.D., and H. H. Taylor, F.R.C.S. May 22d, 1888.—25. Demonstrations of Diseases in the Chest, and their Physical Diagnosis. By the Author, 1858.—26. On the True First Stage of Consumption. By the Author, 1867.—27. Lectures on the Germs and Vestiges of Disease, and on the Prevention of the Invasion and Fatality of Disease. Reprinted in Part II. of the Author's work On Diet and Regimen, etc., Seventh Edition.—28. See the Author's opinions on Alcohol, pp. 7, 8, Diet and Regimen, Seventh Edition.—29. Special Action of the Pancreas on Fat and Starch. A paper by the Author. Proceedings of the Royal Society, No. 97, 1868.—30. On Diet and Regimen in Sickness and Health, etc. By Dr. Dobell. Seventh Edition.—31. A System of Physical Education. By Archibald Maclaren, of the Oxford Gymnasium.—32. The Wilderness Cure. By Marc Cook. Published by Wm. Wood & Co., N. Y. Price $1.00. Contains full directions as to routes, preparations, and locations for camping out, cost, etc.—33. Chapter IX. The Medical Aspects of Bournemouth. By Dr. Dobell, Second Edition.—34. Bournemouth for Children. By Dr. Dobell in the Prov. Med. Journal, Feb., 1889.—35. The Medical Aspects of Bournemouth. By Dr. Dobell, Second Edition.

ECTOPIC PREGNANCY

AND

PELVIC HÆMATOCELE.

BY

LAWSON TAIT, F.R.C.S., EDIN. & ENG., LL.D.,

M.D. (Honoris Causâ) of the University of New York, Union University of Albany, and the College of Physicians and Surgeons of St. Louis; Professsor of Gynæcology in Queen's College, Birmingham; Surgeon to the Birmingham and Midland Hospital for Women; Honorary Consulting Surgeon to the Brooklyn Hospital for Women, to the Nottingham Samaritan Hospital for Women, to the Wolverhampton Dispensary for Women, and to the West Bromwich District Hospital, etc.; Honorary Fellow (late President) British Gynæcological Society; Honorary Fellow, American Gynæcological Society; President of Birmingham and Midland Counties Branch British Medical Association, etc., etc.

ECTOPIC PREGNANCY AND PELVIC HÆMATOCELE.

AFTER much consideration I have adopted the phrase ectopic pregnancy, designed originally by Dr. Robert Barnes, as by far the best which can be applied to the curious and most interesting displacement which we have first to consider, for it gives a convenient and very complete definition without expressing any theoretical explanation of the condition. The cavity of the uterus is the proper place for any gestation, but a gestation may be ectopic without being extra-uterine, as in what has been called the interstitial or tubo-uterine variety. I believe we might call all ectopic gestations "tubular pregnancies," but that would be hardly fair to those who still cling to the belief in the occurrence of the ovarian kind. "Ectopic" includes them all and therefore I adopt it.

The literature of this subject is very extensive and the confusion which exists in it is almost as great, but there are two works which stand prominent for different reasons, and to these I propose to make some extended allusions, for I am greatly indebted to both of them for valuable information. The first is that of Dr. William Campbell, a teacher of midwifery in Edinburgh, who published in 1842 a work in which its industrious author has collected in all probability all the material up to his time, thus forming a mine from which many a quotation has been made by subsequent writers without any kind of acknowledgment. Campbell seems to have had no great critical acumen, however, and his material is confusing alike in its abundance and its utter want of arrangement. His notions of pathology were of the vaguest kind and his capacity for believing all that was told him must have been extensive. His work, however, stands as a landmark in the literature of the subject, as the first real effort to place into its

appropriate position of importance a subject which, up to that time, seems to have been regarded more as a curiosity than as one of the most dreadful calamities to which women can be subjected. He also exhaustively investigated the literary history of the subject, and his book is of great interest in showing how often discoveries have been made and how easily they are forgotten.

A book of a very different order is that of Dr. John S. Parry, of Philadelphia, published in 1876. It is at once remarkable for its scholarly research and fine critical sagacity. Most unfortunately this promising author died in the same year, and I never look at the finely cut, handsome young face, which looks out upon me from the book as its frontispiece, faced by a pathetic letter from his mourning widow, but I become persuaded that in Parry's death one of the greatest lights in gynæcology of my time was lost to us. Had he lived to give us a second edition of his book, its few incompletenesses would have been filled up and its few errors rectified. Where he has got astray has chiefly been by the delusive use of statistics, a point which I shall deal with by-and-by.

I have already discussed at length my view upon the physiological process of impregnation and the machinery concerned in it, so that I need not do more here than repeat that the uterus alone is the seat of normal conception, that as soon as the ovum is affected by the spermatozoa it adheres to the mucous surface of the uterus; that the function of the ciliated lining of the Fallopian tubes is to prevent spermatozoa entering them and to facilitate the progress of the ovum into the proper nest; further, that the plications and crypts of the uterine mucous membrane lodge and retain the ovum either till it is impregnated or till it dies or is discharged.

With such views it is easy to understand the cause of tubal pregnancy, for we have only to turn to the papers of Arthur Johnstone and Bland Sutton, to see that desquamative salpingitis could at once put the mucous lining of the tube into a condition exactly similar to that of the uterus, and in that condition access of spermatozoa would be possible, retardation of the ovum in the tube would be inevitable, and its immediate adhesion to the tube-wall after impregnation would be as easy and as likely as its occurrence in the uterus. The cause, therefore, of ectopic gestation or tubal pregnancy will be any pro-

cess or accident which has reduced the Fallopian tube, so far as concerns its internal lining surface, to the same condition as the uterus.

Virchow long ago drew attention to the fact, that at post-mortem examination of cases of ectopic gestation ending fatally at the period of primary rupture, traces of previous pelvic peritonitis were often found, and nothing is more common than to find a record of such attacks in the history of cases that come under clinical investigation. Indeed there is one fact about these cases which is very notable in the relation, that a very large proportion of them have a history of prolonged sterility and menstrual suffering, showing that their procreative machinery was out of gear. Thus we often have the history common to tubal mischief that after a first labor there was an illness with marked symptoms of pelvic trouble, then a long period of sterility, then the ectopic gestation ending in rupture. In my clinical records of such cases I have laid special stress on this feature of their history as a guide to diagnosis. Parry impresses this by saying that "women who have become pregnant with a child outside the uterine cavity frequently show a previous inaptitude for conception. The interval between marriage and the first impregnation is frequently long. If the woman has borne children a period of sterility frequently precedes the extra-uterine pregnancy," and he gives a long list of authorities from whom he elicits confirmatory statements. This is eminently suggestive of the view I have advanced that ectopic gestation is caused by destruction of the proper ciliated epithelium of the tubes, and there are many other points to be successively discussed, which all point in the same direction. Indeed there is no argument against this save the belief that impregnation takes place usually in the tube. For this belief there is no foundation in fact, nothing at all except the misinterpreted facts obtained by experiment in the lower mammals. In these, spermatozoa have been found high up in the cornua of the bipartite uteri and these cornua have been erroneously supposed to be Fallopian tubes, whilst they are nothing of the kind. The Fallopian tubes do not really exist save in the higher order of animals who have assumed the upright position. If we accept this view the physiology of the process of reproduction is immensely simplified and the pathology of ectopic gestation becomes intelligible. I cannot see

that any other views than these are consistent with the recent discovery of Arthur Johnstone and Bland Sutton, nor indeed can any others be reconciled with the facts of ectopic gestation as unravelled by modern surgery.

We have now to deal with the varieties of ectopic gestations and I propose at once to dismiss all previous classifications as inconsistent with the facts as they have occurred in my own experience and incompatible alike with the view of the explanation of the cause of ectopic gestation which I have offered and with the physiology of impregnation. The uterus being regarded as the only site possible for normal pregnancy and the tract through which the ovum passes and in which it may be impregnated in the abnormal process, it follows as a matter

FIG. 1.—Section of Normal Fallopian Tube. (After Bland Sutton.)

of course that all ectopic gestations must, in their origin, be tubal. A possible exception to this may be the impregnation of an ovum in its vesicle before it leaves the ovary—a matter I shall discuss immediately.

A clinical distinction of two kinds of tubal pregnancy must be made, though pathologically they must be regarded as quite similar. This division occurs between the cases in which the fertilized ovum becomes attached to the inner wall of the tube where it is free from uterine tissue, and those cases where the ovum cavity is formed by the distention of the tube at that part imbedded in the structure of the uterine wall. These cases have been called "interstitial" and I propose to retain this term.

The process of development of an ovum in the tube at any part of it, inevitably results in rupture of the tube. In the

"interstitial" cases, the rupture, so far as is known, always takes place into the peritoneal cavity, and I cannot imagine any other way in which it might go, though we have assertions that a diagnosis has been made of tubal pregnancy which has ended by the ovum being discharged through the uterus. Such cases are easily dismissed from serious discussion, for I have never seen a preparation of interstitial pregnancy which could, by any possibility, have been diagnosed from normal pregnancy before the period of rupture. It is easier to believe, therefore, that such cases as I speak of have been errors of diagnosis than that the uterine tissue has been ruptured and the pregnancy has become intra-uterine. And here let me state that about this subject, as indeed about nearly everything else in this book, I do not give as a fact anything which has not been verified, either by post-mortem or ante-mortem examination. Any man who gives an opinion that he diagnosed a tubal pregnancy, or any other lesion, and that its course was this, that, or the other, merely upon the unaided discrimination of symptoms or the dim light of a pelvic examination, I regard with so much suspicion that I do not accept his evidence for argument save under exceptional circumstances. Post-mortem records, museum specimens, and the facts observed at operations yield evidence which is usually incontrovertible, and such as these only do I care to use. The interstitial ectopic gestation ruptures uniformly as I have said, and so far as we know, into the peritoneal cavity. The period of its rupture seems to be variably from three to twenty weeks; a fact which I derive from post-mortem record and museum specimens solely, for I have had no operative experience of this disaster and have had only one case within my own associations.

Ectopic gestation in the free portion of the tube infallibly involves rupture at some part of its progress before the fourteenth week, in fact I think I might say the twelfth week, for out of an enormous number of specimens I have examined I have entirely failed to satisfy myself that rupture has been delayed later than the twelfth week, and I have seen it as early as the fourth week of gestation. This rupture I propose to term "primary rupture," and it constitutes in one direction, the most disastrous accident known among women.

This tubal rupture takes two directions: (*a*) into the peritoneum, which is the fatal form; and (*b*) into the cavity of the

broad ligament, a form which yields the variety of ectopic gestation which I propose to call extra-peritoneal, which was called the "sous-peritoneo-pelvienne" variety by Dezeimeris, and which alone yields all the cases which go on to the period of viability, all the lithopædia, all the suppurating cysts discharging into bladder, rectum, etc., and also the cases which by secondary rupture of the ovum cyst, get called "abdominal pregnancy."

This is the view of ectopic gestations I first propounded in

Fig. 2.

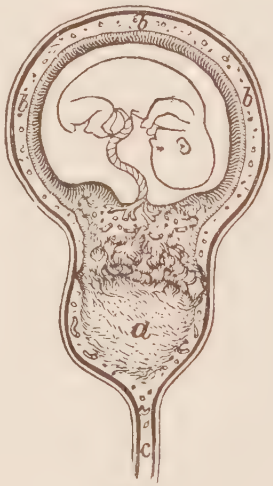


Fig. 3.

Figs. 2 and 3.—Diagrammatic Section of Fallopian Tube representing the two directions of Rupture, 2, into the peritoneal cavity; 3, into cavity of broad ligament; *a*, clot at point of rupture; *b*, wall of Fallopian tube; *c*, cavity of broad ligament with (3) folds separated by hæmic effusion *a*.

1873, and Parry commended it with the expression that it at least had the merit of simplicity. I have, during the fifteen years which have elapsed, missed no possible opportunity of examining preparations of ectopic gestations, and nearly a hundred cases have passed under my own observation, directly or indirectly, for post-mortem investigation or surgical interference, and I have not found a single fact inconsistent with the views just briefly announced and now to be discussed at length. On the contrary, these views of ectopic gestation bring harmony where formerly all was discord, make orderly what has hitherto been nothing but confusion. We may,

therefore, construct a genealogical table of ectopic gestation, which gives the history at a glance as follows:

SCHEME OF ECTOPIC GESTATIONS (in tubo-ovarian tract).

I.—Ovarian, possible but not yet proved.

II.—Tubal, in free part of tube, is (*a*) contained in tube up to fourteenth week, at or before which time primary rupture occurs, and then progress of the gestation is directed into:

(*b*) Abdominal or intra-peritoneal gestation uniformly fatal (unless removed by abdominal section), primarily by hemorrhage, secondarily by suppuration of the sac and peritonitis;

(*c*) Broad ligament or extra-peritoneal gestation;

(*d*) may develop in broad ligament to full time and be removed at viable period as living child.

(*e*) may die and be absorbed as extra-peritoneal hæmatocele.

(*f*) may die and the suppurating ovum may be discharged at or near the umbilicus, or through bladder, vagina, or intestinal tract.

(*g*) may remain quiescent as lithopædion.

(*h*) may become abdominal or intra-peritoneal gestation by secondary rupture.

III.—Tubo-uterine or interstitial is contained in part of tube embraced by uterine tissue, and, so far as is known, is uniformly fatal by primary intra-peritoneal rupture (as *b*) before fifth month.

A few cases of pregnancy in hernial sacs have been unearthed by Campbell and Parry, but these, so far as can be judged by the somewhat meagre details and unsatisfactory accounts of most of them, can hardly be regarded as instances of ectopic gestations. They are rather instances of a hernial uterus in which impregnation has been accomplished.

The first division of our subject in natural order is the much-discussed "ovarian" gestation. Concerning this I may well quote a sentence from Parry, for though it is not directly applied to this point it most truly may be: "Special treatises on obstetrics, as well as periodical medical literature, teem with statements which are utterly unreliable, and which are calculated to mislead investigators of this subject."

The beliefs prevalent on the subject of impregnation naturally enough have always influenced the theories of writers on ectopic gestations, it is so now in my own instance, and therefore we find writers of the time of Haller believing that ovulation was excited by coitus, expressing many strange notions

about abnormal pregnancies. The belief that the spermatozoa reached the ovum in the ovary and that impregnation was effected there, brought about the notion at the end of last century and the beginning of this, that most ectopic pregnancies were ovarian. Hausman made a series of observations which were regarded as final, and therefore the belief not only became universal but it infects our text-books to this day, for authors of text-books copy one another with scrupulous fidelity and unblushing absence of acknowledgment. But if Hausman's observations on animals are carefully perused, and of course no writer of a text-book would ever think of going to the original authority for information, it will be found that the observations on different animals give results so inconsistent with one another that it becomes absolute simplicity of intellect to attempt to apply them to the human being. Finally, Sir Everard Home published a paper in the "Transactions of the Royal Society" before his audacious inventions were made manifest), and the belief in ovarian pregnancy became a creed, and it remained so till Velpeau raised the cry of unbelief. Even Campbell (1842) says that he believes cases of ovarian pregnancy are not so rare as his predecessors had asserted. Yet he quotes Velpeau as having examined four such cases, aided by two competent assessors, and as having easily determined that the ovary was not involved in three of them: "but in the fourth they experienced not a little difficulty in determining the product of conception, which did not exceed in size that of a flea, to be placed, not in the substance, but in the cyst between the peritoneal and proper tunic of the ovary." At another place (p. 29) he describes the occluded and distended tubes of a prostitute from one of which a white oval body the size of a garden pea with some white viscid matter (doubtless an old pyosalpinx) was forced out by pressure, as an ovarian pregnancy. Again he quotes the dissection of a child aged thirteen, a confirmed masturbator, in whom was a dermoid cyst of the left ovary, as confirmation of his view. So credulous is Campbell that on a point like this his book is absolutely foolish. He has, however, discovered a number of descriptions which might be accepted as authentic but from their extreme antiquity (1682, 1697, 1735, and 1767). Unfortunately the preparations of none of these cases can be traced, even the instance said to be visible in the museum at Wurzburg seems to

have disappeared, and certainly in modern times there has not been exhibited any preparation which can bear the test of critical investigation.

Of course it is impossible to admit any case as one of ovarian pregnancy in which no post-mortem examination had been made, indeed even when such an examination is made it would have to be at the hands of a competent observer only that an assertion of an ovarian pregnancy could be accepted. The uterus and both tubes would have to be recorded as intact and we should have one ovary present and the other not to be accounted for save by its existence on the cyst of the ovum; and in the cyst wall of such a case microscopic evidence of the presence of the ovarian tissues would be required.

In several cases of tubal pregnancy which I have dissected it was a matter of the utmost difficulty to find the corresponding ovary, even when it was perfectly clear that the seat of the pregnancy was one of the Fallopian tubes. In one of my dissections I could not find the ovary, and yet that case was, with perfect certainty, one of tubal pregnancy. In Spiegelberg's paper there is only one case cited to which these tests apply with any degree of satisfaction, and therefore I give the details in full.

An abdominal section was performed under circumstances of great difficulty, and after peritonitis had been some time in existence, the sac had become closely adherent to the great intestine and to the right wall of the pelvis. On both sides the tubes were normally distributed, but the left one, after a course of 7 cm., disappeared on the walls of its broad ligament. The right tube extended 10 cm. along the upper edge of the thickened broad ligament toward a sac which was united by the ligamentum ovarii to the ala vespertilionis of the uterus; it had a diameter of 10 cm. and was in a collapsed condition. After the tube had reached the sac it could be traced along its surface for a distance of 22 cm. and was permeable for a distance of 12 cm., and in the remaining 10 cm. of its length it disappeared as a narrow, smooth band on the outer surface of the sac. There was in this neighborhood a small dermoid cyst in the wall of the sac without any distinct boundary. The sac itself had two layers, the outer of which was thick and firm, and the inner one fine and delicate, these two being capable of separation. The inner layer was clearly

the chorion, for over its greater part it had the structure of placenta, which was thickest at the bottom of the sac and thin at the upper part.

Spiegelberg therefore concludes that the right ovary was the bag containing the child. He could find no ovary on the right side, but he found distinct ovarian elements in the outer wall of the sac. It must be pointed out that, in the first place, the post-mortem examination is admitted to have been not very efficiently performed, and the description given of the tube makes it, I think, quite as likely that it was a case of pregnancy in the broad ligament which resulted from the rupture of the tube on its lower aspect—that being the most common variety of the tubal pregnancies which are not fatal in their early rupture—as that it was a case of ovarian pregnancy. The fact that there was present an ovarian tumor is proved by the existence of a dermoid cyst. This would account for the somewhat wide distribution of ovarian elements in the wall of the sac, and as Spiegelberg does not claim to have found ovarian elements all over the wall of the sac, I think we may be quite justified in being somewhat sceptical even about this case; though I frankly admit that the eminence of the observer and the manifest care with which all his records are given make it quite possible that his conclusions are correct.

In a paper published by M. Puech upon this subject he describes a case in which the left Fallopian tube, like the right, was fixed behind the ovary by adhesion, but had remained permeable. Its pavilion was closed in a great measure, but not completely, and admitted a probe. The left ovary measured 46 mm. long, 26 mm. broad, and 18 mm. thick. It contained Graafian follicles of various degrees of development, the largest being 8 mm. in diameter. On its outer extremity was a rounded body about the size of a large cherry, its largest diameter being 20 mm., while its smallest was 12 mm. Its envelope was transparent and furnished with well-marked reticulated vessels. At one spot a deep violet coloration was seen over a space about the size of a lentil, and around this the envelope was thickened. Over most of the rest of the surface a yellowish substance could be seen through the translucent envelope. On opening the cyst with scissors a prominence with a villous surface was found attached at the area of coloration, while over the rest of the surface a layer ½ mm. thick

could be easily separated from the cyst wall. The villous prominence was furnished with large vessels, and formed a semi-ellipsoid measuring 11 mm. by 10 mm. On incising this with cataract scissors it was found to contain a cavity distended by a clear fluid, and in the fluid floated an embryo in the form of a vermiform body 1 mm. long, curved in the middle and swollen at one extremity. It was enveloped in an excessively delicate membrane by which it was fixed to the presumed chorion.

Of course the whole conclusion in this case depends upon the assumption that this vermiform body, only 1 mm. long, was an embryo. It may have been one, but certainly there is no proof advanced in favor of this view; and although I am by no means prepared to deny its accuracy, I am certainly very doubtful about it. If it was an embryo it could only have been one of a few hours' existence, and one could hardly expect to find the machinery of the whole process so defective that the pavilion of the tube—the most important part of the whole machinery—was so damaged as to be, according to M. Puech's description, almost closed and fixed behind the ovary by adhesion. One would have at least expected this adhesion to have been over the seat of the rupture, and yet it is distinctly stated not to have been so. I have seen so many queer-looking things in ovarian cysts and follicles that I am not inclined to admit that this vermiform body has been shown conclusively to have been an embryo.

A very important paper was published in 1859 by Professor Arthur Willigh ("Vierteljahrbuch für Pract. Heilkunde") in which the author suggests that critical investigation by means of the microscope is necessary to determine the reality of the so-called ovarian gestation. By this test he dismisses absolutely the evidence of a number of preparations which had been labelled in various museums as ovarian pregnancy, even one to which there was attached the great authority of the name of Kiwisch.

Such a test is wanted, for instance, in the case narrated by Dr. Walter, of Dorpat ("Monatschr. für Geburtsh.," Ap., 1862). There the account is given merely to the effect that the right ovary had become developed into a long tumor, its long axis being in the same direction as the body of the full-grown child, and that no other trace of the right ovary could be discovered;

but some evidence is needed more than is given that this tumor possessed ovarian structure. We know perfectly well that an ovary can be enlarged by cystic growth indefinitely, but we can always identify by microscopic investigation the origin of the growth.

Walter's specimen is still in the Dorpat Museum, and I would suggest a careful investigation of it.

A large number of cases have been published in modern times with the title of ovarian pregnancy and placed before us in the most reckless fashion. No less an authority than Hildebrandt, of Berlin, published a case as one of ovarian pregnancy in 1864, where a lot of old fœtal débris was discharged by the rectum and the patient recovered, not a scrap of evidence being given, or being suggested, as to where the pregnancy was, though the very fact that it was discharged by the rectum is conclusive that it rested in the broad ligament and originated as a pregnancy in the free part of the tube, and that it ruptured and passed into the extra-peritoneal variety. Another case to which the same criticism applies has much stress laid upon it by Dr. Parry on account of a post-mortem record which is perfectly satisfactory, except that there is no proof that a substance the "size of a honey bee which was found to escape on making the incision into the ovary, which was enlarged to the size of a very small hen's egg," was in reality a fœtus of the sixth or seventh week, as Dr. Parry believed. In fact, it is an endless task to go over the numerously recorded cases of this kind. Not one of them has been subjected to the necessary condition of criticism, a satisfactory compliance with which alone can establish the occurrence of ovarian pregnancy.

Parry (p. 38) says, "whatever doubts had previously existed, they were settled by Granville's description of an example of this form of aberrant gestation." But when the original description and figure are turned up it is found that Granville figures only a small cystic ovary with a gelatinous lining to the cyst, a very frequent object. There is no fœtus, not the faintest resemblance of one, but Sir Charles Clarke assured him that at one time there was an embryo hanging pendulous from the yet visible rudiment of an umbilical cord. In fact, Granville's case is of no more value than any of the others.

Parry sums up the subject by saying that the weight of

authority is in favor of the possibility of ovarian pregnancy. Its possibility I admit, because I can easily imagine a Fallopian tube glued on to the ovary and deprived of its lining epithelium, permitting the contact of the spermatozoa with a follicle burst within the area (of the ovary) of adhesion. Then the spermatozoa might infect the ovum before it escaped from the follicle, the ovum might adhere to the follicular wall and then develop. But there are so many contingencies in such a case that the doctrine of chances makes it so remote that its occurrence may be regarded as likely as the birth of a blue lion or a swan with two necks, like a heraldic monstrosity—a mere pathological curiosity. Finally, it would have no kind of clinical interest or importance not already decided upon in the case of pregnancies in the free part of the tube, so that we may avoid any further discussion of ovarian pregnancy as futile. If it does occur it must be rare and will be curious. If it never occurs so much the better.

I do not propose to discuss further the varieties of ectopic gestation which have been proposed by previous writers, for in doing so I should merely introduce elements of confusion which I am anxious to avoid. I shall merely say, therefore, that my second variety, the tubal pregnancy, arises from the attachment of the fertilized ovum to any part of the lining membrane of the tube, from the pavilion onward. There can be no doubt that the pavilion may itself become the seat of the gestation, and I am free to accept a sub-variety of the ovario-tubal as a possibility. It would occur amongst these numerous cases when the pavilion has become agglutinated to the surface of the ovary and has communicated by the bursting of a follicle under it, with the substance of the ovary itself. Tubo-ovarian cysts form in this way, and I have seen a large number of them. I have never seen anything like a pregnancy of this kind, however, and therefore while I admit the possibility of its occurrence I can say nothing about it. It must be clearly understood that such a variety, if accepted, would be different from the alleged ovarian form, for in this the condition would be that the ovum was developed in the ovary with the tube free and not attached to the ovary. That a fertilized ovum may drop into the cavity of the peritoneum and become developed there, is a contingency I cannot accept for a moment, for the powers of digestion of the peritoneum

are so extraordinary that an ovum, even if fertilized, could have no chance of development. What have been called abdominal pregnancies are clearly exceptional cases where primary tubal rupture at the end of the third month has not proved fatal, where the extruded placenta has made for itself visceral attachments wherever it has touched, or where secondary rupture of a broad ligament cyst has converted an extra-peritoneal ectopic gestation into one within the peritoneal cavity. That the first of these processes is by far the most common condition has been proved to me beyond doubt in my opera-

FIG. 4.—Actual View of Placental Villi (*c*) encroaching upon and causing thinning of the muscular wall (*a*) of the Fallopian tube; *b*, maternal blood sinuses. (Drawn by E. Teichelmann from section made by Berry Hart.)

tions, for I have seen the ruptured tube within a few days of the catastrophe containing the great bulk of the placenta, while the villi of the extruded portion have been engaged in making epiphytic inroads on intestine, bladder, the back of the uterus, and the folds of the omentum. I have pulled these villi out of the living crypts they have made, much as one pulls a barnacle out of its bed, leaving bleeding holes behind them. But Berry Hart has proved this beyond dispute, for he has been able to inject the placenta which had come out of its ruptured tube and acquired these strange and unusual associations, and I here give a drawing of one of the preparations showing the process.

From the relations of the outer extremity of the tube I should suppose that a tubo-ovarian pregnancy, or one in the pavilion, must of necessity always end by rupture into the peritoneal cavity, and I think it would do so within the period which limits the existence of the purely tubal ectopic gestations, that is thirteen or fourteen weeks. Clinically, therefore, there would be no advantage in multiplying by further subdivisions the varieties of tubal gestations. Whatever difference of opinion on these matters there may be, there can be but a uniform consensus of belief to this effect, that by far the greater number of cases of ectopic gestations are tubal. I believe that they are really all tubal, and in this consists the novelty of my views on the subject. I do not see any difficulty in believing even that a pregnancy originally tubal may be completely extruded from the tube, that the tube may contract and heal, and that a secondary and wholly intra-peritoneal gestation may thus be formed (Dezeimeris), as in the remarkable case recorded by Maticki ("Monats. für Geburtshülfe," Mai, 1860), where the uterus and its appendages could all be traced, and where the attachment of the placenta had become almost wholly omental. Having seen all stages of such a process as this would require, I am sure that this is possible, and the very rarity of the observation, unique so far as I know, proves how exceptional the completion of such a process must be, as we would naturally expect it would be. There is indeed nothing more remarkable about it than the well-established fact that by axial rotation an ovarian tumor may be twisted off its pedicle and grow entirely from the omentum or, as I have seen, from the ascending colon. Similarly I have seen this strange transplantation in all its phases and in all stages of the process. That such a hypothesis is tenable, is shown by the actual fact recorded by Lecluyse ("Bulletin de l'Academie de Belgique," 1869), that an intra-uterine gestation became ventral by the ovum escaping through an aperture left in the uterus by the defective healing of the wound of a previous Cæsarean section. The placenta became attached chiefly to the small intestines, and the history does not give any clue to a sudden rupture I think it far more consistent with the facts given, that the fistulous opening was gradually dilated during the early weeks of pregnancy, before the differentiation of the placenta as a cake, and

that the ovum was gradually passed through the opening, the placenta making epiphytic inroads on whatever it came in contact with.

Concerning the statistics of ectopic gestation, Parry very well says that with our present facilities for arriving at the truth in regard to the location of the ovum, it is believed that "we are not warranted, excepting in rare instances, in asserting that the ovum is developed in any particular portion of the genital canal, unless we have the opportunity of making a post-mortem examination." With this I entirely agree, and can only qualify his remarks by adding that we can accurately state the position of the ectopic gestation, when we remove the parts by abdominal section to avert the need of post-mortem examination. I have now been concerned directly and indirectly in the post-mortem examinations of twenty-six women who have died from hemorrhage into the peritoneum (intra-peritoneal hæmatocele) from ruptured ectopic gestation. I have had to operate forty times for the same cause, and I have witnessed about ten similar operations by other surgeons, making in all the unique experience of seventy-six cases. In every one of these the seat of the pregnancy was ascertained to be without doubt the Fallopian tube, and in only one was the seat of pregnancy in that part of the tube embraced by the uterine tissue. Interstitial ectopic pregnancy must therefore be very rare, and that form which occupies the free part of the tube must have an overwhelming preponderance, and the other alleged forms I have yet to see. All the cases which have occurred in my experience in which the gestation has gone beyond the period of primary rupture have been in the cavity of the broad ligament, where they were lodged by that rupture. These facts are so inconsistent with the laboriously (and I think uselessly) collected statistics of Parry that further research must be made before any conclusions are accepted. I do not see how Parry's statistics can be of any value for any purpose whatever, collected as they are for the most part from imperfect records made by men who were unskilled in pathological research.

Parry says, "It is very rarely indeed that an opportunity is obtained to examine an unruptured cyst in the early stages of its development." I doubt very much if such a case has ever occurred. Certainly the instances he quotes will not bear

critical investigation. Indeed, the best of the lot (Stanley's case) is clearly not accepted by Parry, for he emphasizes the fact that no embryo was found. I am of the opinion that no authentic description exists of an unruptured tube-pregnancy. Of the frequency of ruptured tubal gestations we require no more proof than the current literature of our profession, which abounds with instances, and there are few men of experience in general practice who cannot call to mind one or more examples of this ghastly catastrophe. Parry says, that "The almost universal opinion of the profession is that this accident is uniformly fatal, and if not so, that we have no reliable means of combating its dangers." Much discussion has taken place of late years as to the possibility of diagnosing tubal pregnancy before the period of rupture, and many strangely dogmatic assertions have been made to the effect that such cases have been diagnosed and successfully treated. I am bound to say that I am exceedingly sceptical concerning the correctness of these statements, and one fact alone would justify my attitude. It is this, that of all the cases that I have operated on, and in many where I have seen the post-mortem examination and have known the history, the patients have made no complaints till the alarming symptoms of rupture have set in. I have only seen one case before the period of rupture, and there I diagnosed tubal occlusion and distention easily enough; but the question of the woman being pregnant never entered the mind of any one who saw her, and for reasons which will be plain when the story is read. See "The British Gynæcological Journal," Part XIII., p. 38, from which the following is an extract:

"The woman came to me a few weeks ago in the ordinary course of out-patient practice, with symptoms of obscure pelvic pain of several months' standing—in short with the usual symptoms of tubal disease. She was examined and I came to the conclusion that it was a case of gonorrhœal salpingitis, and so clear were the symptoms that I used the case to demonstrate to my pupil, Dr. Ricketts, the nature of the symptoms in that disease. That was on a Monday. On the Thursday she turned up again with the most acute symptoms —she was bent double and could hardly walk. Finding that the whole floor of the pelvis was fixed in one mass she was at once admitted. The next morning I opened the abdomen and

found a ruptured tubal pregnancy, than which nothing was less suspected. I defy anybody to have diagnosed such a case beforehand, for the woman had not even missed a period."

The fact is that the notions of Antoine Petit of 1710 still permeate the professional mind, and in spite of all that can be said they are handed down from text-book to text-book with unfailing regularity and uniform inaccuracy. Of these misleading statements Parry said, "Could they be verified, the detection of extra-uterine gestation would be an easy task; but unfortunately for the comfort of the obstetric surgeon scarcely one of them contains a grain of truth; yet strange to say the opinions of Petit influenced and impeded the progress of our knowledge on this subject for more than half a century." Parry might have said for a century and three-quarters.

The curious thing is that the great bulk of my patients had no suspicion that they were pregnant at all, and therefore the first factor in a correct diagnosis was absent. Even when this leading point is present, there is generally nothing unusual about the sensations of the patient till the period of danger. As Parry well says, "The patient in the first instance supposes herself to be pregnant, and during the first four or five or eight weeks nothing particular occurs to warn her of her anomalous condition. The usual signs of this early period of gestation appear successively; or, indeed, she may enjoy better health than she did during the same period of previous pregnancies, when suddenly and without any warning the unfortunate victim of this terrible accident is seized with very characteristic symptoms."

But the very fact to which I have drawn attention, that a very large proportion of these victims, a large majority in my own experience, are women who either have never been mothers or who have not been pregnant for many years, shows how misleading the whole history may be. The last thing these women would admit would be pregnancy.

I must point out here that Petit is right on one point to a very large extent, though by no means uniformly, when he says that "the menses, contrary to what is seen in normal gestation, continue to appear, but in smaller quantities throughout the pregnancy." Menstruation is sometimes suspended absolutely, as in normal pregnancy, but more usually

it occurs irregularly and profusely, so that here again we are misled. In fact, the history of these cases is more usually a source of danger than a help to the diagnosis, and unless some exceptional incident occurs, or unless the patient is a good deal more anxious about the state of her pelvis and a good deal less reluctant to have it examined into than Englishwomen are as a rule, no diagnosis is possible before the period of rupture, for the patients make no demand upon us. Among the women of other countries it may be different. I cannot improve on the words of Parry in continuing this vexed question of early diagnosis of tubal pregnancy, and therefore I quote further: "An extra-uterine gestation is frequently ushered in quietly enough, and during the first four or six weeks all may go well, but after this time symptoms supervene which in their violence are as unlike the signs of uterine pregnancy as the surface of a stormy sea is unlike that of a dead calm. The one moves on with some sort of regularity, the discomforts of the condition appearing in a certain order, but the other follows no plan and sets all order at defiance. This is the period of rupture which is (in my own experience) limited between the fourth and the twelfth week of pregnancy." I possess, and have frequently exhibited, a preparation of a ruptured tubal pregnancy which proved fatal in a woman aged thirty-one after an illness of only seven and half hours. She was under the care of Dr. Guthrie Rankin, of Warwick, and Dr. Thursfield, of Leamington, and the following is the history of the case: "On November 2d, 1887, at 1:30 P.M. Mrs. ——— was seized with pain in the abdomen, followed by vomiting and faintness. Dr. Guthrie Rankin was called in, the pain was relieved by an opiate; but collapse followed, and death ensued at 9 o'clock the same evening. She was seen in consultation by Dr. Thursfield just before death. She was the mother of three children, suckling the youngest, aged seven months, of good constitution, with no history of previous illness. At the post-mortem examination the abdomen was found full of clots, estimated at from seventy to eighty ounces. The left Fallopian tube presented an ovoid swelling which had ruptured, and was full of blood clot; on examination this swelling proved to be a tubal pregnancy.

One curious point about the preparation is that the ruptured ovum in the tube, as it is seen in the preparation bottle,

looks exactly like the ovary and every one who sees it at once says, "Case of ruptured ovarian pregnancy." But a little more careful examination displays the ovary uninjured, and the further fact that what is taken at first to be the ovary really is an ovum in the Fallopian tube of certainly not more than five weeks. The rupture which caused death was not larger than a pea. I mention these facts to show how carefully records of these cases must be made.

On the other hand I have seen no case of ruptured tubal pregnancy (primary rupture) either in my own practice or in museums in which there was evidence to show that it was over the twelfth week. Of course I am not talking of cases where the pregnancy had gone on in the broad ligament after primary rupture into that cavity, but purely of those of fatal primary rupture requiring operation for the arrest of hemorrhage.

The cause of the primary rupture of the tube is chiefly in its thinning at the site of the placenta. When distended either by pregnancy or otherwise, the walls of the tube never thicken materially. Certainly in tubal pregnancy there is no imitation of the thickening of the muscular coats of the uterus. The villi of the placenta permeate the walls, seem even completely to penetrate them, and the blood-vessels increase enormously in size, especially the veins. Some slight exertion occurs, such as stooping at some household work, a violent attack of pain comes on, the patient becomes faint, collapsed, cold, pulseless, and anæmic, and dies almost uniformly if unaided. This is the story of a great number of these cases, for quite a number of cases in which I have seen post-mortem examinations, the women have been found dead or dying, and suspicions of foul play have not unfrequently been aroused. Sometimes these symptoms abate, the patient recovers for a few days and even gets about, then a recurrence of the peritoneal hemorrhage occasions a revival of the serious symptoms, and this may be repeated at intervals several times before the fatal issue is arrived at. A most notable example it was of this which drove me to attempt to save these cases by prompt surgical interference; it was indeed an epoch-making case, for it has revolutionized our practice in these cases.

In the summer of 1881, I was asked by Mr. Hallwright to see with him in consultation a patient who had arrived by

train from London in a condition of serious illness, that illness having been diagnosed by Mr. Hallwright as probably hemorrhage into the peritoneal cavity from a ruptured tubal pregnancy. The patient was blanched and collapsed, the uterus was fixed by a doughy mass in the pelvis, and there was clearly

FATAL CASE OF FALLOPIAN PREGNANCY AT EIGHTH WEEK. (AFTER DUGUET.)

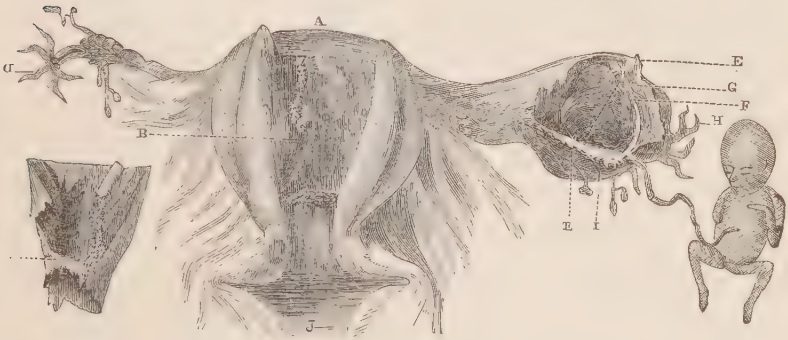


FIG. 5.—*A*, Uterus laid open on the anterior surface; *B*, part of the *decidua* still adherent to the right uterine cornu; *C*, decidua, nearly entire, expelled before death; *D*, right tube and ovary, normal; *E*, *E*, margins of artificial opening in left tube; *F*, umbilical cord; *G*, placenta; *H*, pavilion of the left tube; *I*, vascular plexus, ramifying over the tubal covering of cyst, from which the hemorrhage occurs on its rupture; *J*, vagina.

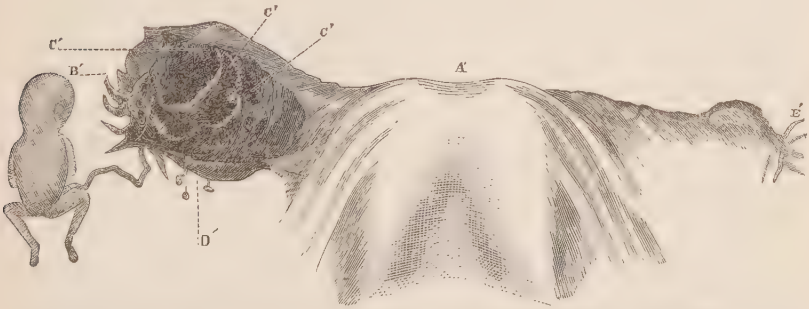


FIG. 6.—*A*, View of the posterior surface of the uterus; *B*, fimbriæ of left tube; *C*, *C*, *C*, rents in tubal covering of cyst, corresponding to site of placenta, from which the fœtus escaped and hemorrhage came; *D*, ovary attached to lower surface of cyst, and increased in size; *E*, right tube.

a considerable amount of effusion in the peritoneum, but no distinct tumor could be felt above, and I agreed with Mr. Hallwright as to the nature of the lesion. This gentleman made the bold suggestion that I should open the abdomen and remove the ruptured tube. The suggestion staggered me, and I am ashamed to have to say I did not receive it favorably.

I saw the patient again in consultation with Mr. Hallwright and Dr. James Johnson and again I declined to act upon Mr. Hallwright's request, and a further hemorrhage killed the patient. A post-mortem examination revealed the perfect accuracy of the diagnosis. I carefully injected the specimen, which was removed, and I found that if I had tied the broad ligament and removed the ruptured tube I should have completely arrested the hemorrhage, and I now believe that had I done this the patient's life would have been saved. The appearances in this case are precisely given in the annexed illustrations from Duguet.

A most striking contrast to this terrible incident will be found in the following case, when, thanks to the ability and firmness of Dr. Dolan, of Halifax, I was able to save a valuable life:

Late on the evening of February 16th I received a telegram from Dr. Dolan, of Halifax, to proceed at once to that town to operate upon a case which he believed to be one of ruptured tubal pregnancy. The following is the account which Dr. Dolan has given me: "P. W., aged twenty-nine, married, four children living, youngest two years old, had a miscarriage nine months ago, has always been regular but missed the last period. Felt uneasy for the last few weeks; felt, she said, as if there was a weight and as if the womb were coming down the passage; enjoyed good health up to this time. About 9:30 A.M., February 11th, I was called to see her and found her in a state of collapse. She revived and then complained of pain in her abdomen. Symptoms like those of colic, vomiting, abdomen distended, great deal of flatus. This continued for some hours; was relieved by ether and champagne. She had several attacks during the day, and I saw her altogether seven times. At 10 P.M. same night she begged for something to give her sleep, and I gave her a dose of chloral, bromide of potash, and camphor water. She slept the whole night. A nurse had been obtained immediately after the first attack. On the morning of February 12th she was, to all appearances, perfectly well, was free from pain, and, as she said, she felt as if there was nothing the matter with her. There was, however, a good deal of flatus, and the abdomen was distended. She told me she had gone to bed on the Tuesday night perfectly well, but on rising in the morning about 7 A.M., she felt a

sudden pain about the umbilicus. When she got up she tried to work it off. I told her husband I feared there was some internal hemorrhage caused by rupture of tube, but as she was so much better I would wait and see whether I was right. She was kept in bed in charge of the nurse, and not allowed to move. This treatment was continued until the following Thursday. There was no return of pain or collapse, and she said she did not know why she was kept in bed. At midnight I was hurriedly summoned to see her. She had been out of bed for a short time and almost the same symptoms came on —sickness, tendency to faint, cold sweats, with a sense of fulness in the abdomen. Her appearance was changed, face was blanched, the abdomen was distended, but there was no localized swelling. By resting she again revived. I told her husband that I was now certain as to what she was suffering from and an operation would be required. He gave me permission to call in Mr. Lawson Tait, whom I telegraphed for as soon as I could." On my arrival I completely agreed with Dr. Dolan's diagnosis, and I opened the abdomen without further delay and removed an enormous quantity of clots and bloody serum and débris. The tubal pregnancy was on the left side. I tied the broad ligament, removed the pregnancy, washed her out thoroughly, and put in a drainage-tube. Dr. Dolan stayed with her all night feeding from time to time with diluted champagne. She gradually rallied, there was but a slight discharge from the tube, very little pain, the pulse came down day by day, and on February 25th she was regarded as convalescent and is now in a condition of perfect health.

What a contrast lies in such a pair of cases! And to make the lesson still more emphatic let me make a long quotation again from Parry's book (pp. 211–13). "In speaking of the result of this pitiless termination of extra-uterine gestation, it was stated that so few recovered from it, that all hope of such a happy result is to be dismissed in considering the treatment. No doubt, notwithstanding the statement of Rogers to the contrary, a few women have recovered, though the number is very small—so small that when one is called to a case of the kind, it is his duty to look upon his unhappy patient as inevitably doomed to die, unless he can by some active measures wrest her from the grave already yawning before her."

"A bleeding vessel through which the red stream of life is

rushing away, can be ligatured. A gangrenous limb which is destroying the possessor by sending its poisonous emanations to the remotest regions of the body can be amputated. A cancerous breast, which is sapping the vitality of its victim hour by hour, can be removed with the prospect of temporary relief. An aneurism that places life in constant jeopardy, can often be cured by proximal or distal ligation. The tumultuous action of the heart organically diseased may be quieted till nature restores the balance, after which the person may enjoy a long and even a useful life. Even phthisis now counts its many cures; but here is an accident which may happen to any wife in the most useful period of her existence, which good authorities have said is never cured; and for which even in this age, when science and art boast of such high attainments, no remedy, either medical or surgical, has been tried with a single success. From the middle of the eleventh century, when Albucasis described the first known case of extra-uterine pregnancy, men have doubtless watched the life ebb rapidy from the pale victim of this accident as the torrent of blood is poured into the abdominal cavity, but have never raised a hand to help her. Surely this is an anomaly, and it has no parallel in the whole history of human injuries. The fact seems incredible, for if one life is saved by active interference it may be triumphantly pointed to as the first and only instance of the kind on record. In the whole domain of surgery—for we cannot look to other than surgical measures under the circumstances—there is now left no field like this. In this accident, if in any, there is certain death. How often do we see persons recover from injuries which their surgeons tell them will be mortal, if they do not submit to a grave and terrible operation, and which with a dogged determination they refuse to have performed, preferring to perish rather than to suffer such grave bodily mutilation; or else, with a keener instinct, they foresee a happier result and get well notwithstanding the evil prognostications of the surgeon and in defiance of all the laws which, as man with his fallible knowledge supposes, govern human injuries. But in rupture of an extra-uterine fœtal sac, in the early stages of pregnancy, a whole lifetime—a whole century—is not enough to enable one person to make two errors in regard to the prognosis of this accident."

"The only remedy that can be proposed to rescue a woman

under these unfortunate circumstances is gastrotomy—to open the abdomen, tie the bleeding vessels, or to remove the sac entire. The first suggestion of performing gastrotomy to save a woman dying from early rupture of the cyst, came, so far as we know, from our countryman Dr. Harbert, while to Rogers belongs the credit of formulating the arguments in favor of this practice and bringing them prominently before the profession. Since he wrote the same plan of treatment has been advocated by Meadows, Hewitt, and Greenhalgh in a discussion before the Obstetrical Society of London. Koeberle, Behier, Schroeder, and Atlee countenance the proceeding, but no person has yet performed gastrotomy for the relief of this accident. *The great impediment to the adoption of this treatment is the uncertainty of diagnosis.*"

Mark the importance of the last sentence, which I have italicized, for this sentence it is, reiterated by almost every writer on abdominal surgery up to 1878, and continued as a tendency by a great many still, which has stood in the way of our success. I have long since thrown it to the winds, and when I find my patient "in danger of death from conditions within the abdomen which do not seem to be clearly of a malignant nature, but a correct diagnosis of which is impossible, I open the abdomen and at once make the diagnosis certain and a successful treatment possible."

This is the rule I laid down in 1878, adding to it, for other purposes, that I did the same thing when "the conditions were such that the patient's life was miserable by reason of suffering which could not be relieved, or at least had not been—by all other measures." The result has been an enormous advance in abdominal surgery, obtained only after a severe struggle with the authority of the elders, who asserted that the abdomen was a region into which the writs of ordinary surgical laws should not run.

This principle of exploration is nothing new, in fact the way it is sometimes used or rather abused is almost horrible. I once saw a surgeon, who is now a baronet and has a Court appointment, remove a breast with a tumor in it. After he had the whole thing away in his hands, he drew his knife across the tumor and out spurted a lot of pus, "laudable pus." He had made his exploration after the treatment was complete. If he had explored first his diagnosis would have been completed,

his blunder saved and the radical and exaggerated treatment rendered wholly unnecessary. I have similarly seen a limb amputated for a sequestrum opening into the knee joint, which a preliminary exploration would have shown to be capable of removal without amputation and the limb would have been saved. Crowds of illustrations of this kind of theory could be given; showing, in the first place, that complete accuracy of diagnosis is no more possible in the breast than it is in the abdomen, that exploration is a sound principle when there is doubt, and that many ghastly blunders would be saved if the practice were extended into general surgery. Absolute accuracy of diagnosis in the abdomen is very far from being possible; only the ignorant assert that it is, and only fools wait for it.

After the terrible lesson given to me by Mr. Hallwright's case, I did not see another example of ruptured tubal pregnancy, or one which I suspected to be of that nature till I was called to Wolverhampton by Mr. Spackman, on June 17th, 1883. There could be no doubt as to the nature of the case, and Mr. Spackman was fully aware of it before I was summoned. The patient was clearly dying of a hemorrhage, and I at once advised abdominal section. The fœtus, about the twelfth week, was lying among masses of clot and coils of intestine and to these latter the partially extruded placenta had obtained new attachments. These I cautiously separated and occasioned fast and copious bleeding at every point. I wasted much time in trying to stop this hemorrhage, so that by the time the operation was finished my patient was practically dead. We got her to bed alive, and that is all that can be said. I thought much about this case, for it was a bitter disappointment; I thought I should achieve a triumph and I had only a failure. But my conclusion was speedily arrived at that I had blundered, that the true method of operating in such a case was to separate adhesions rapidly, regardless of bleeding, and make at once for the source of the hemorrhage, the broad ligament, tie it at its base, and then remove the ovum débris and clots at leisure. This I have done now in thirty-nine cases, with one death, and I think I may fairly say that I have really achieved a surgical triumph. My example has been widely followed, and the success is almost uniform.

The diagnosis of tubal pregnancy before rupture of the tube

is not easy, as I have said, because the patients do not claim our attention. What symptoms there are, as in the solitary case where I had a chance of making a diagnosis, are merely those of tubal occlusion and distention—matters very easy to diagnose and to treat. If I ever should make a diagnosis of tubal pregnancy before rupture I should advise its immediate removal by abdominal section as being more certain and far more safe than the fancy methods of puncturing the cyst and injecting poisonous fluids or passing through it some kind of galvanic current. There can be, there clearly is, from the statements of those who have tried these plans, neither certainty nor safety about them; and they will commend themselves only to such as, by lack of courage and skill to obtain good results, have only bad records to show in abdominal section.

The diagnosis of tubal pregnancy at the time of rupture may be made with certainty seven times out of eight, and may be guessed at in the eighth instance. They are too serious to be lightly regarded at any time, and are practically coincident with those of pelvic hæmatocele. If the rupture takes place into the broad ligament they are the symptoms of extra-peritoneal hæmatocele. If the rupture takes place into the peritoneal cavity they are the characteristic and most serious group which belong to intra-peritoneal hæmatocele.

No more appropriate place than this occurs to me to discuss this much-confused question, if for no other reason than that I have never seen an intra-peritoneal hæmatocele that was not due to a ruptured tubal pregnancy; and very many cases of extra-peritoneal hæmatocele (effusions of blood into the broad ligament) have undoubtedly been tubal pregnancies which have ruptured between the peritoneal folds of that important structure. The difference between them is all important in every way, for the intra-peritoneal ruptures seem to be almost uniformly fatal, while the extra-peritoneal hæmatoceles, whether arising from tubal pregnancies or not, should certainly be left to take their own course, unless they give signs that they are suppurating.

A most especial interest was given to this question by a trial which took place at Liverpool some two years ago, which raised the whole question, and displayed the extraordinary confusion which existed then in the professional mind upon it.

The first important contribution to the literature of pelvic

or abdominal hæmatocele was the work of Bernutz and Goupil, translated by Dr. Alfred Meadows, and published by the New Sydenham Society in its English form in 1866. It appears to me a matter of great regret that the writers of our text-books on gynecology have so neglected this admirable work, most of them seem never to have read it at all, and in those where it is quoted it is clear that nearly every one of the writers has failed to understand the meaning of the French author. An instance, by no means remarkable as an exception, may be found in one of the most recent text-books on gynecology—that by Dr. Emmet, of New York, and we see that throughout his chapter on this subject this confusion is remarkably prevalent. The chapter begins with the definition that hæmatocele is an "accidental collection of blood in the pelvis, either in the peritoneal cavity, or outside the peritoneum, or within the connective tissue of the pelvis." This definition is faulty to begin with, because the second and third varieties must of course be classed together, and to class under the same name—the common name of hæmatocele—two conditions which must be so absolutely apart as hemorrhage within the peritoneal cavity and hemorrhage outside it, is the very fountain and origin of all the confusion which has arisen. Dr. Emmet quotes Nélaton as having given the first accurate description of the pathology of the lesion; but in reality Nélaton's views, from the very words he coined to express them, are very largely answerable for the confusion. M. Nélaton regarded the origin of hæmatocele as being from the rupture of a Graafian follicle, the bleeding naturally gravitating from the surface of the ovary to the bottom of Douglas's cul-de-sac, the most dependent point, and for this the term he invented was "retro-uterine hæmatocele." On page 228, Dr. Emmet gives a diagram labelled a "retro-uterine hæmatocele" in which the section of the blood-clot is clearly enough placed behind the uterus, but a glance at it will show that such clot never could possibly arise from an ovary, so that either Dr. Emmet is wrong in his notions of the pathology, or he has altogether misunderstood M. Nélaton. On page 231 he gives a diagram which is really the diagram intended by Nélaton, but in which the blood-clot is peri-uterine, and therefore the case in the second instance comes under the definition and title which we owe to Simpson.

Between the appearance of the first real essay on the subject by Bernutz in 1848, and the translation of his larger work in 1866, a great many contributions to the literature of this subject were made, each of which advanced some peculiar theory on the subject, and to all of these there may be urged the objection that they were too exclusive, and they did not in any instance, as it appears to me, grasp the whole pith of the case. The word "hæmatocele" is a convenient though not very accurate term, and so long as it is limited to the idea of an effusion of blood it may be taken as the basis of our consideration. The moment, however, such terms as true hæmatocele and false hæmatocele were introduced confusion reigned supreme. Bernutz held that true hæmatocele consisted of an effusion of blood within the peritoneal cavity, while Simpson argued that it could never be an intra-peritoneal effusion. What I am inclined to advise, and for reasons that will be given immediately, is that the phrase "pelvic hæmatocele" ought to be retained to cover all effusions of blood which have their origin in the pelvis. This I advise because it would cover the vast majority of cases of effusion of blood into the peritoneal cavity; for if we exclude the results of traumatic lesion, there are very few effusions of blood into the peritoneal cavity which have not a pelvic origin—a fact which is at once indicated by the extreme rarity of the occurrence in men. With this simplification we can look over the great bulk of the literature on this subject with a certainty of arriving at more logical conclusions than if we did not accept the limitation. Bernutz and all other writers agree in recognizing the fact that any effusion of blood must be regarded rather as a symptom than a disease of itself, and this is true enough if we are discussing merely the etiology of the condition; but if we regard the condition in itself as an entity we certainly cannot accept this as a limitation, for whatever the origin of the effusion may be the moment the effusion is in existence it becomes in itself a disease, and sometimes an extremely severe one. But in the two great classes into which I am about to divide hæmatocele this is far more true about the first class than it is about the second, and this is the first indication that we get of the differences which are found to exist between the two classes. This difference is created *ab initio* by differences in the anatomical relations of the effusion.

In the pelvis—indeed we may take the whole abdomen and say in the abdomen—an effusion of blood must be either within the peritoneum or outside it. And let us just speak for a moment on what the primary, what the initial result is in any effusion of blood under these two different circumstances. Let us take an imaginary case of rupture of a blood-vessel in the neighborhood of the kidney by reason of a blow or other injury. Effusion of blood in that neighborhood must of necessity be extra-peritoneal; it would travel through the cellular tissue, and by reason of the very fact it had so to travel, provided there was no rupture of a vessel into the pelvis of the kidney, the effusion would be limited, the interstices of the cellular tissue would form the very best of all known hæmostatics, and I find it difficult to imagine that an effusion of blood—let us call it a renal hæmatocele—in the neighborhood of the kidney could be so extensive as to be fatal, always supposing it was not a main trunk which had been wounded. But, on the other hand, if we imagine from some cause or other a blood-vessel of the kidney bursting into the cavity of the peritoneum, there would be no natural hæmostatic to assist it in the arrest of the hemorrhage; the bleeding would go on indefinitely, and unless some means could be secured to assist nature in arresting it, the patient would almost with certainty die.

Let us now take another illustration. Suppose that a small vein on the posterior peritoneal surface of the uterus were to rupture and to bleed into the peritoneal cavity. There the blood would of course naturally tend to coagulate, but not in the same way as when extravasated into the connective tissue. All of us who have experience in abdominal surgery know that when blood flows in quantity into the peritoneal cavity, probably by reason of its dilution by the lymph always present there, and easily excited into excessive flow by any abnormal condition, it does not show much tendency to coagulate, save in a very fitful and fragmentary way. One of the most remarkable proofs of this is the influence of the drainage-tube in arresting hemorrhage. If the cavity is kept dry by frequent withdrawal of blood and serum oozing from torn pelvic adhesions, the bleeding will soon stop; but if drainage is not kept up the bleeding will probably prove fatal.

Supposing, on the other hand, that a small vein should

rupture in the tissue and between the folds of the broad ligament, we should again have exactly the same condition as I have imagined to occur about the kidney; in fact it would be still more marked, for, in the first place, the cellular tissue through which the bleeding could permeate is much more limited in quantity than it is in the neighborhood of the kidney. Again, we have a space between the folds of the broad ligament which is not capable of rapid distention to an indefinite extent. The broad ligament when distended forms a limited cavity, and we shall then have two processes by which the tendency to excessive hemorrhage is arrested; the first is the natural tendency on the part of the interstices of the broad ligament to limit the bleeding; and again the pressure of the broad ligament itself, as a membrane distended and resisting further distention, exercises pressure upon the bleeding point and becomes a powerful natural hæmostatic.

These anatomical considerations alone, were they supported by no other facts at all, would be enough to persuade us into an acceptance of the division, which has often been described but never precisely laid down by writers of this subject, of pelvic hæmatocele into the two classes of extra-peritoneal and intra-peritoneal; the former corrected and checked by two powerful agencies which are absent in the latter, while the hemorrhage in intra-peritoneal hæmatocele is actually favored by the dilution of the blood as it passes out of the bleeding vessels. The confusion which has arisen from a want of the recognition of the two classes of cases of intra- and extra-peritoneal, may be seen by taking up any text-books on the subject, and turning to the allusions which are made as to the frequency of the occurrence of hæmatocele or to its differential diagnosis and still more to the treatment. Thus, Dr. Emmet says: "If we limit the acceptation of the term 'hæmatocele' to an accumulation of blood passing into the peritoneal cavity, the accident is comparatively a rare one; but if it is held to embrace all blood accumulations in the pelvis, the occurrence is certainly a far more common one than the profession at large have any conception." If we accept the first sentence of this passage as alluding to intra-peritoneal hæmatocele the statement is relatively correct; and if we accept the second sentence as referring to extra-peritoneal hæmatocele the statement is absolutely exact; but if we go a few more pages

further on in Dr. Emmet's book we find him attempting to make a differential diagnosis between hæmatocele—of which he has given no precise definition either for extra- or intra-peritoneal effusion—and tubal pregnancy, and the confusion becomes positively amazing; for it will be seen as we proceed that for intra-peritoneal hæmatocele by far the most common cause is the tubal pregnancy for which Dr. Emmet desires to find a differential diagnosis. This is what he says upon treatment: "Surgical interference has been advocated by many in its practice, and been urged as the necessary procedure at an early stage. Unquestionably cases must occur when the surgeon would be wanting in sense of duty if he did not assume the responsibility and puncture the mass. But with a large majority of cases such interference would be criminal, as it needlessly places the life of the patient in jeopardy." Here, again, what Dr. Emmet says is absolutely true about extra-peritoneal hæmatocele, and it is absolutely untrue about intra-peritoneal effusion. I only desire to say that I have taken up Dr. Emmet as an example of this confusion in English writings, not because he is worse than others, but merely because his work happened to be the first text-book on gynecology which caught my eye as I started to write this lecture. If we accept the anatomical and physical facts before alluded to as a basis, we shall find that it is not a difficult matter to reconcile a very large number of discordant facts and many discrepancies in the views of various authorities; in fact, the whole story of hæmatocele may be reduced from confusion into order. We shall find also that the two varieties of hæmatocele are different in their relative frequency, in their causation, in their history, and of course particularly in their relative fatality, different in their symptoms and the signs by which they may be diagnosed, and, finally, in their demand for surgical interference.

Dr. Bernutz has expressed an opinion to which I have already alluded, "that the bloody tumor which is left as the remains of a hemorrhage has no right to be regarded as a specific disease apart from what has caused it." This is true, I hold, of intra-peritoneal hæmatocele, but not of the extra-peritoneal variety. When hemorrhage into the broad ligament occurs, the arrest of the hemorrhage has already been brought about, in the vast majority of cases, by Nature's own

methods, probably even before the accident has been diagnosed, and therefore all we have to do with is the thrombus, and in the great bulk of cases that may be and generally is let alone. But cases do arise, as I shall tell you by-and-by, when it becomes a serious disease, for if the sac of the broad ligament bursts into the peritoneal cavity the hæmostatic pessure is relieved and bleeding goes on, the two forms of the lesion co-exist, and the patient bleeds to death. That such an ending may occur and has actually occurred is known by a case I shall quote as a result of this secondary rupture of the broad ligament pregnancy cyst, the primary rupture having taken place at the ordinary period and the direction of rupture being into the cavity of the broad ligament. The secondary rupture takes place into the cavity of the peritoneum and proves fatal. I have seen no such case, but more than one is faithfully recorded by Bernutz, and such a case is recorded by Goupil, and is a perfect example of what I can fully believe to be possible, though I have not seen it, therefore I quote it at length.

S——, aged thirty-two, from a delay in menstruation thought herself pregnant, and regarded a metrorrhagia which occurred, as an abortion, though she had seen no trace of an ovum. On admission, the abdomen was distended, and very tender on pressure, and it was resonant on percussion. The cervix was open and the uterus was pushed somewhat to the left and forward by an enormous swelling which was behind it. The posterior cul-de-sac was occupied by a fluctuating tumor which was felt filling up the pelvis entirely and the fluctuation was very distinct. The diagnosis was (and I regard it as one of the most brilliant on record) intra- and extra-peritoneal blood tumor, probably accompanied by extra-uterine gestation. She gradually grew worse and died on the third day after admission. She died, because in 1855 M. Nonat, under whose care she was, had not been infected by the "restless spirit of surgery let loose" since 1878, which has done so much to save cases such as this.

The post-mortem record of this case, however, is a perfectly peculiar record of facts. In the peritoneal cavity about twenty-five ounces of black fluid blood and clot were found constituting the intra-peritoneal hæmatocele, the cause of the patient's death. When that was removed an ovoid tumor was

observed covered by the peritoneum of the broad ligament. It seemed to be formed by a mass of blood. This was the extra-peritoneal hæmatocele. At the bottom of the left recto-uterine cul-de-sac the peritoneum forming the posterior layer of the broad ligament presented a perforation with a communication between the recto-vaginal cul-de-sac and the cellular tissue separating the peritoneal layers of the left broad ligament. On making an incision into the ovoid tumor a small fœtus was discovered.

The importance of this record cannot be overestimated, for it proves, as I shall show afterward, how some cases of broad ligament hæmatocele arise. It shows that broad ligament hæmatocele may occasionally be fatal by becoming intra-peritoneal hæmatocele, and it shows us (this case has shown me) how thirty-eight out of forty of such cases may be saved from death. Further, it proves what is perhaps not very pertinent to the present discussion, that the views I have advanced about the tubal origin of all extra-uterine pregnancies are correct. The only other case of this kind familiar to me is one very imperfectly narrated by Duverney, as having occurred in 1712. These two cases are all the records I have found of the coincidence of intra- and extra-peritoneal hæmatocele, and the combination was due in both instances to rupture of a broad ligament pregnancy with hemorrhage into the peritoneum. Both cases ended fatally by reason of the hemorrhage into the peritoneum. I have seen dozens of cases of broad ligament hæmatocele and have never met with a fatal one. I have seen nearly eighty cases of intra-peritoneal hæmatocele all resulting in death save those (with two exceptions) in which abdominal section was performed for the purpose of obviating death, so that we find a very wide difference in the results of the two classes of cases in my experience. It will also be found that when the real difference between the varieties is understood, it will explain all the discrepancies in the views held by various authors and all the confusion will cease.

I propose to deal first of all with the extra-peritoneal hæmatocele and to give first in detail two cases which prove in every way its character and relations, and which illustrate also two of the exceptional instances in which it requires to be interfered with.

C. T., aged twenty-six, was placed under my care in December, 1883, by Dr. Faussett, of Tamworth, on account of a large parovarian tumor. I operated on January 3d, 1884, and removed a cystoma of the left ovary weighing fourteen pounds. There were no adhesions, the pedicle was long and thin, and the operation was as easy as possible. A metrostaxis appeared about twenty-four hours after the operation, as is usual after such operations, the only peculiarity in this instance being that the loss was very abundant. It suddenly ceased after being present for about twelve hours and immediately the patient was in great pain. From having seen the same accident under similar circumstances very frequently, I knew at once what had happened. I examined and found, as I suspected, a large hæmatocele of the left broad ligament. The hæmatocele increased slowly in size until a tumor could be felt above the brim of the pelvis, and the patient suffered greatly. I also found that the rectum was completely blocked, as I had seen it often before, by a stricture caused by the effused blood dissecting round the rectum outside the peritoneum. This is one of the signs of broad-ligament hæmatocele which has not yet been noted by any writer with whose work I am acquainted, and it is of great importance. It does not—indeed it cannot—occur in an intra-peritoneal effusion. In the case of C. T., I tapped the hæmatocele from the vagina and drew off a large quantity of tarry blood, but in fourteen or fifteen hours the sac had filled again, and the patient had become exsanguine. I therefore re-opened the abdomen, opened the distended cavity of the broad ligament, emptied out the blood fluid and clots, sponged it out with vinegar and water, fastened the edges in the aperture to the edges of the parietal wound, and placed in a drainage-tube. The patient then made a rapid recovery. I only wish to emphasize the fact that this second operation made it absolutely certain that the effusion was in the cavity of the broad ligament, and not in the peritoneal cavity, for the peritoneal covering of the blood mass was perfectly free from adhesions, and its entire relation could be made out with the greatest ease, quite as easy as if it had been a post-mortem instead of an ante-mortem examination.

The second case was one in which a tubal pregnancy had ruptured into the broad ligament, and the subsequent effusion of blood was so great that it caused complete obstruction

of the rectum by annular constriction. The diagnosis of broad ligament hæmatocele was easy enough, but I did not diagnose its cause, for the patient had never missed a period. She had been married four years and had never been pregnant. Suddenly she experienced violent pelvic pain while engaged in some social amusement and the symptoms so rapidly advanced that in about four hours she was collapsed. I was called to her and found a large, ovoid, well-defined and quite firm tumor above the brim of the pelvis, the roof quite fixed by an effusion which followed the archings of the pelvic fascia and completely blocked the rectum. It was so firm that I decided not to tap it but to open the abdomen, and it was well I did so. Next morning I carried out my proposal and removed from the cavity of the broad ligament about two pounds of blood-clot and a fœtus and placenta of about the ninth week. I sponged out the cavity with vinegar and water, stitched the opening in the broad ligament to the opening in the abdominal wall, as in the operation for pelvic abscess, and the patient made a rapid recovery.

This case proves that tubal pregnancies rupturing into the broad ligament (*c*, in the scheme) may occasionally require interference at the time of primary rupture.

Speaking of a series of cases by Nonat, M. Bernutz says very quaintly that there should have been at least one necropsy in order to demonstrate the legitimacy of the different diagnoses—that is, between what he calls true (intra-peritoneal) and spurious (extra-peritoneal) hæmatocele. But these five cases recorded by M. Nonat are precisely in point in the present discussion, for four of them were diagnosed by that eminent gynæcologist as being the subjects of extra-peritoneal effusion, and they all recovered, while the patient in whom he dignosed intra-peritoneal hæmatocele died, and M. Bernutz was gratified by an accurate post-mortem record. Nothing could prove more satisfactorily than this, what I contend for, that it is the anatomical relations of the two kinds of hemorrhage which make all their differences, and now we examine them carefully before death, and by that very examination prevent death by curing them, we get the first corroboration of all that M. Bernutz has said.

Of extra-peritoneal hæmatocele there are only two causes known to me, one very common, and one relatively rare. The

first is sudden arrest of metrostaxis, which may either be normal menstruation or the pseudo-menstruation which occurs so constantly after abdominal operations. The first case I have just detailed is a typical example of what occurs very often after any operation on the broad ligament, and to the inexperienced surgeon is a fertile source of worry. The accident is always indicated by the sudden access of pain, and often an alarming feeling of faintness. The pulse always rises, and sometimes the temperature does so too. On examination the uterus will be found to be fixed on one side, sometimes on both, and this occurs with a suddenness that puts inflammatory effusion out of the question altogether. In the majority of cases the effusion is not extensive enough to be felt above the brim of the pelvis, but in severe cases it is, and then it forms a rounded and distinctly limited tumor, with a feeling of distinct fluctuation. This upward limitation of the tumor and a distinct vaulting of the upper surface, the effusion of blood round the rectum, and a peculiar concave vaulting of the lower surface of the mass, form the characteristic signs of extra-peritoneal effusion of blood. The mass is, in fact, like an irregularly shaped jelly-fish, rounded above, concave below, and this shape is uniformly regulated by the relations of the peritoneum and pelvic fascia. The edges of the mass are felt to fade off downward on the walls of the pelvis, just as the groins of a Norman crypt fade off on the brackets or capitals which support them. The effusion of the intra-peritoneal hæmatocele, contained in the rounded cavity of the retro-uterine cul-de-sac, bulges into the vagina like a dilated bag. I cannot form any exact estimate of how many cases of these operative hæmatoceles I have seen, but it certainly is not less than fifty, and is more likely to be seventy or eighty, and I have been induced to meddle only with the one I have narrated. In every case in which I have diganosed the condition the patient has recovered, and in the necropsies which have been made upon cases operated upon by me no record of the incident occurs, so that I conclude it is an accident very nearly, if not quite, devoid of mortality. Its only drawback is that it delays convalescence for ten or fourteen days, and otherwise I believe it to be of no account at all.

Extra-peritoneal effusion of blood is also very common apart from cases of operation, but by reason of the same sudden

arrest of a metrostaxis. Its symptoms in such cases are very much what I have described—sudden pain, a feeling of faintness in severe cases, with a rise of pulse, and even of temperature. On examination the uterus is felt fixed and generally pushed forward with a boggy swelling behind or on one side of the uterus, and if the effusion be large the mass is felt distinctly limited by the distended broad ligament above the brim of the pelvis, this latter condition being the essential diagnostic difference between the two varieties of hæmatocele. An intraperitoneal hemorrhage unlimited in quantity or by membrane has never yet been felt by me as a defined tumor above the brim, and I have now had a large experience of such cases in which the diagnosis was confirmed by operation or post-mortem examination.

Cases of effusion of blood into the broad ligament by sudden arrest, or at least associated with sudden arrest of menstruation, are, as I have said, very common, and I am sure that large numbers of these occur without the patients thinking it worth while to ask for medical assistance, and they get quite well without it. It is, as I have said, a condition probably almost free from primary mortality, and has only a remote secondary mortality when one thrombus breaks down and suppurates, and brings about the tedious condition of pelvic abscess.

Effusion of blood into the broad ligament also occurs from another cause, much more rare and probably much more fatal, certainly much more serious—I mean rupture of a tuba pregnancy about the twelfth week of its existence into the cavity of the broad ligament. It is now pretty well admitted that the scheme of the pathology of extra-uterine pregnancy which I have just given in detail covers and explains all the facts of that curious condition, and now it is capable of being used to help us to understand pelvic hæmatocele. Briefly, to recapitulate what I have said at length elsewhere, this view is that impregnation under normal conditions can and does occur only in the uterus. So long as the ciliated epithelium is in action in the tubes, spermatozoa cannot enter them, and the ovules cannot adhere to their walls. But the moment an ovule reaches the cleansed and fresh endometrium infiltrated with spermatozoa, its adhesion occurs. Destruction of the ciliated epithelium of the tubes by desquamative salpingitis,

or otherwise, reduces the internal tubal surface to the condition of that of the endometrium, and then entrance of the spermatozoa, impregnation, and adhesion of the ovum are possible, and we have the occurrence of tubal pregnancy. The tube is distended by the growing ovum, and as it is not capable of indefinite distention it ruptures, and the seat of rupture seems to be determined by the site of the placenta where the sinuses have so channelled the walls as to weaken them greatly. A glance at a section of a Fallopian tube will show (see p. 466) that there are two areas in that section which will severally give very different results, as one or other of them is the site of the rupture. One is much the smaller of the two, and is situated between the layers of the broad ligament, forming, as it were, an ideal roof to the cavity of that space. Rupture here of course means that hemorrhage occurs into the cavity of the broad ligament, and that an extra-peritoneal hæmatocele results. Such a case may, as I have already proved from Bernutz, become fatal in its later course by a further and second rupture of the broad ligament cyst and hemorrhage into the peritoneal cavity. But I feel sure that the great majority of these cases end then and there by the natural cure as mere extra-peritoneal hæmatoceles; the ovum dies and everything is absorbed (*e* in my tabulated scheme). I am quite sure that I have watched several cases of this kind. In the minority of cases the ovum is not killed but develops into a broad ligament pregnancy (*d* in my tabulated scheme), formerly known as the "sous-peritonéo-pelvienne" variety of Dezeimeris. I have had seven cases of this kind which I have operated upon, saving five of the mothers and three of the children. Every one of the cases of extra-uterine pregnancy operated upon at or after the full time with which I am familiar, comes easily within this explanation, and it makes quite simple what is an otherwise wholly unintelligible jumble in physiology as well as pathology. These cases may die at any point of their subsequent progress up to the full time, and then either remain quiescent as lithopedia (*g* in the scheme), or may suppurate and be discharged in various directions (*h* in scheme). The conclusions, therefore, are that, save under three sets of circumstances, extra-peritoneal hæmatocele is an accident perfectly free from danger. These are (*a*) when a secondary rupture of the cyst occurs with continued bleeding

into the peritoneum; (*b*) when it is merely a stage in the growth of extra-uterine pregnancy; (*c*) when it goes on to suppuration, a condition I shall refer to in its proper place.

I do not think that suppuration of a broad ligament hæmatocele is very common, and yet I have met with quite a large number of cases. Of course by the ordinary methods of proceeding, and those which certainly ought to be adopted in the milder cases, it would be very difficult indeed, if not impossible, to make anything like an exact differential diagnosis between a suppurating hæmatocele of the broad ligament and several other conditions which I need not specifically allude to, and therefore any assertions concerning them, after dealing with them as they used to be, and as I say they ought to be dealt with in the majority of instances, by tapping from the vagina, may easily be met by the criticism adopted by M. Bernutz against M. Nonat, that at least one necropsy ought to be in existence to prove the assertions. I have, however, already pointed out that an abdominal section performed before death is, for such a purpose as this, quite as satisfactory as a post-mortem examination. In the sixty-third volume of the Transactions of the Royal Medical and Chirurgical Society I published a series of six cases in which I had adopted, for reasons of extreme seriousness, an abdominal section for dealing with pelvic abscesses which otherwise would have opened out in the usual disastrous ways. I say there that "these six cases have all been, so far as I can discover, cases of suppuration occurring in pelvic hæmatoceles," and I may at once dismiss this question by saying briefly that this depended upon the fact that in clearing out the cavity of the abscesses I removed a considerable quantity of laminated, broken down, old clot. All these cases were undoubtedly extra-peritoneal hæmatoceles.

Of these cases I select only one as a characteristic example, to illustrate alike their pathology and treatment; and the history of the case is eminently suggestive that it had its origin in a broad-ligament pregnancy.

The patient was sent to me by Dr. Flynn, of Birchills, now of Kingstown, Dublin. She was forty-five years of age, and had never been pregnant save one doubtful miscarriage soon after marriage nineteen years before. Symptoms resembling those of hæmatocele had occurred eight months before (after

an arrest of menstruation for three months) I saw her, and since that time she had been losing flesh, had lost her appetite, was troubled by constant thirst and night sweats, and had a rising night temperature. The uterus was fixed in a mass of effusion occupying the left broad ligament and partly the right one also, and the mass on the left side encircled the rectum, forming a pronounced stricture of the rectum, as hæmatoceles of the left broad ligament frequently cause. No point of fluctuation could be felt in the pelvis, but the symptoms pointed clearly to the presence of pus. I therefore determined to open the abdomen, and readily obtained the consent of my colleague to this proceeding. A large abscess was opened just behind the base of the bladder, between which and the uterus it principally lay, but stretching round behind the rectum. The floor and posterior wall of the abscess were found to consist of old laminated blood clot, so that its origin had been in a blood effusion into the broad ligament. A glass drainage-tube was inserted and this was changed for one of Chassaignac's wire tubes on the eleventh day after operation. She sat up on the twenty-first day and the tube was finally removed on the twenty-sixth. She went home on the thirtieth day perfectly well and has remained so ever since, now nearly eight years.

About thirty of such cases have been operated on by me and have all recovered. I say about thirty because I could not be sure that all these cases of pelvic abscess originated in suppurating hæmatocele, and I can form no estimate of how many of these were originally cases of tubal pregnancies bursting into the broad ligament and then suppurating, but I suspect that more than half of them were.

Therefore, I conclude that extra-peritoneal hæmatocele arising from tubal pregnancy, though rarely fatal, has serious consequences in a fairly large proportion of cases. How different it is with intra-peritoneal hæmatocele from the same cause, I have already sufficiently indicated. My first experience of this condition was one of the saddest things I have ever known, a young married lady, one of the most charming and brilliant of women, the daughter of an author known wherever the English language is spoken, the wife of one of the most brilliant of surgeons, died after a short illness, and after a post-mortem examination, a ruptured tubal pregnancy

was found to be the cause of an enormous intra-peritoneal hæmatocele. Another case of immense importance in my own experience, and I venture to think of still greater importance in the history of surgery, I have already given, because from that point we have been able to fulfil the indications so urgently presented by the quotations I have made from John Parry.

During the twenty years which elapsed between the case I am speaking of, and the first of my surgical ante-mortem experiences of these dreadful accidents, I had seen at least twenty-three cases of a similar kind, and therefore I can entirely confirm what M. Goupil says of these cases to this effect —"So frequent is the occurrence of intra-pelvic" (by this he means the true or intra-peritoneal) "hæmatocele that I have made an analysis of forty-two of my cases, which are irrefutable as to their diagnosis." He gives us the causes of these cases. 1. Hemorrhage caused by the rupture of dilated utero-ovarian veins. 2. Hemorrhage from rupture of the ovary. 3. Hemorrhage caused by rupture of the Fallopian tube. 4. Hemorrhage from the fœtal cyst itself having ruptured; and he says, "The largest number of cases fall under the last head" —5. Hemorrhage within the fœtal cyst. Probably, now, M. Goupil would group the last three causes under one head—at least I certainly should. At another place he says that ruptured tubal pregnancies are very common; for according to Nonat, Baudelocque saw five examples in three months, and I know that any one who takes a research in our serial literature will find them in abundance. The final argument as to their not being so rare as our text-books seem to assert, is the fact that between January, 1883, and July, 1888, I operated upon thirty-nine cases, and succeeded in saving thirty-seven of them—a very striking contrast to the old plan of letting them alone to die. I have never seen a case of suspected rupture, or one in which we suspected intra-peritoneal effusion of blood, recover if left alone.

As to the causes of intra-peritoneal hemorrhage I have been able to speak only of two from personal experience—the first, and by far the most common, being ruptured tubal pregnancy. The second is hemorrhage from some torn adhesions or badly-tied vessel after an abdominal section. Thus, I tied the pedicle of one ovarian tumor with catgut, and the patient died on the fourth day after the operation. I found a large

intra-peritoneal hæmatocele due to the digestion and loosening of the ligature. In searching the literature of this question I have found one case due to the rupture of an aneurism of the cœliac axis, and a large number of cases having a traumatic origin chiefly from rupture of the liver. Bernutz and Goupil have collected a few instances due to rupture of dilated uterine and ovarian veins not connected with pregnancy; also two cases of rupture of the ovary in pregnancy. But everywhere the evidence is overwhelming that the most fertile source of this most fatal accident is rupture of a Fallopian tube dilated by a fertilized ovum. In very many of these cases a feature of great interest is the fact that the first attack of hemorrhage is generally not fatal, and that the records yield incontestable evidence that it may require the repeated occurrence of bleeding to bring about the fatal issue. In some of these attacks hemorrhage seems to have been separated by long intervals. Thus, one case recorded as having occurred in the Maison d'Accouchement in 1816, where the history makes it evident that the tubal rupture occurred at the usual time in the third month, the fatal hemorrhage did not occur till the sixth month of extra-uterine gestation. In this case the fœtus was found, so that there could be no doubt as to the nature of the case.

But in some of the instances recorded by Bernutz and Goupil themselves—and I prefer their facts to all others, being as they are so carefully sifted and so free from any efforts to theorize—it seems to me that the history was that of tubal rupture, repeated hemorrhage, absorption of the gelatinous fœtus, and final death from hemorrhage; so that when the post-mortem was made, the absence of a fœtus, which the authors note, blinded them to the real nature of the case. That the fœtus may disappear by absorption is made certain by my own experience. I have removed it only twelve times in my forty cases, though I have found the placenta in every one. Thus, case thirty-two in the work of Bernutz and Goupil is an instance of fatal intra-peritoneal hæmatocele due to a ruptured Fallopian tube which was distended by a tumor to the size of a pigeon's egg. I have no doubt that a microscopic examination of the tumor would have shown it to have been a placenta. I think that this explanation applied to many of the carefully recorded cases of these distinguished French

authors, in which the details given would incline us to characterize them as being identical with that which was the subject of the recent lamentable trial at Liverpool. There are half a dozen cases in Bernutz's book which are identical with that case, except that they were all fatal, whereas the Liverpool case was successful, the patient having been saved from death by surgical pluck and skill. That woman's peritoneum was occupied by a quantity of blood-clot and blood serum so great that it could be recognized by palpation before the operation. One of the Fallopian tubes was the source of hemorrhage because it had a quantity of blood and blood-clot in it when removed. Here is a case almost identical, taken from the "Lancet" of 1848. "A woman aged twenty-eight was suffering from rheumatism, when she was suddenly seized with nausea, vomiting, and pain in the right side of the abdomen; her face became anxious and pallid, the pulse imperceptible, the extremities cold, and the respiration oppressed, in short, collapse was complete, and she died in twenty-four hours, evidently from internal hemorrhage. On opening the abdomen a quantity of blood was seen, but no rupture of any of the viscera could be detected. In the pelvis a clot was found in the left Fallopian tube. On separating the uterus, its cavity was filled with muco-sanguineous fluid and lined with a decidual membrane. The left tube contained a clot of blood the size of an almond. About an inch from the uterus at the upper part of this tumor was a rent, and within was a small sac, so compressed and deformed by the clot that it was impossible to say whether or not it was an ovum. The left ovary was the size of an apple, filled with blood and ruptured."

Another case on the authority of Dr. Tilt is given by Bernutz at page 196: "A multipara, aged thirty-seven, was seized with lumbar pain, the menses were four days late, the left hypogastric region became tender, and some tympanites and vomiting followed. She succumbed in ten days. On post-mortem examination there was general peritonitis, a large clot of blood filled the left iliac fossa and pelvic cavity, the uterus was normal in size, and the appendages on the right side healthy; the left half of the uterus and its appendages were larger and more distended than the right; the Fallopian tube midway the size of a nut; a probe introduced at the fimbriated extremity passed into a cavity in the centre of the clot, which

dilated that portion of the tube." Another on page 197 is to the same effect: "On post-mortem examination all the abdominal organs were observed to be quite healthy, but very bloodless. In the pelvis a large quantity of blood was discovered, clotty and fluid. After careful examination of the principal blood-vessels, arterial and venous, without discovering anything abnormal the uterus was examined, together with its appendages and the source of the hemorrhage was soon apparent, a small rupture of the right Fallopian tube being discovered at about half an inch from its distal extremity; blood was oozing from it, and it was evident that this was the seat of the bleeding. The tube itself was also a good deal enlarged."

Dr. Goodall also gives a fatal case where death occurred from about eight pounds of blood being lost from the Fallopian tube, though there was no evidence of a tubal pregnancy.

Case 2 given by Bernutz (page 208) is extremely instructive, because it is a fatal case of hemorrhage from the Fallopian tube in a young woman, aged twenty-two, due apparently to measles. "On post-mortem examination hemorrhage was found to have proceeded from the left Fallopian tube, which was distended to the size of the index finger, and contained about two ounces of blood, partly fluid, partly coagulated, and through the abdominal orifice as much as sixteen ounces of blood had escaped into the pelvis. These samples of hæmatocele occurring in rubeola, scarlatina, and variola, demonstrate that this accident may occur in any severe fever."

Finally, I wish to refer to a case originally contributed to the "London and Edinburgh Monthly Journal" for 1841, because it establishes beyond all doubt, that fatal hemorrhage can occur from the Fallopian tube into the peritoneum, under circumstances where the occurrence of rupture of the tube by an ovum is out of the question. An illustration of the tube is given. "A large quantity of blood was effused into the abdomen and pelvis, mostly coagulated but partly fluid. At first it was impossible to say whence all this blood came, but on examining the pelvic viscera solid coagula were observed protruding from the open orifices of the Fallopian tubes. The tubes themselves were filled with blood and distended at a short distance from the uterus up to the distal extremity. The condition of the parts is very well shown in the accom-

panying sketch, which represents the serious state of one of the tubes and the appearance of the clot attached; the latter has a sort of lobulated appearance produced by the constrictions exerted upon it in its passage along the tube. The other tube was the same. The greater part of the blood found in the pelvis escaped from the tubes no doubt in a fluid state, but that which was attached to the tubes was coagulated before it left the canal, as is evident from its shape."

Concerning the prognosis of such cases Goupil says: "It is but too true, I fear, that we are authorized in saying, that all the cases of intra-peritoneal hemorrhage arising from extra-uterine pregnancy end in death—in fact all the cases that I have quoted have terminated in death; generally it has taken place in a few hours or days, and though death has been delayed for six months (as in the case already quoted) it is wholly exceptional." This was absolutely true in my own experience till I was emboldened—shall I say till I was shamed—by Mr. Hallwright's case, into opening the abdomen and saving their lives.

We come to the following conclusions: That in the great majority of cases of extra-peritoneal hæmatocele, even when due to ectopic pregnancy, the disease may generally be left alone, being rarely fatal, and that it is to be interfered with only when suppuration or extreme hemorrhage has occurred. That, on the contrary, intra-peritoneal hæmatocele is fatal with such almost uniform certainty that so soon as it is suspected the abdomen must be opened and the hæmorrhage arrested. In the overwhelming majority of cases the source of the hæmatocele will be found in the broad ligament, and then it can be dealt with, and with every prospect of success. If any one objects to this I appeal again to the canon of surgery which is of uniform application: For surgical hemorrhage cut down and tie the bleeding point; if a big branch of the femoral artery were bleeding my colleagues who deal in such cases would cut down and tie it. Why should Poupart's ligament be a line of demarcation within which this surgical writ will not run? Why should my friend Mr. Bryant be allowed to do to the external iliac artery what I am prohibited from doing to the internal iliac division? Indeed, at page 202 of Bernutz and Goupil's work they assert this principle: "The indication in such a case is plain—we must stop the hemorrhage."

A very admirable contribution has recently appeared from the pen of Professor Charles A. L. Read, of Cincinnati, which has so completely corroborated what I had already published on the matter that I venture to make some abstractions from his writings.

"It is stated by Mr. Lawson Tait,[1] in the Ingleby Lecture for 1886, that Bernutz,[2] in 1848, was the first to recognize the causal relationship existing between tubal pregnancy and hæmatocele, and to indicate the proper line of treatment, viz., laparotomy and ligature. I am disposed to accept this claim for Bernutz, so far as it relates to a suggestion of treatment; but I must insist that all the essential conditions of intra-abdominal accumulations of blood from ruptured tubal pregnancy were recognized and described[3] by one of Mr. Tait's own countrymen, Dr. John Burns, of Glasgow, as early as 1814. It is true, Burns does not use the word 'hæmatocele,' which was coined by Nélaton a quarter of a century later, and is such a positive misnomer that it had better never have been coined at all.

"Burns, however, said that 'the sac might burst and the patient die from hemorrhage.' He clearly described the subsequent changes when he said that 'irritation is produced, inflammatory symptoms supervene, and hectic takes place.' He still further indicated his insight into these cases when he said of them that 'the most frequent termination is that by inflammation ending in abscess.' And I know no better way to round out a description of what is now known as hæmatocele than by employing Burns' observation that the suppurating contents of a burst tubal pregnancy 'might be enclosed in a kind of cyst of lymph.' Blundell,[4] writing in 1830, recognized the same condition, and said that he did not doubt that 'many women die in this way, but, being buried without examination, the real cause of their death is never ascertained.' He even went so far as to mention the expediency of abdominal incision for the control of the hemorrhage, but doubtless under the pressure of the mediæval conservatism of Guy's Hospital—a conservatism which still exists in that institution—he abandoned the idea, leaving it to be again suggested by Bernutz thirty years later, and finally to be realized by Mr. Lawson Tait after the lapse of a half-dozen decades. Between Blundell and Tait, numerous writers touched upon both topics,

but relatively few of them recognized that tubal pregnancy caused intra-peritoneal hæmatocele. As soon as Nélaton, and, subsequently, Bernutz and Goupil, began to write upon hæmatocele, the profession appeared to drift away from the definite etiology and pathology so clearly outlined by John Burns, until latter-day authors attribute the condition to almost every other than what I believe to be the one most fruitful cause."

"It is fortunate, however, that the other side of this important pathological question has not been entirely neglected. From the time Blundell, in 1830, and Bernutz, in 1848, guessed at the nature of these cases and suggested the expediency of abdominal section for the control of the progressive hemorrhage, there was no one to put the suggestion into practice until Mr. Lawson Tait,[5] in 1883, did the operation. It was the beginning of a new era in the intelligent understanding and management of these hitherto intractable cases. Prior to that time the mortality was almost one hundred per cent; since that time I have heard of no one losing a case subjected to Mr. Tait's line of treatment, except in one instance, and that was a case in Mr. Tait's own hands. The pathology upon which the treatment was based, and which has been confirmed by the revelations of the treatment itself, has, however, been more generally accepted than has the practice. Schroeder,[6] J. Veit,[7] and Kiwisch[8] are among the leading Germans who agreed with Fritsch,[9] that 'the most frequent source of the hemorrhage is the ruptured ovisacs of a tubal or other extra-uterine pregnancy.' Among gynæcologists, Tait, Imlach, Berry Hart, Thomas, and Emmet are among the most conspicuous who concur in the doctrine, while among the obstetricians, Lusk, Parvin, Barnes, Galabin, and Playfair may be mentioned; indeed, it appears that among those who are the closest students of this question there is practical unanimity that tubal pregnancy is the most common cause of intra-peritoneal hæmatocele, but, as I have intimated, there is less unanimity on the subject of treatment. Why there should be variance on this question, when there is practical agreement on the more primary and fundamental one, is not apparent, unless we shall find that different constructions are placed upon those pathological changes which take place subsequent to extravasation. To my mind those changes were described

with great fidelity to truth, if not to details, by John Burns, already quoted.

"With this pathology as the guide, the duty of the surgeon with regard to treatment is, to my mind, obvious. It is merely an application of that general law of surgery which, as formulated by Tait and applied to these cases, is as follows: 'For surgical hemorrhage, cut down and tie the bleeding point; if a big branch of the femoral artery were bleeding, my colleagues who deal with such cases would cut down and tie it. Why should Poupart's ligament be a line of demarcation within which this writ will not run?' Bernutz and Goupil[10] say: 'The indication in such cases is plain—we must stop the hemorrhage.' No person, I fancy, who properly realizes the situation in the cases will deny the propriety of the proposition; but it occurs to me that there are other indications than hemorrhage for surgical interference—the damaged tube, the fœtal structures, and the placenta tissue demand attention. Of the latter two it may be said that, even though they become incapsulated and partially absorbed and the residue remain innocuous, they cannot be considered in a surgical sense other than as foreign bodies, and, as such, constant menaces to the health and life of the patient. The tube of necessity becomes destroyed as an oviduct, and, if left intact, it will only be to figure at a later period as a hæmato-salpinx or a pyo-salpinx, and consequently to demand extirpation."

Dr. Read concludes this paper with the following summary: (1) Intra-peritoneal hæmatocele is an intra-peritoneal accumulation of blood. (2) Ruptured tubal pregnancy, the most common form of extra-uterine fœtation, gives rise to an accumulation of blood within the peritoneum. (3) In consequence of the fluid condition of the extravasated blood and of the yielding character of the adjacent tissues, the hemorrhage has a tendency to continue. (4) In consequence of the death of the fœtus, there is developed a marked tendency to suppuration. (5) In consequence of becoming a foreign body, the product of conception, even though it become encysted, is a constant source of danger. (6) The damaged tube, if left *in situ*, can serve no other than a pathological purpose. (7) Abdominal section is therefore called for (*a*) to control progressive hemorrhage, (*b*) to remove dangerous débris, (*c*) to

extirpate worthless appendages, and (*d*) to overcome septic conditions.

I have now to conclude this important branch of my sub, ject by submitting a list of the cases, properly authenticated-as the custom now is, in which I have performed this operation up to time of writing. The number is thirty-nine (the full number is forty, including the extra-peritoneal case given on p. 498), and I need hardly say that this number includes every case of the kind I have done. There have been only two deaths. The first was due to my want of appreciation of the proper principle of the operation, and the second was due to the fact that the patient was practically in *articulo mortis* when I operated, interference having been too long delayed. The results on this list show a very different possibility of prognosis in this dreadful disaster when promptly treated on sound surgical principles, from that so hopelessly pronounced by Dr. Parry when they are left alone: "From a careful examination of this subject it must be acknowledged that a happy termination of the rupture of the cyst is exceedingly rare." "Of 149 cases in which the ovum was located in that portion of the tube which does not traverse the tissues of the uterus, 145 died."

COMPLETE LIST OF CASES.

No.	Residence.	Medical Attendant.	Age.	Date.	R.	D.
1	Wolverhampton	Dr. Spackman	41	17, I., 1883	..	D.
2	Solihull	Dr. Page	40	3, III., 1883	R.	..
3	Birmingham	Dr. Taylor	37	10, IV., 1884	R.	..
4	Birmingham	Dr. Wilson	27	21, V., 1884	R.	..
5	Birmingham	Dr. Leech	34	6, VI., 1884	R.	..
6	Walsall	Dr. G. Sharpe	28	23, VII., 1884	R.	..
7	Smethwick	Dr. Pitt	31	29, X., 1884	R.	..
8	Birmingham	Dr. Farncombe	30	28, XI., 1884	R.	..
9	Birmingham	Dr. Ward	35	9, XII., 1884	R.	..
10	Wolverhampton	Dr. Scott	41	9, II., 1885	R.	..
11	Birmingham	Dr. A. E. Clarke	30	2, IV., 1885	R.	..
12	Birmingham	L. T.	37	5, V., 1885	R.	..
13	Birmingham	Dr. Whitcombe	25	11, V., 1885	R.	..
14	Birmingham	Dr. Whitley	34	2, VII., 1885	R.	..
15	Birmingham	L. T.	42	11, VII., 1885	R.	..
16	Wolverhampton	Dr. Watts	31	2, IX., 1885	R.	..
17	Manchester	Dr. Walter	26	6, IX., 1885	R.	..
18	Birmingham	L. T.	28	19, IX., 1885	R.	..
19	Birmingham	L. T.	42	23, X., 1885	R.	..

No.	Residence.	Medical Attendant.	Age.	Date.	R.	D.
20.	Coventry	Dr. Davidson	37	31, X., 1885	R.	..
21.	Tipton	Dr. Price	24	2, II., 1886	R.	..
22.	Oldbury	Dr. Cunningham.	35	3, VII., 1886	R.	..
23.	Birmingham	Dr. Wilson	32	16, VII., 1886	R.	..
24.	Tipton	Dr. Price	34	27, IX., 1886	..	D.
25.	Birmingham	Dr. A. E. Clarke	44	26, I., 1887	R.	..
26.	Birmingham	Dr. Hoare	31	18, II., 1887	R.	..
27.	Halifax	Dr. Dolan	29	17, II., 1887	R.	..
28.	Coleford	Dr. Prosser	29	27, IV., 1887	R.	..
29.	Walsall	Dr. Gordon	30	6, V., 1887	R.	..
30.	Birmingham	Dr. Lafarelle	44	19, IX., 1887	R.	..
31.	Birmingham	Dr. Wilson	29	20, IX., 1887	R.	..
32.	Wrexham	Dr. Williams	37	30, IX., 1887	R.	..
33.	Nottingham	Dr. Hunter	37	16, XI., 1887	R.	..
34.	Birmingham	L. T.	37	16, XII., 1887	R.	..
35.	Birmingham	Dr. Harmar	41	7, I., 1888	R.	..
36.	Birmingham	Dr. Vokes	30	16, II., 1888	R.	..
37.	Kidderminster	Dr. Jotham	38	11, V., 1888	R.	..
38.	Derby	Dr. Carter Wigg	27	12, VI., 1888	R.	..
39.	Ilfracombe	Dr. Slade King	27	9, VII., 1888	R.	..
40*.	Birmingham	Dr. Drury	26	28, VIII., 1888	R.	..
41*.	Birmingham	Dr. Bracey	27	29, VIII., 1888	R.	..
42*.	Birmingham	Dr. Hallwright	35	25, IX., 1888	R,	..

The patients were all married women. Cases marked with an asterisk have occurred since these lectures were written.

Two of these cases require further details for special reasons, the first (Number 13), as strangely enough she fell a victim to a second calamity of the same kind, and the whole of her history is as follows:

On May 10th, 1885, Mrs. E. R., aged 25, was sent to me by Mr. W. P. Whitcombe, Victoria Road, Aston, suffering from urgent abdominal symptoms. The history was to the effect that she had been ailing from a short time before Christmas. She thought it was pregnancy. Menstruation had been suspended for three months. In April she had a period, and again early in May, and at the latter time she compained of violent pains in the lower abdomen, and on the 9th she had an attack of fainting with vomiting, the pain being referred to the lower abdomen. When I saw her she looked extremely ill and anæmic. A large ill-defined mass existed on the right side of the uterus intimately associated with the organ, and the roof of the pelvis was fixed. There was no difficulty in diagnosing the case to be one of ruptured tubal pregnancy. I opened the

abdomen on the 11th, and found the belly full of blood-clots and bloody serum. I removed the right Fallopian tube, which was occupied by a pregnancy of about the third month, and in its walls a large rent had occurred, through which the fœtus and placenta were partly protruding. Some points of bleeding from the intestine required touching with perchloride of iron, I inserted a drainage-tube, and the patient made an easy and rapid recovery. The case is published in a short paper on Ruptured Tubal Pregnancy, in the "British Medical Journal" of December 19th, 1885.

About eighteen months after this operation she was confined of a child, at the full term, being attended by a midwife, and there was nothing remarkable about the labor.

About fifteen months after this confinement she again became pregnant, and her husband states that during the period of this pregnancy (which she thought had turned four months) she had no symptoms of note, but only complained at intervals of slight pain in the abdomen, but not sufficiently severe to induce her to call in medical assistance. The only point on which he lays any stress was that she stated that she felt the child very plainly, more so it seemed to her than at the same period in any previous pregnancy.

Mr. Whitcombe was sent for to see her in the afternoon of March 9th, but he being from home, the patient was seen by his assistant shortly before 1 o'clock on that day. She was lying fully dressed on the bed, her knees drawn up, and was complaining of great pain in the hypogastrium. She was extremely pale and almost pulseless, and had had some vomiting. Mr. Hall was informed that only half an hour before she had been cleaning her fireplace, and, in the act of stooping, was seized with acute pain and a feeling of faintness. Stimulants were at once administered, and every effort made to restore her without avail, and the patient died shortly after 5 o'clock, clearly from internal hemorrhage.

Mr. Whitcombe made a post-mortem examination, and has been kind enough to give me the following particulars: He found the abdomen full of blood-clots and fluid blood; a large clot was adherent to a portion of the placenta which protruded from the uterine wall, and when this clot was separated it had a quantity of villous placental tissue adherent to it. All the organs were very anæmic and there could be no doubt that

the hemorrhage was the cause of death. Mr. Whitcombe was good enough to bring me the preparation, and aided by my assistant, Mr. Teichelmann, I am enabled to give the following report and drawing of the appearances presented.

There can be no doubt that the specimen represents an interstitial tubal pregnancy of the left side. The cavity in which the fœtus is situated is separated from the true uterine cavity by a strong septum of uterine tissue springing from each side of the uterine walls. The under surface of the septum and the rest of the uterine cavity is lined by hypertrophied mucous

Fig. 7.—Drawing from Preparation of Interstitial Pregnancy (Case XIII.) now in Queen's College Museum.

membrane (decidua) (B). The stump of the right Fallopian tube (C) is attached to what appears to be the lower angle of the uterus, but which is really the much-displaced upper angle. This displacement, however, is only apparent, and arises from enormous development of the left cornu of the uterus. A fine probe may be passed from the true uterine cavity into this stump. The left Fallopian tube (A), on the contrary, communicates with the cavity in which the fœtus and placenta lie, and the rupture has taken place in the upper and back part of the left uterine cornu. In this case we have the almost incredibly strange instance of a woman suffering from tubal

pregnancy twice, with the still stranger fact of her having a normal pregnancy between the two occurrences. From the first of her disasters she was saved by prompt surgical interference, and she might even have been saved the second time; but there can be no doubt that the poor woman's doom was sealed before medical assistance reached her, and there was no time then to effect the interference which was necessary. All the appearances of the preparation point to the fact that the woman's estimate of the period of her pregnancy was correct, and we have therefore an indication that the interstitial form

Fig. 8.—Diagrammatic Representation of Interstitial Tubal Pregnancy at Time of Rupture.

of pregnancy does, as we might have expected it would, take a longer time to arrive at the period of primary rupture than do those cases in which the pregnancy occupies the free part of the tube. In these latter we have no evidence as yet of any instance going beyond the twelfth or thirteenth week before primary rupture. It may be noticed here I am introducing a new phrase in using "primary rupture." I do so because I am becoming convinced that unless we make such a distinction as I have indicated we shall still continue some of the elements of confusion which exist about this interesting displacement.

It is perfectly clear that in all cases of tubal pregnancy,

when the ovum is growing, the tube must burst, and that it bursts in two directions, either into the peritoneal cavity or into the cavity of the broad ligament. In the free part of the tube this rupture takes place, as I have said, about the twelfth or thirteenth week. In the interstitial form, the case before us shows that the rupture may be deferred to a later date. The primary rupture into the peritoneal cavity seems to be almost necessarily fatal alike to mother and child; but when the rupture occurs into the cavity of the broad ligament, it may be followed by a continuance of the development of the child, and these only are the cases in which the child is permitted to reach a viable period.

In a recent number of the "New York Medical Record," a case is reported by Dr. Taft as being one in which no rupture had taken place. But the description given makes it perfectly certain that this was a case where the primary rupture had taken place into the cavity of the broad ligament. In this group of cases a secondary rupture at any period is possible, and therefore it is that the adoption of the terms, used strictly to indicate relative dates, will become very useful. This secondary rupture was most clearly demonstrated in Nonat's celebrated case as given by Bernutz, a case which, on account of the occurrence of this secondary rupture, is full of the greatest interest (see p. 498). This secondary rupture explains such an occurrence as that in Jessop's celebrated case.

Connected with the case I am now discussing there are many important points worth alluding to, some of which are new, and others, though quite familiar, are worth noticing on account of the confusion which still seems to exist in the mind of most recent writers on this subject, some illustrations of which I have already given. The patient was rather an intelligent woman for her class, who, having undergone the terrible experience involving her first operation, had obtained a fairly full knowledge of the nature of the accident, and what had been the condition as a consequence. Yet, with this dreadful experience, and the knowledge of it when the same condition recurred, so little did she suffer that, up to the moment of rupture, knowing she was pregnant, she never thought of asking for medical assistance; and this was the case also in her first tubal pregnancy. There were no symptoms whatever to draw

attention to her state until the rupture occurred; indeed, there were no symptoms even calling for examination.

The strangest thing of all to me is that, in the enormous experience I have now had of tubal pregnancy, I have never but once been called upon to make an examination until the rupture had occurred, and in that case there was neither history nor symptoms which enabled me to do more than determine that there was tubal occlusion; not, indeed, until the rupture occurred and the abdomen was opened was a diagnosis possible. Under these circumstances I think I may be excused for maintaining a somewhat sceptical attitude concerning the correctness of the diagnosis of those gentlemen who speak so confidently of making certain diagnosis in cases of tubal pregnancy before the period of rupture, and who speak with equal confidence of curing the cases by a puncture, either simple, medicated, or electrolytic.

The great bulk of the utterances in these directions may stand very well in "society discussions," or in "library papers," but they will not stand the test of bedside experience. Upon these points I have been much misrepresented, and am glad to have an opportunity of clearly stating my views; but I wish to state that after the period of rupture a diagnosis can be, and has in my own experience been, made correctly in the majority of instances.

Another point in connection with this interesting case is the fact, made abundantly clear by the preparation, that, no matter what the symptoms had been previous to rupture, physical examination could not have permitted any diagnosis other than that of normal pregnancy of about four months and a half.

This is my solitary experience of interstitial tubal pregnancy, but it so closely resembles a number which I have seen in museums that I take it to be quite typical of its class. I am, therefore, disposed to believe that from physical examination interstitial tubal pregnancy could not be diagnosed, and I can imagine no symptoms which would help us to recognize it before rupture.

The whole of the museum specimens of this class do not appear to amount to more than five or six, there is one in the Edinburgh College of Surgeons, one in the museum at Guy's Hospital, one in the museum at University College Hospital,

and another in the museum of the College of Surgeons, described by Mr. Alban Doran. That authority has also mentioned two in the museum of the London Hospital, which clearly, however, do not belong to this class at all, but are broad ligament pregnancies. Dr. John Parry speaks of 31 cases of interstitial pregnancy in his table of 500 of all kinds, but that he is mistaken in the great bulk of this group of 31 there can be no doubt whatever. This is indeeed one of the illustrations of the want of critical acumen on the part of Dr. Parry in making his statistical collections. There can be no doubt that this interstitial form is much more rare than he believed.

If we were to assume that in such a case as this a diagnosis could be made, much ingenious speculation might be indulged in as to what could have been best to do for the patient. If a correct estimate of the relation of parts could have been made, clearly what ought to have been done was to dilate the cervix, divide the septum freely, and empty the cornual cavity. To have attempted to destroy the child would not have benefited the patient one bit. The placenta would have gone on growing; and even if it had not, putrescible material would have been left, which must have burst into the peritoneal cavity. At the time of rupture, if surgical assistance could have reached the woman with sufficient promptitude, she might have been saved by a hysterectomy; and from the appearances at the post-mortem examination, there is no doubt that this could have been easily accomplished.

The last case on my list is also worthy of recital in detail, because it proves what we might have expected would occur occasionally, though it has never before been clearly proved, that intra-peritoneal rupture of a tubal pregnancy may not have an immediate fatal ending from hemorrhage. In such a case we might expect that intra-peritoneal digestion of the ovum would prove a satisfactory solution of the difficulty; but here it did not. A suppuration process interfered, the patient had repeated attacks of peritonitis from which she nearly died, and had I not relieved her from the presence of the decomposed remains of the ovum in a large suppurating cavity, there is no doubt but that one or two more such illnesses as it had already caused would have seen the end of the case.

The patient in question was twenty-seven years of age, had been married six years, and had never been, as far as she knew, pregnant, this being not unfrequently a leading feature of these cases. She had menstruated with perfect regularity until Christmas, then she had missed till March, and during the whole of that time she had been confined to bed with what was called inflammation of the womb, and was attended during that illness by a well-known practitioner in Liverpool. At the beginning of March she had so far recovered as to be able to get up for a short time, but on the second day of her getting out of bed, she was suddenly seized with acute violent pain, and was kept in bed again for three weeks with what was said to be, and what clearly was from her description, an attack of acute peritonitis. Early in April she returned home to Ilfracombe, and was then seen by Dr. Slade King, who recognized the fact that there was a tumor on the left side of the uterus. In April she had an attack which she described as being very like a recurrence of the peritonitis that she had in the previous month, and there had been two or three attacks since then, more or less severe. She had menstruated twice for a fortnight each time, the loss being very profuse, and the pain extremely severe. When I saw her on July 4th, she was emaciated, in constant pain, quite unable to get about, and evidently suffering from the presence of pus in the pelvis. Examination revealed a tumor, quite as large as a fœtal head, on the left side of the pelvis, fixed and extremely tender to touch. Such a history gave no clue whatever to what proved to be the real nature of the case, for even the suspension of the menstrual flow from January till March was what precisely might have occurred in a case of hæmatocele of the broad ligament, or in several other conditions, which might have been referred as an explanation of this case. Certainly, in the minds of those who gave the history, the suspicion of pregnancy had never been entertained, and my own diagnosis did not include a differential suggestion in the direction of tubal pregnancy, but was given as that of suppuration of the left Fallopian tube. That diagnosis proved to be perfectly correct so far as it went, but to be complete it ought to have been extended to include suppuration as the result of ruptured tubal pregnancy, but such an extension did not occur to me. The state of the patient was such as to demand immediate interference, and therefore I

opened the abdomen, and found a state of matters, the details of which were easily ascertained, and were as follows:

The omentum was glued over the contents of the pelvis, and I had a little difficulty in detaching its fringe from the base of the bladder. After I had done so, I found several coils of intestine adherent below it, and on removing these, I at once opened up a cavity from which escaped a quantity of extremely fœtid purulent fluid.

This cavity was as large as a Jaffa orange, and the first thing I came in contact with was a large mass of easily detached substance recognized at once by my fingers as a piece of placenta. I removed it, and the naked-eye appearances confirmed what I had uttered about it before I removed it. I then easily recognized that the cavity from which I had taken it was formed of the dilated and distended Fallopian tube, forming the anterior, posterior, and lower walls of the cavity, while the upper part was composed of the coils of intestine and omentum, which I had partly detached. All round the cavity I could feel a number of sharp hard points, and these I easily recognized as fœtal bones imbedded in the walls of the cyst. I removed as many of them as I could, and found that they were what I had believed them to be, for included in what I removed where a number of fœtal ribs and flat bones. I then proceeded carefully to detach that part of the cyst formed by the Fallopian tube, and when I had done so, I tied the pedicle, and removed what you now see before you. The presence of fimbriæ proved conclusively the accuracy of my supposition. We have here, then, a case of the greatest possible interest, for it proves what certainly has not been completely established up to the present time, that rupture of a tubal pregnancy into the peritoneal cavity may not be fatal at the time of rupture by reason of recurrent hemorrhage. There is one case quoted by Campbell, and originally narrated by the late Mr. Samuel Hey, of Leeds, in which I think it is possible to accept this conclusion as very nearly proved, but the difficulties of a certain diagnosis of ruptured tubal pregnancy are so great that without the complete proof which can be obtained only from a post-mortem examination or an abdominal section, it is very easy to throw doubt upon any such record.

Here, however, we have absolute proof of the occurrence of

tubal rupture into the peritoneal cavity, not only without a fatal issue at the time, but apparently without the occurrence of much hemorrhage. It is, however, very likely that such cases are very rare.

In the absence of this fatal incident of hemorrhage, it is not difficult to believe that the whole contents of the tube may be absorbed by the peritoneum, as the fœtus was in process of being absorbed in this instance; and but for the occurrence of suppuration, it probably would have been so completely absorbed in a few months that no trace of its existence could have been recognized. The facts, however, that in nature's own process of cure an interruption by suppuration occurred, leading to such extreme peril that the patient escaped narrowly at least three times from peritonitis, and that if she had been left alone long her death from the recurrence of this trouble would have been absolutely certain, shows completely that it is never safe to leave these cases to a natural termination, and that their treatment by electrolysis is mere nonsense.

It is impossible to imagine that the Fallopian tube could ever have resumed its functions, after being submitted to such an accident as this, and it is not difficult to believe that for months after, if not for years, it would have continued liable at any moment to the suppurative process, which you see here had taken place.

The patient has made an easy recovery, and under these circumstances, and with a growing experience of the small fatality resulting from this operation, I unhesitatingly recommend the removal of the Fallopian tube, together with the remains of the pregnancy in every instance, and as soon as possible.

It must now be clear that the progress of an ectopic gestation is the subject of a great cataclysm, the primary rupture of the tube duct, which may, and in the great majority of instances most certainly does, arrest its progress by destroying at one blow both mother and child, unless the surgeon boldly steps in to save the former.

We have now to consider the minority of cases in which the ovum survives the process of rupture, and this it can do only when the rupture takes place into the cavity of the broad ligament.

When the rupture takes this direction there is a great

probability that the process is accompanied by hemorrhage into the cellular tissue, and that we have a hæmatocele resulting. The actual proof of this I place on record at page 498, and I have no doubt, as I have already said, that many of the suppurating hæmatoceles upon which I have operated, have been originally tubal pregnancies, in which the rupture has taken place into the broad ligament and has caused hæmatocele. And I am quite as certain that many of the extra-peritoneal hæmatoceles which we see, and with which we never interfere, are also produced in this way. It is easy to understand now how Parry could say that "Extra-uterine gestation may be occasionally confounded with pelvic hæmatocele. It may sometimes be impossible to distinguish between them." This must be clearly the case when the hæmatocele is a mere stage or a result of the processes of the extra-uterine pregnancy. The confusion into which Parry gets at this point is very interesting, for it leads him to a series of quotations, and a series of inconsistent conclusions which go a very great way negatively to show the value of the scheme of ectopic pregnancy for which I am now arguing. Thus he says, "It has been stated, however, that peritonitis, by which means alone intra-peritoneal effusions can become encysted, rarely follows the rupture of an ectopic gestation," and in this he is perfectly correct. Peritonitis rarely occurs in these cases, and the talk there about the collections of blood becoming encysted by inflammatory process is the merest nonsense. The encystment is brought about by the distention of the broad ligament, by the effusion of blood into its cavity, and of course exists from the first. Parry quotes a case from Matthews Duncan, in which the latter authority gives his facts so clearly as to admit of no doubt as to what had happened, but without in the least understanding them. Duncan says that the woman had all the symptoms of intra-peritoneal hemorrhage, about a month before her death, whereas it was extra-peritoneal hemorrhage into the broad ligament, which he describes. He goes on to say that the process of encystment was going on with every prospect of recovery, when the tumor (a broad-ligament hæmatocele) burst into the cavity of the peritoneum (by a secondary rupture) causing fatal peritonitis. It would be difficult to find a clinical record so clearly given as this, with the evident conclusions so maladroitly overlooked and erroneous inferences

put on record. The strangest thing, however, is that this broad-ligament hæmatocele of Duncan is correctly interpreted by Parry in another part of his book, but still the true conclusion escaped him. In another passage Parry strongly urges the argument against the encystment theory by peritonitis, as follows, and I entirely agree with him: "There are few things in regard to extra-uterine pregnancy which excite more surprise than the rarity with which peritonitis is noted upon examination after death from rupture of the fœtal cyst. The practical conclusions that may be drawn from a careful investigation of this subject are: That peritonitis is a rare sequel of rupture of the cyst, and even when pain, tenderness, and other symptoms of this affection supervene after the escape of the ovum, they do not necessarily indicate the existence of inflammation."

"Peritonitis so rarely follows rupture of an extra-uterine gravid cyst, that the possibility of its occurrence need not be taken into consideration in the decision of any questions relating either to prognosis or to treatment."

Here the views of the process following the primary tubal rupture which I have advocated explain all the difficulties of the situation. Dezeimeris was the first to discover the fact that there was such a thing as a pregnancy under the pelvic peritoneum, though he neither recognized its frequency nor discovered the process by which it was brought about. But there was no disputing Dezeimeris' facts, for almost as soon as they were published they were confirmed. As late as 1842 Campbell disputed them, and brought forward the familiar "encystment theory" as an alternative explanation. "In the *sous-peritonéo-pelvienne,* or second variety of Dezeimeris," Campbell says, "it is difficult to comprehend how the ovulum can insinuate itself under the peritoneum which is reflected over the organs situated in the brim of the pelvis. Through time, certainly, the connections of the original cyst with the adjacent parts become so numerous that when superficially considered the ovum may seem to be enveloped by the layers of the broad ligament; but how it can pass under this appendage it is impossible to conceive." But the explanation is now before us, and Dezeimeris' facts have been confirmed by every unprejudiced observer.

As we have from this point to deal exclusively with cases

in which the direction of rupture has been into the cavity of the broad ligament, I must ask to be excused further re-iteration of the fact, and it must at the same time be taken for granted that when I speak of effusion of blood in connection with these cases, I mean effusion into the broad ligament only —extra-peritoneal hæmatocele. The only exception to this will be when I speak of secondary rupture, by which I mean rupture of the broad ligament, distended as the result of the primary rupture and its resulting hemorrhage, as in Nonat's case (p. 498), or in the case just alluded to, as so misunderstood by Matthews Duncan. This secondary rupture must, if it cause hemorrhage at all, pour the blood into the peritoneal cavity, and thus produce intra-peritoneal hæmatocele. If, when the rupture takes place into the broad ligament, the blood effusion should be considerable, it is not difficult to understand that the ovum will frequently be killed at once, and be absorbed in time as the blood itself is. The whole thing will disappear, and the patient will get well, and I have no doubt that this is the origin of many of the inexplicable hæmatoceles of the broad ligament which we meet with. I have already given a case of the kind proved by abdominal section. I have as little doubt that in this way very many cases of ectopic gestation have a fortunate ending.

But they do not all die in this way, and many of them go on developing in their new position, and their development may go to the full time. On the other hand, the death of the ovum may occur at any time up to the full period, and then a change, which I believe to be perfectly uniform, goes on slowly. The first part of this process is that the liquor amnii is absorbed, and then the soft parts of the fœtus and the bones are also as far as they can be. At the end we have a small cyst in the broad ligament containing fœtal bones and débris of fœtal tissue. From the record of numberless cases in the literature of this subject it is certain that ultimately most of these cysts begin to suppurate, and cause much suffering. Some of them we know remain quiescent, and are found as lithopedia, little and big, on the post-mortem table. Parry knew this, and expresses the fact well, when he says that "If the woman does not perish from rupture of the cyst during the first four or four and a half months of gestation, it is not likely that an opportunity will offer to inspect the body until at, or near, or

even some time after, the close of pregnancy." But as he did not recognize the process of rupture into the broad ligament he failed to understand the position, and his clinical accuracy suffers accordingly, while his subsequent pathological speculations are full of error. Thus, he continues his guesses about "encystment," though he has himself given the most conclusive argument against it, that the encysting process of inflammation is extremely rare—personally, I doubt if it occurs at all. He says, "Though the presence of an encysted fœtus is not incompatible with life, and even with comfort and usefulness, the woman who bears such a burden within her is in constant danger of the cyst taking on inflammatory action, which will greatly endanger and may even destroy her."

When the bone-containing cyst suppurates, the matter seeks an exit, and that is found uniformly in one of four directions: through the rectum, by far the most common; through the posterior vaginal cul-de-sac, the next in frequency; through the bladder; and most rarely through the abdominal wall at the umbilicus. "During the discharge of the decomposed child," says Parry, "the mother is subjected to all the dangers which result from the absorption of purulent and putrid matter," and he gives a table which, though it has no absolute value, is immensely suggestive of the terrible mortality of this process. He has tabulated 330 cases, and of these 105 died, and we may feel quite sure that, as in all such reckonings, this is an under-statement of the true death-rate, as we never hear so much of unsuccessful cases as of those that have a satisfactory ending. The cures involve a great amount of suffering, for they go on for years, and therefore deserve the surgical interference for which I shall afterward advance arguments.

But, first of all, let me say that the four directions in which the débris is evacuated prove clearly that its seat is the cavity of the broad ligament. If the seat of the trouble were the left broad ligament, and the effusion had dissected the peritoneum up from the rectum in forming the annular stricture of which I have spoken, it is into the rectum that the abscess would most likely lead. I have seen a number of these cases, and have removed fœtal débris through a hole in the rectum, opening straight into the cavity of the broad ligament; and, with my finger in the aperture and a sound in the uterus, I

have proved the site. The bladder and posterior flexure of the vagina would naturally expect to be the next most available seats of evacuation, and in the case of the latter opening I have again proved, as in the case of a rectal aperture, that the cavity of the broad ligament was the seat of the débris cavity. Finally, in the case of a lady who came to me from South America, and who for years had been passing phosphatic calculi from the bladder, the nuclei of which were fœtal vertebræ (the bodies of them), I opened the abscess cavity from above, without opening the peritoneum, cleared out a quantity of pus, fœtal hair, phosphatic deposit, and fœtal bones, and promptly cured her. I could pass my finger into the bladder by a hole in its right wall, and as the uterus was quite fixed *in situ*, there was no doubt but that the abscess was the result of the death of an ovum which had been extruded into the right broad ligament.

The exit of the products of the fœtal decomposition at the umbilicus was not intelligible to me till I saw the marvellous frozen sections, made by Berry Hart, of a cadaver in which he found a well-advanced broad ligament pregnancy. Then this and the many other riddles were cleared up; but these had all better wait till I speak of the relations of the peritoneum as altered by the growth of a broad-ligament pregnancy.

Let me speak of the abscesses opening by rectum, vagina, and bladder more in detail.

In all of these the history helps but little, for the story is seldom more than that of obscure pelvic trouble ending in an abscess bursting and continuously discharging into the rectum, and it is not till the arrest of some sharp spicula of fœtal bone in the anus declares the true solution that the nature of the case is discovered. Most of these women suffer severely till the abscess bursts, and then they are able to get about at times, though on the whole they lead invalid lives. The mortality is doubtless quite what it is asserted by Parry, though I never saw a fatal case. All that have come under my own care have been easily cured by the complete emptying of the sac.

The cases where the abscess has burst through the vagina have histories very much as in the former case, and they certainly suffer less, and the cure is easier still. Quite lately a woman came to my out-patient department with the remains

of a fœtal femur sticking out of a hole just behind the cervix, and to the left. I enlarged the opening, took out two or three teaspoonfuls of débris, and she was cured within a month. Parry tells us that under exceptional circumstances the contents of extra-uterine fœtal cavities may make their way to the surface by a fistula through the perineum, and he quoted Dr. Yardley, of Philadelphia, as having recorded an observation of this kind. Such an occurrence would clearly form only an extension of their method of extrusion by the vagina, the walls of the vagina and bladder being separated by the advancing abscess, and the opening taking place as low down as it could in the perineum.

In the cases when the discharge is into the bladder the story is very different. Parry says it is much more fatal than discharge in other directions; and I think this is very likely, for in addition to the pelvic abscess we have the very serious complication of cystitis, leading to pyelitis and abscess of the kidney, a complication I certainly have not seen, but one which is, on *a priori* ground, very probable. But the mere sufferings of the patient, greatly enhanced by the formation of phosphatic concretions, would alone be enough to increase the mortality.

Curiously enough I have never yet seen one of these cases in its early stage, though I have for long expected to come across them, because it has been my habit for years to deal with all pelvic suppuration by abdominal section. By this method I get results far more rapid, complete, and permanent than in any other way, and doubtless some day I shall have the experience of opening a suppurating fœtal cyst before it has made its external opening. As I have said, I have opened one from above after it had already made its own way into the bladder, and with a brilliant result, for the patient was cured at once after years of suffering. But the case does not reckon as one of abdominal section, as I did not open the peritoneum, and by the definition I have adopted and have illustrated and defended elsewhere, this is necessary to constitute an abdominal section.

I feel quite confident that if these cases were dealt with by opening from above in their earlier stages, much of their mortality would disappear, and the patients would be spared years of suffering. I would treat them as I do pelvic abscesses, and

if the peritoneum were opened I should close it in my usual fashion, by stitching the opening in the walls of the cavity of the broad ligament to the opening in the parietal peritoneum (see pelvic abscess), after emptying the decomposing débris and cleaning out the cavity. I have now done over fifty operations of this nature, and not only has there been no mortality, but the cures have been so rapid, complete, and permanent as to give me perhaps more satisfaction than almost any other class of my work. I have said nothing about the differential diagnosis of broad-ligament abscesses originating in the deaths of ectopic ova, because I hardly think it possible till bones are found in the discharges, and then of course it is easy enough. Before this has happened I have had no experience of them, as I have said; when I do have I shall certainly not trouble about the differential diagnosis, and the want of it will certainly not delay my interference for an hour, for my rule is to get pus out of the pelvis as soon as I am satisfied it is there.

The death of the fœtus may occur, as I have said, up to any time of fœtal life, and if suppuration of the fœtal cavity occurs there can be but little variation in the processes, or in the proceedings required for their relief. Of course the larger the fœtus the greater the trouble, the more urgent need for interference; and the larger the fœtus the greater the possibility of the sac bursting at the umbilicus, an accident to be afterward discussed.

Now we come to the later stage, and the last division of my subject, the minority of the minority of cases, where the ovum survives and grows toward the full time.

During this process of growth the secondary rupture of the broad ligament sac may take place, and prove fatal, as in the cases recorded by Nonat, Bernutz, and Matthews Duncan. Such an accident would give rise to alarming symptoms, similar to those observed in primary rupture, and so far as we know from a few recorded cases, the accident would be quite as fatal. One case of such a rupture has been recorded which was not fatal, and in which the child was removed, and it forms an instance perfectly unique in the history of ectopic pregnancy, for the child was absolutely free in the peritoneal cavity, not encapsulated by cyst. Mr. T. R. Jessop, who records the case, puts it among what he calls, quoting the text-books, the "abdominal variety." If he had said intra-perito-

neal variety his language would have been more accurate, but as a matter of fact it stands by itself, and may therefore be known as *the* case of intra-peritoneal ectopic gestation. Fortunately no post-mortem was necessary, but it is perfectly clear from the history, that about the tenth week she had a "rupture" and that this was tubal is, in my belief, quite certain. If the pregnancy had ruptured its way into the peritoneum it would have been at once digested; for I am certain, from what I know of the digesting powers of the abdomen, no gelatinous fœtus of the tenth week could resist them. I interpret this case then to be one where a broad-ligament pregnancy on the right side went on till the seventh or eighth month, and that then a secondary rupture of the broad-ligament cyst took place, the child escaped into the peritoneal cavity, and continued its life among the intestines, its tissues having arrived at a period of development by that time which enabled them to resist the efforts of digestion which doubtless would be directed toward them. The ruptured cyst would contract and disappear toward its edges, and the placenta was found where it is found in the great bulk of broad-ligament cysts, plastered over the pelvic contents.

The following is an abstract of the case: "M. C., aged 26, has enjoyed fair average health up to the commencement of the illness. In March, 1869, she gave birth to an only child, after a labor in all respects natural, and, having weaned the child, she menstruated with moderate regularity up to 1874. From the beginning of January, 1875, her menstruation ceased, and she believed herself to be in the family way, early in March she was about two months pregnant, while washing she was suddenly seized with violent pain in the right side of the belly, which caused her to faint, she was taken to bed, and her ordinary medical attendant was sent for, she was suffering from violent pain in the abdomen, with swelling, vomiting, retention of urine, and high pulse, and for two months she was confined to bed, suffering from abdominal pain, sickness, and loss of appetite. Toward the middle of May she began to feel the movements of a child, and at the same time noticed a hard swelling in the lower part of the abdomen, toward the right side. On the 13th August Mr. Samuel Hey and Mr. Clayton in consultation determined the existence of an extra-uterine living fœtus, and she was taken to the Leeds Infirmary, under

Mr. Jessop, the same day. The abdomen was throughout distended. At the umbilicus and below was a large rounded prominence, which gradually sloped off toward the ensiform cartilage, and terminated inferiorly somewhat abruptly in a hollow, which was bounded again by a lesser prominence immediately above the pubes. On a closer examination the umbilical prominence presented the characters of a child's breech; the cleft and the two buttocks were distinctly traceable through the thin abdominal walls, and extending upward in a straight line toward the sternum the little prominences of the vertebral spinal processes were plainly perceptible. Above the pubes two feet could be made out, and above the umbilicus, immediately below the ribs, it was not difficult to map out the outlines of the two scapulæ. The rapid beating of the fœtal heart could be most distinctly heard toward the right side above the umbilicus. The breasts were enlarged and the areolæ were fairly developed."

"On examination, per vaginam, the uterus felt somewhat enlarged, and on measurement by Simpson's sound its cavity was found to be 2½ inches in length. The uterus remained motionless, while the abdominal contents were swayed from side to side. On several occasions the movements of the child were plainly visible, and indicated considerable vigor. After repeated careful search we were unable to satisfy ourselves of the presence of a placental souffle. The diagnosis of extra-uterine gestation seemed complete. The woman's condition was becoming extremely critical. Under these circumstances it was decided to remove the child by abdominal section. With the full concurrence of my colleagues, I accordingly proceeded to perform the operation at 12:30, on the morning of the 14th of August."

The patient having been placed under the influence of ether, and the bladder emptied of urine an incision six inches long was made through the linea alba, with the umbilicus at its centre. The abdominal wall was unusually thin, but more vascular than common; and the peritoneal lining, though natural on its free surface, appeared thick and velvety on section. Immediately upon the completion of the incision the breech and back of the child, thickly coated with vernix caseosa, came directly into view. At the upper part of the wound the omentum was seen lying like a cape upon the child's shoul-

ders, and inferiorly the funis, of natural appearance, passed transversely across the wound, and was traced round the external aspect of the left thigh of the fœtus to its attachment at the umbilicus. The child was in a kneeling position, its breech presenting toward the mother's navel; its head, folded upon its chest, buried beneath the omentum and transverse colon; the soles of its feet pointing toward the pubes, and its knees resting upon the posterior brim of the pelvis. . . . Its removal was readily effected. The funis was tied and separated in the usual manner, and the child was handed over to the custody of two gentlemen previously appointed to look after its well-being. . . . It was now seen that the gestation had been of the "abdominal" variety; no trace of cyst or of membrane could be found. The child had lodged in the midst of the bowels, free in the cavity of the abdomen. A few bands of unorganized lymph of a very friable nature lying upon, but not adherent to, intestines, were readily removed by sponging, and about an ounce of a clear serum was found in the peritoneal cavity. On tracing the umbilical cord, the placenta, having a larger superficial area than natural, was seen covering the inlet of the pelvis, like the lid of a pot, and extending some distance posteriorly above the brim, where it apparently had an attachment to the large bowel and posterior abdominal wall. Near its centre was a round prominence, which seemed to correspond with the swollen fundus of the uterus beneath. Great and especial care was taken not to cause the smallest disturbance to its connections. The placenta was indeed left untouched. On the 29th of October the wound is reported as quite healed; and three weeks later she returned to her home. From that time to the present she has kept in good health.

Menstruation commenced about a month after she left the infirmary, and has recurred at regular periods ever since. The child was as healthy, vigorous, and large as an average child born in the natural way; and it continued to thrive well until July, 1876, when, after a week's illness, it died of croup and inflammation of the lungs at the age of eleven months."

I have placed this case by itself, because it is the only one of its kind, and the only one which, after critical investigation, will admit of being termed "abdominal," or intra-perito-

neal pregnancy. Certainly those quoted by Parry will not do so, and I have met with no others.

Another somewhat similar case is published in the "Die Krankheiten der Tuben," by L. Bandl and is to be found in Tarnier and Budin's book, and is as follows: "In the case reported by this last author in a multipara examined several times, he diagnosed extra-uterine pregnancy. The child was living and arrived at full time. The patient refused gastrotomy; phenomena of false labor, and expulsion of the decidua occurred, and some symptoms of peritonitis having supervened, she succumbed. He immediately performed laparotomy; the child, who weighed 3,800 grammes, was extracted alive, but it only breathed three times and died. The following day, at the autopsy on the mother, they found in the abdominal cavity about 2,500 grammes of thick fluid, but nowhere could they discover the fœtal membranes. There existed, however, a pocket which inclosed the fœtus on all sides, but the walls of this pocket were formed by false membranes about four or five millimetres thick, and which hid the anterior, posterior, and lateral abdominal walls, the small intestines, the ascending colon, the descending colon, etc. On the internal surface of the pouch were a certain number of threads, some thick and some thin, which extended from one wall to the other. A mass which comprised the placenta in its thickness lay in part on the internal iliac fossa, and penetrated into the little basin on the right side. Some very dilated vessels, being the size of a raven's quill, were very close to this placenta. The umbilical cord, part of the fœtus, formed a handle round the uterus, and penetrated by a circular orifice, which was a centimeter and a half in diameter, into a cavity of which the walls were smooth; the fœtal surface of the placenta limited this cavity, into which the finger could easily penetrate. Outside the opening round the cord were prominences of wrinkled ovular membranes of a yellow-brown color, and dating from the first months of the pregnancy. Here the evidence of the remains of the broad ligament cyst clearly point to the occurrence of secondary rupture."

Under the circumstances of Jessop's case nothing could have been easier than the diagnosis, though there is one source of error which I have met with several times, and no authority, Perry excepted, makes any allusion to it, so far as

I know. At page 103 he says: "I have met with an example of thinning of the abdominal walls a few years since, which was exceedingly puzzling. I was asked by Dr. E. W. Watson to see a young woman, to decide the nature of an abdominal tumor, which was the size of a seven and a half or eight months' gravid uterus. Upon making pressure upon the enlarged abdomen a fœtus was felt receding from beneath the finger, against which it immediately rebounded. It was so superficial in its situation that it appeared impossible to believe that there was anything more than the skin of the abdominal wall interposed between the fingers and the child."

It is clear that in such a condition we must have not only a "thinning of the abdominal walls," but a want of development of the uterine tissue; and a few cases in which this arrest of development was so remarkable that the walls were no thicker than a single fold of a towel, forms a part of the curiosities of my experience. In one case in the practice of Mr. Langley Browne, of West Bromwich, we found a very thin uterus extremely retroverted. In the others the conditions were those of extremely thin walls, with some kind of displacement, as latero-flexion or retroflexion, and in these patience always solved the doubts. If I met with a case where any urgent symptoms existed, I would not hesitate to use the sound or use my dilators if necessary; for the worst that could happen, in the event of mistake, would be a premature labor.

This condition of extreme thinness of the uterine walls, in a pregnancy perfectly normal in every other respect, is a point which has not yet received the notice it deserves. It is, however, of sufficiently common occurrence to be a source of difficulty and danger, and therefore I propose to say here what I have noticed about it, in the hope that it may draw the attention of some one engaged in obstetric practice who may be able to investigate it more fully. I can now recall eight cases in which I have been consulted concerning a supposed extra-uterine pregnancy, yet in which there was only an extreme thinness of the uterine walls. I have no record of three of the cases, but of the others I have more accurate data than mere recollection. The features of all of them had much in common, and the known histories of four quite establish this. The ordinary symptoms of pregnancy were present in all of

them, and in only one was there any doubt as to its existence. The question generally was, Is the child in the abdominal cavity? and sometimes I had great difficulty in persuading the gentlemen who brought the patients to me that the position of the child was normal. Save in one case—that seen by me with Dr. Whitwell, at Shrewsbury—there was a marked absence of the liquor amnii, so that the movements of the child could be seen and felt in a most striking manner. In the pelvis the finger came upon the presenting part of the fœtus, as if it lay immediately under the mucous membrane; and it was only on very careful investigation that the attenuated cervix uteri could be made out, spread over the body of the child.

These cases were, with one exception, all under the seventh month. In the eighth and ninth months the walls of the uterus thickened, the quantity of liquor amnii increased, and the cases terminated in perfectly natural labors. The exceptional case I have seen within the last few days, and the pregnancy had advanced well into the eighth month. Vaginal examination makes it quite clear that the pregnancy was intra-uterine, while from the appearance of the abdomen alone the conclusion would have been inevitable that the child lay among the intestines.

These facts were given to me in connection with Mr. Langley Browne's case, also with a case which was watched by Dr. Hill Norris, and attended by him in her confinement. In Dr. Whitwell's case there was a large, thin-walled cyst, through which the child could be felt with the most astonishing distinctness, and it floated about as if it were perfectly free in the abdomen. He wrote to me afterward that "the patient went on very well, that some time before the expiry of gestation the fœtus became much more a fixed body, which undoubtedly showed an increased thickening of the walls of the uterus, as well as enlargement of the fœtus, and that her labor was quick and without any subsequent hemorrhage."

The other conditions with which extra-uterine pregnancy may be confused, before the death of the child, are (*a*) displacement of the normally pregnant uterus during the early months of pregnancy, complicated with fibro-myoma or cystic disease of the uterus; and, more rarely, (*b*) pregnancy of one-half of a double uterus. In a case which I saw with the late Mr. Ross, of Wakefield, I diagnosed either extra-uterine gestation

or a double uterus with pregnancy of one side, and it turned out to be the latter. Frequently we have considerable lateral displacements of the normally pregnant uterus, especially in unmarried women, sent to the specialist as something very different to what they really are.

But it is in cases seen after the death of the child, or at least when the time of the expected confinement has passed so long that if there is a child it is sure to be dead, that our most serious difficulties in diagnosis are met with.

The first point to consider is the history given by the patient of her supposed pregnancy, and the events which occurred at and after the time of her expected delivery. It is somewhat remarkable, and I think it is in favor of the views of the pathology of tubal pregnancy which I have advanced, that the majority of the instances of this abnormality occur in women who have not borne children previously, or in those who have had no children for many years. This point in the history of the patient is therefore always noteworthy. The other matters requiring careful consideration are the sudden arrest of the menses, the gradual increase in size, the occurrence of symptoms of labor at or about the end of the ninth month, and the subsequent diminution in size. Of all those points, the last is the only one having the importance of a sign; but it must always be borne in mind that no history, however complete, is of sufficient weight to establish a diagnosis unless there be some distinct physical signs in support of it. This I lay down as a rule based upon a remarkable experience, which I published in detail in the Transactions of the Obstetrical Society of London for 1874. In this case I had diagnosed double ovarian tumor, but was completely misled by a subsequent history which the patient volunteered. This was to the effect that just three years before she had believed herself pregnant, because her menstruation had ceased for eight months, her abdomen had slowly enlarged, and so had also her breasts. She was also quite sure that she had often felt movements, and, indeed, had all the feelings that she had experienced in each of her seven pregnancies. One day, when walking in the street, she was seized with pains, exactly like labor pains, and these lasted for four hours. At these pains she felt no surprise, fully believing that she was in labor. She felt as if a child was about to pass from her, and was

aware of the "swelling pressing downward." She afterward felt this "pass back into the belly," the pains ceased, and her size remained unaltered. At this false labor there was no discharge. Up to the time when I first saw her she is quite certain no diminution of her size had ever occurred, and that there had been very little increase, if any.

The physical signs of the case were those of multilocular disease of both ovaries, and on them I need not dwell. I found it was so when I operated, and the operation was successful. The lesson of the case is that we should place very little confidence in the statements of patients, if they are not in harmony with physical signs. I must plead in extenuation, that I never saw a woman farther removed from any taint of hysteria, and, being an illiterate woman, there could have been no cramming up of symptoms from books. The strongest points in her story were the arrest of menstruation for eight months, and the very complete narration of the phenomena of labor, and on these points I had corroboration of her statement.

This singular imitation of the process of labor is a striking feature in most of the cases in which an ectopic gestation is carried beyond the normal period, and seems thus to indicate the conclusion that the initial mechanism of labor is not in the uterus, as generally supposed. It was first noticed in 1652 (Phil. Trans., Vol. V.) by Vassal and has been constantly alluded to by writers recording such cases, one case being given in the "Memoirs of the Medical Society of London" in 1789, when the spurious labor went on eight days, and then abdominal section was performed. The child was dead, and, as the placenta was unfortunately removed at the same time, the patient died in four hours. Campbell gives a great deal of curious information on this point as on others, and he especially emphasizes the records of cases where there has been a "show" and separation of secundines. (For once [p. 120] Campbell indulges in a piece of criticism based on a wholesome scepticism concerning the utterance of "a veteran practitioner," who I suspect was Hamilton. He says: "Those of the profession who have been led to bestow some share of attention on the subject under consideration, will excuse me from entering my dissent against the dictum of a late veteran practitioner, who imagined that there was something so characteristic in the

mere moans of the patient, that it would be sufficient for the medical attendant to hear them once, to enable him to pronounce any future case to be one of a certain variety of extra-uterine gestation.") It is also worthy of note that he gives a long list of records where it is especially noted that up to the occurrence of the false labor no trouble of any kind was encountered by which the patient was led to suspect that there was anything wrong. The gestation in the case which I am now discussing, which led me astray as much as anything, was the suppression of menstruation, and the digest of the records made by Campbell on this point is worth quoting at length to show how little trust can be placed in histories. "In many instances of the different varieties of misplaced gestation, the catamenia are suspended; frequently, however, they appear regularly in each of the early months; in some cases they flow at uncertain periods; and in other examples they are either profuse, or limited in quantity. In many cases, at an uncertain period of gestation, we have hemorrhage, uterine effusions, the extrusion of coagula, of bodies which resemble moles, or portions of the placenta. These appearances have occasionally led to the belief that the patient has actually aborted, so that the ovum was originally not extra- but intra-uterine, and had escaped through a rent in the uterus into the peritoneal cavity, the extruded body in either case being viewed as the placenta. Cases attended with much uterine excitement, whether arising from unusual exertion, or some external injury, are the most likely to be accompanied by these latter phenomena" (p. 104).

The weak points in the story of my case were those I did not attach sufficient weight to, and they were those alone on which we ought to place any reliance whatever. They are that she had no "show" during the false labor, and that her size did not diminish after it. Having now almost exhausted, I believe, the literature of the subject, I am satisfied that these two circumstances are invariable in extra-uterine gestation which has gone past the period. The first is due to the general excitement and congestion of the organs involved, specially to the enlargement of the uterus, which is always present to some extent; and the second, to the absorption of the liquor amnii after the death of the child. The complete arrest of menstruation during the period corresponding to

normal pregnancy is far from being a constant condition. But even though it were like its accompanying signs, such as enlargement of the breasts, darkening of the areolæ, increase of Montgomery's tubercles, malaise, vomiting, etc., it would help us to do little more than suspect a pregnancy. Sometimes there is metrorrhagia, due to the large size and empty condition of the uterus, a symptom which would incline us to the diagnosis of uterine myoma. Parry has fully investigated this point in the numerous records he has collected, and tells us that "the uterus, except in some rare instances, undergoes striking alterations, both in its structure and volume. Its development has been found to vary from twice the size of an unimpregnated organ to the volume which it is known to attain when gestation is four months advanced."

After the death of the child, auscultatory signs cannot, of course, be made available; though in one of my cases, where the child was clearly dead, the placental sound was heard at my first visit, but had disappeared entirely at my second, ten hours afterward—a set of signs which tended to confirm my diagnosis.

The invariable condition of the uterus in extra-uterine pregnancy, whether before or after the death of the child, is that it is intimately associated with the tumor, generally in front of it, movable to a limited extent, always enlarged before the death of the child, and remaining so afterward if the placenta be attached, as it generally is, to the posterior surface of the fundus. The most important point is that the cervix is always quite open—in my cases almost admitting the finger. Under such circumstances, if a fœtal heart is audible, the case is clear. If not, then the character of the tumor must be taken carefully into account. If the case is seen soon after the death of the child, the tumor will be soft, more or less obscure ballottement will be felt in it, and possibly a part of the child may be made out by rectal, vaginal, or supra-pelvic examination. It is at this stage the great difficulties in diagnosis are met with, and Parry has so well summed this up that I cannot do better than reproduce what he has said on the subject:

"If the patient is not seen until after the death of the child, the diagnosis of an extra-uterine pregnancy may be very difficult. Many years may have intervened before the woman

comes under notice. Of course, if the cyst has opened into the bladder, bowels, or vagina, or a fistula has formed through the abdominal walls, there will be little or no trouble in arriving at a correct conclusion. Difficulty will arise only when the cyst has not ruptured, or, having opened into the bladder or into the rectum out of reach, it has not discharged any of its solid contents. Under these circumstances, a correct conclusion can be reached by carefully sifting the clinical history. No point is too minute for examination. As a rule, it will be found that all such women have a firm conviction that they were pregnant when the abdominal tumor made its appearance. Though more than a score of years may have passed, they will not have abandoned the idea that they still carry a child somewhere in the abdominal cavity. Such women will nearly always give the history of labor at or near term, attended with uterine hemorrhage, and followed by the secretion of milk; after which they will assert that the abdomen diminished in size, and that this diminution steadily continued until the tumor reached the dimensions presented when the patient comes under observation. This association of phenomena is very characteristic, and when they are all present, erratic gestation should always be suspected. The diminution in the size of the abdomen after labor is a most important symptom."

After the absorption of the liquor amnii, the character of the tumor in extra-uterine pregnancy alters very much. The uterus may become smaller and more mobile, and parts of the child may be felt, especially in the rectum, such a sign at once pointing out the nature of the case. These prominences, and likewise the "bosselures," or knobs of the hands and feet, which are often felt above the pelvis, may be closely imitated by the small nut-like cysts of small ovarian tumors, and especially by the hard irregularities of dermoid cysts. These resemblances existed in the case I have narrated above to a considerable extent, but to a very much more marked degree in another patient, where I removed both ovaries—one dermoid—but where the resemblances, fortunately, did not lead me astray. If the cyst be packed down in the pelvis, the deception may be great, and nothing but an exploratory incision will clear up the case. I would strongly recommend that, in such cases, the aspirator should not be used. In a joint, or in

the pleura, where the conditions between which diagnosis has to be made are limited in number, this instrument is doubtless of great use, as it is for treatment as well. But in the abdomen and pelvis it is very different. The aspirator may tell you a tumor contains serum, blood, or pus, but that helps you but little as to the seat of the disease, and nothing at all as to its treatment. Besides, the risk of the aspirator is great, quite as great as the risk of an abdominal section. The use of the aspirator in my special line of practice is therefore diminishing, has almost disappeared, and in all cases of abdominal tumor where there seems a reasonable prospect of doing good to the patient, I open the abdomen and make out the condition. I have never had to regret this practice, and I very often have had reason to be pleased with its results. Parry's evidence on this point is so strong and important that I quote it at length to strengthen my position:

"In cases of doubt, the fœtus being dead, the trocar has been used to draw off some liquor amnii in order to confirm the diagnosis. Unless it has been decided to operate immediately for the removal of the fœtus, the use of the trocar is utterly unjustifiable. A few, but very few, women have long survived its use. Mr. Jonathan Hutchinson, in a clinical lecture upon this subject, says that this practice 'is in itself attended by great danger, nor shall I deal honestly with you or myself if I do not candidly admit that, with due care and patience, I do not think that paracentesis ought to be necessary in a case of fœtal tumor simulating ovarian dropsy.' Mr. Hutchinson reached this conclusion after having been so unfortunate as to see fatal peritonitis follow the use of the trocar in his hands. Dr. Cardeza's patient was tapped after consultation with Dr. W. L. Atlee, of Philadelphia, on November 19th, and the latter gentleman performed gastrotomy five days later. As soon as the cyst was opened, 'there was a rush of offensive gas.' Jordan used the aspirator for diagnostic purposes, the woman, there is every reason to believe, having no bad symptom at the time. She was given chloroform, the puncture made, and two hours after 'complete collapse came on.' Speaking of the use of the aspirator under these circumstances, Dr. Jordan remarks: 'The doubts cast on my diagnosis, and the variety of opposing views in regard to the nature of the case, which unfortunately resulted in the

use of the aspirator, were nearly the cause of the patient's death.' "

Slow-growing cancer of an ovary or in the neighborhood of the uterus, especially behind it, might be difficult to diagnose by physical signs from extra-uterine pregnancy of long standing, but the history would here greatly help us. The increase would probably be steady, and if a rapid accession to the growth took place, a temperature chart would settle the difficulty; for the only condition which could induce rapid increase of the cyst of an extra-uterine pregnancy is suppuration, and this would tell its story on the chart in lines that could not be mistaken. The history of the case would probably help, but it might just as easily lead one astray, as in the case I have detailed. I once saw a very eminent obstetric physician attack an abdominal tumor which, from the history mainly, he had assured himself was an instance of ectopic gestation gone beyond the full time. He asked me to examine the case and give an opinion, but as the physical signs were in no way distinctive from those of a large uterine tumor, certainly not myomatous, I said I should depend more upon the exploratory incision than upon the history. The event proved that the history was entirely fallacious, for the tumor was a mass of cancer of the omentum, adherent to and involving everything.

After the liquor amnii has been absorbed, and the contents of the ovum cyst consolidated, the relations of the mass to the uterus and the other pelvic viscera are made so close by the placental connections, that the physical signs never can be very clear, and therefore, alternative diagnosis of fibrocystic tumor of the uterus must be the refuge of uncertainty. (Writers of "library papers" and other inexperienced persons talk so lightly of diagnosis in pelvic and abdominal troubles, and so assuredly of the accuracy of their diagnosis, that I am disposed to ask those who are passing through those stages of their professional existence to read the following extracts on the subject of the diagnosis of ectopic gestation:

"Although from the careful perusal of numerous histories of cases of this nature, some degree of facility of distinguishing their presence may be acquired after a certain period of their duration, and of deciding even, in occasional instances, on the particular variety of such pregnancies, yet assuredly

every practitioner who has attentively studied the subject, must admit the distinction to be a task of no ordinary difficulty." [Parry.]

"Telle est l'obscurité du diagnostic, après l'exploration du col uterin, que les Baudelocque, les Osiander, les Dubois, etc., n'ont jamais osé, au milieu des incertitudes qu'il laisse, entreprendre, au terme de neuf mois, l'extraction de l'enfant." ["Archives Génér.," Vol. xxvii., p. 211."—Lesouef.]) But an exploratory incision will clear up all doubt as to the diagnosis, and at the same time it will put the operator on the road to the proper method of treatment.

After the diagnosis of a case of extra-uterine pregnancy has been satisfactorily determined, the question arises, What is to be done with it? If the child is still alive and near the full term, I believe it to be our duty to operate. If the child is dead, the propriety of operating seems to me quite evident, though it has been disputed by so eminent an authority as Mr. Jonathan Hutchinson. Of course no strict rule can be laid down, and each case must be decided on its own merits; but the records of surgery are so full of instances of the risks which such cases have to run when suppuration of the sac occurs, as it almost always does some time or other, that I think we are in most instances justified in operating. Moreover, the surgical principles on which the operation is to be conducted are now so well established, and its results are so good, that the opponents of the operation seem to me to be in a very illogical position if they still continue to advocate certain other surgical proceedings, of which the results are notoriously bad.

Of late years much discussion has turned on various forms of treatment designed to obviate the necessity for surgical operations, and in the arguments used to support them, an altogether new, and I venture to think, a very immoral element has been introduced. It is to the effect that if the child is alive the proper thing is to kill it, in the belief that the infant's sacrifice is the mother's safety. I am no theologian and this is hardly the place for a discussion on morals, but I am bound to say that this seems a most mysterious kind of belief, and it would put legitimate practitioners of medicine quite on a level with abortion-mongers and reckless craniotomists. Certainly I will have none of it, the more that the

men who urge it happen, commonly enough, to be notoriously unfortunate in all their surgical efforts, belonging generally to the hybrid class of obstetric physicians.

If the death of the child did bring the mother safety, something might be said for the proceeding, but nature kills the child in the vast majority of instances of ectopic gestation, as we have seen, and safety is thereby brought to a mere fraction of the cases, as Parry has proved. Puncturing the ovum sac with needles, medicated or galvanic, is therefore an immoral and dangerous proceeding, which ought to have professional condemnation. Parry is of opinion that all measures that necessitate wounding the cyst without removing the child are not without danger to the woman, and that the question to determine is whether the risks of such a therapeutic measure, though they may be grave, may not be less than those which follow when the accident is abandoned to nature. This is a fair way of stating the case, certainly at the time Parry wrote (1874) it was a very advanced kind of statement, but now we can speak with far greater certainty. He himself says in this very passage that future experience must settle the question. I venture to think that my own experience settles the question in favor of surgical interference in ectopic gestation at the time of primary rupture. I think there is no appeal against the decision to cut down and tie the bleeding point. No acupuncture, simple or medicated, and no electrolytic charlatanry will save a woman who has a vessel bleeding into the peritoneal cavity. If the child survives that rupture it has a legal and a moral right to its life, and ought not to be deliberately killed, as has been done by Dr. Braxton Hicks and Dr. Aveling. Parry says of this case, narrated by the former authority:

"The observation of Dr. Hicks is more important, since it involves less speculation. This case has already been alluded to. The patient died, when four months pregnant, of internal hemorrhage, the result of an attempt to destroy the fœtus by puncturing it with a trocar. About a fortnight before her death she had some symptoms of rupture, but these were not distinctive. At the post-mortem the cyst, which had originally contained the ovum, was found ruptured; and outside of it, having formed new connections, was the perfect ovum with its placental attachments, on the side opposite

the opening into the cyst and to the posterior surface of the uterus."

In Dr. Aveling's case both mother and child had survived the primary rupture, and the ovum was going on developing in the broad ligament. Beyond the fact that an ectopic gestation was diagnosed by Dr. Aveling, and was even made clear by him to Mr. Spencer Wells, there was no reason apparent for interfering. If the case had been carefully tended up to the viable period a living child might have been removed. Instead of this the child was killed by galvanism, and that seems to me a wrong thing—a far more immoral thing even than "spaying."

One of the most recent cases in which electricity has been used for the purpose of dealing with an ectopic gestation is that reported by Dr. Buckmaster, of Brooklyn, in the "Medical News," July 21st, 1888, and this case is so characteristic that it may serve as a type against which criticism can be easily and justly directed. Dr. Buckmaster asks three questions in connection with his case, of which the first is: "Was the diagnosis of extra-uterine pregnancy warrantable?" and in reply there can be no doubt at all, for the description that he gives of the accident which occurred to the patient about the ninth week of pregnancy is essentially characteristic of tubal rupture: "She suddenly felt a violent pain in the 'pit of the stomach,' heard a ringing noise in the ears, and fainted. She lay on the floor groaning, and did not have strength enough to call loudly for assistance. She was found in this condition by her husband and removed to her bed. It is said that her face was very pale, and she fainted at each attempt to sit up. She was very thirsty, and 'thought the doctor cruel' in that he did not permit her to drink all the water she desired."

The second question is: "Was the child living when the electricity was first applied?" and then Dr. Buckmaster gives a categorical reply in the affirmative, when really it is a matter open to the greatest suspicion. I think in all probability from the details of the case given that the patient was suffering from a hæmatocele of the broad ligament, due to the rupture of the tubal pregnancy, to such an extent that the ovum had been destroyed, and that if she had been left alone the absorption of blood would have taken place without the violent

influence of the electric current, just as generally follows when the electric current is not applied. Further, he describes the tumor as not only decreasing in size but changing in character, losing its elastic feeling on account of the absorption of the fluid contents. But supposing the child had not died, his third question comes up for discussion: "Is the uninterrupted current the best means for destroying the fœtus?" and to this I reply, What right had Dr. Buckmaster to destroy the child at all? There can be no doubt from the very clear description given that the pregnancy was in the broad ligament. "An ill-defined mass, elastic to the touch, was distinctly traceable on the right side. Vaginal examination showed that the uterus was crowded forward toward the pubes, and that it was somewhat enlarged and softened. The sac of Douglas was occupied by an elastic mass in which fluctuation could be detected, and which felt not unlike a small ovarian cyst, and seemed to be part of the tumor felt in the right iliac region, from which the uterus appeared free."

After carefully considering the different methods for destroying the fœtus, none of which seemed altogether satisfactory, Dr. Buckmaster continued to use the galvanic current uninterruptedly, but he gives no justification whatever for his determination to destroy the child. All the severe symptoms had disappeared, the patient was suffering from nothing but slight discomfort and the unfortunate fact that Dr. Buckmaster had diagnosed an ectopic gestation. If the case had been left alone a living child might have been the result, for there can be no doubt whatever that it was an extra-peritoneal pregnancy, which, if there really was a living child, would have gone on precisely in the way to be described hereafter. Then, finally, Dr. Buckmaster tells us that three months after the electric treatment the patient still had left a hard mass, which could be felt on making a vaginal examination, and that there was a slight tenderness about it, in fact the physical condition of the patient was precisely that in which he found her, except that the mass had diminished in size, it still remained there, a source of danger, and in all probability will some day suppurate. Certainly three months is far too short a period on which to base any conclusions for the safety of the treatment, even supposing that he achieved the result which he says he desired in killing the child. My own belief is that

he did not do so, for the method which he employed is one which could not by any means be applied with safety to the child, and the strength of the current was not such as is likely to be fatal to anything at all.

In Dr. Buckmaster's papers there are two other points upon which some criticism might be directed. In the first place he credits Dr. T. G. Thomas with the belief that the electrolytic treatment has these great advantages: if any error of diagnosis has been made, it will do no harm, and if the diagnosis be correct, experience proves it to be sufficient.

My answer to such statements is this, that it is by no means clear from experience which we have had in this method that the electric current is without harm, whether the diagnosis be correct or not, and it is equally without proof that it is sufficient to produce the effect desired. Further, Dr. Buckmaster says on his own account, that cases will undoubtedly appear, as the literature of the subject expands, in which, after cutting into the abdomen, it will be found impossible to complete the operation. I say from my own experience that this is absolutely inaccurate, it may be impossible for the immediate operator in certain cases to complete the operation, but the rule ought to be that all such operations should be completed, and any man who has such want of pluck and skill as to stop in the middle of one of them ought not to attempt them. They can all be completed. The second point is that Dr. Buckmaster says that "it has been claimed recently that the placenta continues to grow after the death of the fœtus, but as we have seen no corroborative evidence, it is not worthy consideration at present."

As I am responsible for having first made a statement that I had seen the placenta growing after the fœtus had clearly been dead for some time, let me here draw the attention of Dr. Buckmaster and others to the evidence upon which the statement is based.

In case number six the rupture had occurred apparently in the tenth or eleventh week of gestation, and the placenta was lying in the midst of a quantity of clots, as a round mass the size of a cricket ball, for the most part in the wall of the tube; for when the tumor was removed the placenta was still adherent to part of its inner surface, and the pelvic mass was intact. On slitting it open, the ovum cavity was found to con-

tain about a dessertspoonful of liquor amnii, but there was no trace of fœtus at all.

As we have very frequent experience of this kind of incident—the growth of a large placenta, embracing a small ovum cavity without any, or with only very slight, trace of a fœtus, in the so-called uterine "moles"—we have no reason to do other than expect that they will occasionally occur in tubal pregnancy. As a matter of fact such was the state of matters in this case.

In case 19, when the fœtus was found it was only about 2½ inches long, and had evidently been dead for some considerable time, for it was partly digested; whereas the placenta had grown to be quite as large as that of an intra-uterine fœtation of four months, and it had been forming adhesions to intestine and omentum, giving rise to recurrent hemorrhages, for which the operation had ultimately to be performed. Similar appearances occurred also in cases 24, 30, 32, and 37. At the meeting of the Obstetrical Society, at which Dr. Champneys read his case, Mr. Thornton gave testimony to the same conclusion, and in the first edition of the "Manual of Gynæcology," by Hart and Barbour, published in 1882, there is the following evidence on this important question: "Case of extra-uterine gestation, with death of the fœtus, but continued growth of the placenta, which led to fatal hemorrhage. A. B., æt. 24, had passed two periods without menstruating, and thought herself pregnant; three months ago she began to have irregular hemorrhages three times a month, and in considerable quantity. The tumor was found in the pelvis, the vagina being compressed against the pubis, the cervix reaching about the rim, and the bladder displaced into the abdomen. The tumor was as large as a uterine pregnancy of 4½ months. After a puncture of the cyst with an aspirator needle the patient died with symptoms of internal hemorrhage, and on a post-mortem examination, by freezing the pelvis and cutting sections, the uterus was found to be 5¾ inches long, the fundus being 5 inches above the symphysis, and the cervix so drawn up that the fornices are obliterated. The gestation sac lay in the pouch of Douglas, and was chiefly occupied by the placenta. The cavity of the amnion contained but little fluid, and the fœtus was about the size of a three months' pregnancy."

The continued growth of the placenta after the fœtus had died had led to fatal hemorrhage.

In looking over the records of cases which have gone beyond the full period of gestation, I find numerous illustrations which cannot be other than the growth of the placenta after the death of the child. No emphasis in any case is laid upon this fact, but the descriptions completely establish it. In a case mentioned by the first Mr. Samuel Hey, of Leeds, the patient went over the nine months with a false labor, and the child died. Three months afterward the mother succumbed from the sufferings involved in the carriage of the ectopic gestation. The child was found to be fully formed, and showed no marks of decomposition. As the child had attained a size so unusual as to weigh nearly two pounds and a half, the cyst was supposed to be the right Fallopian tube, but the description makes it perfectly clear that it was the right broad ligament, together with the tube. The placenta in this case must have grown greatly after the death of the child.

Some of the facts which have been recently recorded in the application of electricity for the treatment of ectopic gestation are positively ghastly, as illustrated in the paper by Dr. Matthews Duncan in the Bartholomew's Hospital Reports for 1883.

Electricity was first tried in the form of an induced current as strong as the faradic coil in a Croxeter's combined battery could give. A carbon disc electrode in connection with the positive pole was placed over the tumor on the left side, and a gum-elastic electrode, with a nickel-plated end, was passed into the vagina toward the left side and connected with the negative pole. A current was alternately passed and withheld during periods of two seconds for about a minute and a half. A continuous current of forty modified Leclanché elements was then passed for a space of six minutes, producing slight vesication of the skin, and a rough, dried surface in the vagina. The fœtal heart was heard beating the same evening. On the following day two grains of morphia were injected into the amniotic cavity. An hour afterward the mother began to feel drowsy and her pupils became slightly contracted. It was thought advisable to draw off the liquor amnii, which was done through the abdominal wall by aspiration, eight ounces being removed. The fœtal heart still continuing to

beat, Dr. Duncan, five days later, injected ¼ gr. of morphia into the body of the fœtus, to the depth of two inches, at the spot where the fœtal heart was heard plainest. The operation was twice repeated at intervals of two days, but without the desired result. It was decided now to try and destroy the fœtus by galvano-puncture. Two insulated electrolysis needles were passed into the tumor for an inch and a half and connected with the negative pole of a battery composed of modified Leclanché elements, a carbon disc-shaped electrode connected with the positive pole being applied over the tumor externally. A current from forty cells of the battery was passed for six minutes with occasional interruptions. After the operation the fœtal heart could still be head beating, but more slowly. Four days later Dr. Duncan, having heard the fœtal pulsation, drew off the liquor amnii, with the aspirator, and then injected ♏ xiij. of equal parts of water and liq. morph. hypod. into the fœtus just over where the heart was heard. After this the fœtal heart could not be heard.

The patient died two days subsequently. At the autopsy, twenty-six hours after death, the contents of the cyst were found very fœtid, and the soft parts of the fœtus itself were for the most part as if completely macerated, the bones being exposed. Almost all the internal organs were diffused in the surrounding fluid, or were diffluent. The heart was scarcely recognizable.

Such a record is positively discreditable to the art we practice, a series of ineffectual experiments were tried upon this poor mother and child, one after another, involving fearful suffering and finally double death, when probably both lives might have been saved by following the ordinary rules of surgical proceedings.

If the ovum perishes between the period of primary rupture and the viable period and becomes a source of danger it ought to be removed, but if it can be nursed through the time till the end of gestation it ought to be saved by abdominal section. If the patient discovers herself only after the child is beyond the gestation period and dead, it ought to be removed, for it is a source of perpetual risk. Quiescent lithopedia are far too rare, and suppurating ovum sacs far too common and far too fatal for us to recommend such a risk to our patient. Parry sums up the question very well in the following passage:

"After the death of the fœtus, and the restoration of the normal condition of the system, the retention of an extra-uterine fœtus is not incompatible with a long and useful life, but a woman is never free from danger while she is carrying an encysted child. Violent exercise, injuries, blows, strainings, and similar mechanical irritations may be the exciting cause of inflammation of the sac at any time. Hence, violent pain, with fever and evidences of inflammation following these, always demand a cautious prognosis.

"Depressing diseases, as any of the continued fevers, or local affections which introduce a profoundly typhoid condition, endanger the woman by impairing the nutrition of the cyst, and leading to destructive inflammation."

The earliest case of abdominal section for ectopic gestation which has been found upon record, is that of Primerose, who operated in October, 1594. The history of this patient has become classical. She was twice pregnant with extra-uterine children—first in 1591, and again some time before 1594. The cyst of the first child opened spontaneously through the abdominal wall. The fistula was enlarged, and the child extracted by Jacob Noierus, a surgeon. This operation proving successful, Primerose removed the second child by abdominal section two months later. It is easy to imagine how he was led to perform the second and more hazardous operation. Felix Platerus reported another successful case only three years later. After this we have found no indication that the operation was performed for more than a century. In 1714 Calvo reported a case in France, and in 1764 Bard another in America. (Parry.)

Mr. John Bard was a surgeon in New York, and no one is known to have operateed in that country before him. The patient was the wife of a mason, and the operation was performed several years before it was published, for Mr. Bard communicated an account of it to Dr. Fothergill, in a letter which was dated on the 25th of December, 1759.

On January 14th, 1791, this operation was performed in America for the second time, the subject of it being a Mrs. Cocke, the wife of a Virginia planter. The operation, which was done by Dr. William Baynham, a country physician, was entirely successful. The same gentleman operated with the same happy result upon a negro slave on February 6th, 1799.

This was the fourth American abdominal section for the removal of an extra-uterine fœtus. The third one was performed by Mr. Knight, and communicated to the celebrated Dr. Lettsom, by Dr. Mease, of Philadelphia, and published in 1795. Dr. Baynham's cases are well worth attentive study. They illustrate the intrepidity and good judgment so often displayed by the country surgeon, who, separated by long distances from his fellows, often has to act in the greatest emergencies without the counsel which he may earnestly desire. Almost a quarter of a century passed before the operation was repeated in America. On the sixth day of October, 1823, it was again performed by Dr. Wishart, likewise a country practitioner. The sixth American operation was performed on February 6th, 1846, by Dr. A. H. Stevens, of New York, a man who had all the advantages of a metropolitan experience. (Parry.)

Sprengel, in his "History of Medicine," Vol. VII., p. 290 et seq., refers to the following authorities for cases of this nature, viz., Comax, a professor at Vienna, said to be the first who operated successfully for gastrotomy; Hector and Gassarus, both of Augsburg; Soligen, who is said to have practised the operation repeatedly; C. Denys, a French physician, who relates several cases of extra-uterine conception, followed by abscesses, from which fœtuses were extracted; Runge, a surgeon of Bremen, who operated on a woman in whose abdomen a fœtus had been retained eleven years; Spaering, a Swedish physician, who, with a lancet, opened an abscess, and from the lower part of the abdomen extracted a fœtus of thirteen years' retention; Breyer, of Leipsic, and Weinhardt, both of whom operated successfully by gastrotomy; Professor Colomb, of Lyons, and Professor Josephus, of Rostock, both were unsuccessful. (Campbell.)

Parry gives a number of tables which are intended to show the mortality of extra-uterine pregnancy reaching to and going beyond term, and submitted on the one hand to abdominal section, or left to nature, on the other. But it is perfectly clear that no tables of abdominal operations of any kind are of the slightest value antecedent to the year 1878, when the whole practice of abdominal surgery was revolutionized by the final discontinuance of the clamp in ovariotomy; and further, it is quite clear that the heterogeneous collection of cases of which rarely more than two are contributed to the list by

the same operator, can have little or no value. In turning back over the records of the cases where the details are given, the great bulk of them have been operated upon when the patients were too far gone in illness, the result of accidental complications or suppuration of the sac, to give the collection any value whatever. The following are his conclusions: Of thirty cases in which gastrotomy was performed, or the breach dilated, twenty-eight patients recovered. In twelve cases of gastrotomy performed after the suppurative process was well advanced, ten of the operations were successful. Of nine women operated on, however, during the existence of fœtal life or soon after its extinction, the whole died.

If these conditions had to be accepted, there would be an end of the discussion concerning the saving of the child. I, for one, would say no more about it, and willingly would adopt some means of destroying the fœtus; or I would watch till it died, and then, after waiting awhile, I should remove it. Parry seems to have been greatly impressed with the belief that the fatality attending the removal of living children was due to the "puerperal state," and therefore he advised waiting till the child had been dead some time. In fact, he divided the operations into "primary" and "secondary" on this principle—a most mistaken one. That puerperal women are especially susceptible to surgical influences is true enough; but our recent experiences make me believe that it is only true that they are specially susceptible to the influences of bad surgery and unskilful operators. If, therefore, we have a proceeding based on sound principles and a skilful operator, I believe the puerperal woman has no more to fear from an operation than any others. My own experience in the Cæsarean operation and in the modern methods of amputating a pregnant uterus convinces me that this is so. I never succeeded in getting a woman through a Cæsarean section, and I concluded that it was the puerperal influence. But I know now that this conclusion was nonsense. As soon as I began to amputate the uterus all my patients recovered, and recovered easily, just as ovariotomies recover. I used to do the Cæsarean section merely to save the child, now I amputate the pregnant uterus to save both mother and child, and therefore I begin to look upon a man who does craniotomy as a person worthy of suspicion.

If this revolution has been effected about one puerperal operation, why may not the basis of scepticism be applied to Dr. Parry's tables and their conclusions? Further objections may be urged against them. First of all, the figures are too small for any just conclusion. Then the conditions of individual cases, when unearthed, are so dissimilar that they cannot with any justice be slumped together in tabular form. The great majority of the "primary" cases were operated upon "in extremis," while the secondary cases had been going on in chronic form, and were operated on by specially experienced men. Generally speaking the cases of "primary" operations are only surgical curiosities of a somewhat horrible kind, and of no value whatever. Indeed, Dr. Parry practically admits all this about his tables, for he says (page 223), of the 62 cases tabulated in what he calls "primary operations" were performed for the removal of extra-uterine children, "30 lived and 32 died, a mortality of 51.61 per cent. It is doubtful, however, if this can be accepted as the true mortality after gastrotomy. This result is to be compared with that of the third table, which shows approximatively the mortality of extra-uterine pregnancy left to nature, or, to speak more correctly, allowed to progress without operative interference until nature had pointed out the way in which she intended to effect elimination by forming openings either through the abdominal walls, bowels, vagina, or bladder. Of these women, 52.65 per cent perished, a mortality of only 1 per cent in favor of gastrotomy. This is certainly a very poor showing for surgical interference in this unhappy accident."

The great, and a very important qualification of this last statistical statement is entirely overlooked by Dr. Parry, and yet it is rendered perfectly clear throughout the whole of his writings on the subject, that these cases are only the remnants, the mere survivals of a vast number who died during the processes of suppuration, whereas a table of gastrotomies for living children, or children recently dead, represents no such residuum. To the mortality of the cases left to themselves there must of course be added a large number of those who died when their condition as recognized was beyond remedy, and this number I fancy vastly outruns the number of the residuum.

Here I may speak of the application of the terms "primary"

and "secondary" in connection with these operations, for which Dr. Parry is responsible, and in which I think he has committed a grave error. He regards as primary operations those in which the life of the child was considered in determining the time for interference, or in which the operation was performed shortly after its death, or near term; and as "secondary" operations he has placed all operations performed some time after the death of the child, and when the system of the mother had recovered to a great extent from the "puerperal condition." It seems to me that these terms are most inappropriate and ill-used, and are certain to be most misleading.

In general surgery we have the words primary and secondary operations, more particularly in relation to the amputation of the limbs, used in different senses altogether, and so engrafted in professional parlance as to have become an almost necessary part of our conversation, certainly they are a great convenience. Used, however, as Dr. Parry has proposed they should be, they would be without meaning and would necessarily cause a great deal of confusion. I would greatly prefer that if we were to speak of a primary operation for extra-uterine gestations, we should speak of the operation for arrest of the hemorrhage at the period of primary rupture. My reasons for this are that according to the ordinary meaning of the English language, abdominal section would then be certainly primary in point of date and also primary in the sense of being of greater importance, that is to say of far more frequent necessity. Abdominal section for a viable child is secondary so far as date is concerned, and it is but of very little importance in the matter of frequency. If we take the technical meaning of "primary," as in amputations, to mean operation at the time of accident, when the patient is collapsed from shock, pain, and hemorrhage, then the patient who is suffering from collapse as the result of the violent pain and hemorrhage which occurs at the primary rupture is surely in a condition much more resembling the state of the patient who has to submit to a primary amputation than anything else we can conceive. Most of my patients have been so, as much as if they had been cases of smashed knee-joint, and if left alone these cases must surely die. On the contrary, the women from whom I have removed viable children, or children dead

by reason of having passed the ordinary period of gestation, have far more resembled cases of amputation for disease, and surely they are secondary operations in the technical sense. A further argument against the introduction of these terms in the relation proposed by Dr. Parry, is that they would practically be determined only by saving the life of the child, and though this must be, as strongly argued by Dr. Meadows, the vital element in the further consideration of such an operation, it cannot be the chief element. Finally, by the adoption of these terms and by the argument he urges for their use, Dr. Parry would practically close the door against further advance in the possibility of saving the child: he says, "Notwithstanding the possibility of realizing this happy result, and even of saving both mother and child, as has been done a few times, the primary operation cannot be too emphatically condemned."

I cannot admit such a conclusion for a moment, for the material upon which he has based it is made up of such discordant elements, every one of which requires special qualification, that it is quite impossible to submit it to a satisfactory investigation. But even if we admitted his premisses, his argument after all is based on a nine-per-cent difference only against an operation which has saved child as well as mother; and this even is to be qualified, as I have said, by the mortality having been influenced by unintelligent delay and a vast amount of unscientific instrumentation. One of his cases was operated upon (unsuccessfully of course) after having been in false labor for over a week; and another (equally, of course, unsuccessful) after most strenuous efforts had been made for a whole day to deliver the woman by the forceps.

I therefore advocate the principle of saving a child who has survived the catastrophe of the primary rupture of the tube by being extruded into the broad ligament. If its existence is recognized during its life, the mother ought to be carefully guarded and watched till the false labor sets in, just as we watch a case for puerperal hysterectomy and seize the onset of labor or its early stage, as the most favorable time for both mother and child. From this point of view, therefore, neither the time selected for the operation nor the details of the proceeding will be influenced save by two considerations, not to operate before the child is likely to be viable, provided

the delay necessary does not prejudice the mother, and not to delay at all after the death of the child.

I specially lay this down for the purpose, among others, of excluding all operations for the removal of the child by vaginal section.

Dr. Herman has collected a series of twenty-three cases of vaginal section, with fourteen maternal recoveries and only one child saved. I have unearthed a few more, but this kind of research is really of little value, for when the details of the case come to be perused it is evident that there are so many points of discrepancy, that it is the merest nonsense to argue from such a collection to any general, still more to any particular conclusion.

That vaginal section is an unsatisfactory method for the purpose of saving the child is certain from the constantly recorded difficulties in getting the child out, and only two cases are known where the child has been extracted living, only two cases where it ultimately survived. The mortality of the collection is over 60 per cent, but this forms an argument not half so strong as the records of the tearing of the parts which was revealed at the post-mortem examinations, and the concealed hemorrhage, which was nearly always the ascertained cause of death. My own experience of one case is quite sufficient, and I shall never, under any circumstances whatever, attack a sub-peritoneal pregnancy from the vagina. I give that case in detail as published in the "Medical Times and Gazette" for 1873.

"On July 16th, 1872, I was asked by Dr. Call Weddell, of Bloomsbury, to see in consultation with him Mrs. T., aged 32, who had been suffering for some time from anomalous and perplexing symptoms. She had had one child, nine years previous to the above date, and for some months had been under the impression that she was again pregnant. For some days before I saw her she had been suffering from feverish symptoms, and her condition had evidently become very critical. A crescentiform tumor occupied the pelvis and iliac fossa, giving no special indication of its nature from above, save that at one spot less than half an inch in diameter, and situated about an inch below the umbilicus, there was a distinct bruit, which was much intensified when the pressure of the stethoscope was increased. Vaginal examination revealed a tumor

behind the uterus, occupying the whole available space, immovable, and with a peculiar boggy feeling to the touch. The uterus was open, four inches in internal measurement, and presenting very much the characters as if a miscarriage at the third or fourth month had recently occurred. It was movable over the front of the tumor to a limited extent, the fundus being anteverted and readily felt over the pubis. On examination by the rectum I felt what I believed to be the knee of a child and the edge of the placenta.

"On July 17th the condition of the patient was manifestly much worse, and admitted of no further delay. We therefore placed her under chloroform, and I passed the needle of an aspirator into the retro-uterine tumor and evacuated a few ounces of fluid, which was undoubtedly liquor amnii. The diagnosis being thus placed beyond doubt, I followed the needle with a knife, and came at once on the knee of a fœtus. I enlarged the incision, and delivered a fœtus of about the eighth month, which had evidently been dead for some time. As soon as the child was born I passed my hand through the aperture and searched for the placenta, which I found situated in front. I also found that the cyst had been ruptured above, and that some intestine was extruded into the sac. There was no difficulty in removing the placenta, and no hemorrhage seemed to result from its separation. It weighed when put together nearly three pounds, and was very hard and fleshy. The patient rallied from the chloroform, but sank in a few hours."

I am indebted to Drs. Sawyer and Weddell for notes of the post-mortem examination:

"There was a considerable amount of clotted blood among the coils of the intestines. The uterus was enlarged and displaced, being carried so much to the left that its right margin corresponded to the middle line, and so much forward that its fundus projected over the symphysis pubis. The cyst was large enough to contain two clenched fists, and was situated between the uterus and vagina in front, and the rectum and sacrum behind, the greater portion of it being to the right of the middle line. The cyst was extensively ruptured inferiorly, and the small intestines freely protruded into its cavity."

"The lessons derived from this case and its failure are three: First, that we should not delay interference after the

child has come to the term or after it is dead; second, vaginal section should invariably give place to abdominal section, the latter being more scientific and less risky; and third, that the placenta should not be interfered with, but should be left to separate. I have profited by these lessons, and have since been able to operate on a case successfully."

In this case the temptation to remove the child from the vagina was very great, for it felt just as if it were separated from the fingers by the vaginal mucous membrane, and indeed there was little else. It felt as if a mere notch in the mucous membrane, and the child would come, and it is clear from the records that most of the operators have yielded to the temptation in similar conditions. But to do so is wrong, if for two reasons only. In the first place, as the placental relations are always chiefly pelvic, generally wholly so, the child cannot be dragged out without tearing tissues in which large sinuses have been abnormally developed, and through structures, as they are unyielding, a child can be dragged only with much damage to the tissues, and likelihood of killing the fœtus; then, if there be torn vessels bleeding it is simply hopeless to expect to be able to find them and secure the bleeding points.

A case illustrating the difficulty of delivering a child under such circumstances is seen in one of the two cases known where the child lived.

A woman who had been four days in labor, and exhausted by her efforts, but in whom no os uteri could be traced, though the head of a fœtus was easily felt, was delivered by an incision five or six inches backward and downward through the posterior wall of the vagina. Liquor amnii escaped, and the hand was passed into the cyst to extract the fœtus, which, however, could not be effected, though the abdomen was compressed by an assistant; but extraction was ultimately accomplished by forceps, and although the child, when born, was asphyxiated, it was nevertheless resuscitated. The operation was attended with little hemorrhage, and scarcely any pain; and in two weeks the woman was going about, and no traces of the incision could be discovered per vaginam.—(An American case, "Medical and Surgical Review," vol. ii., p. 132.)

Opening the peritoneal cavity from the vagina is a clumsy and risky method of proceeding under any circumstances, and

while it has no advantage whatever over the suprapubic method, it possesses many disadvantages. Dr. Herman has very well summed up a series of conclusions on this subject, which I here reproduce, pointing out, of course, that in the first four he gives indications of some amount of the usual confusion as to the periods of rupture, and what happens at them. In paragraphs 5, 6, 7 he lays down fatal objections to the vaginal operation, for after the death of the fœtus, and the majority of cases will present themselves after this has happened, it is absolutely impossible to tell where the placenta is, nor is it always certain even when the child is alive. I have twice failed to discover its seat, even with my hands in the fœtal sac. I am also of opinion that the most expert accoucheurs could not accurately ascertain the presentation of an ectopic fœtus until the sac had been opened—at least I once saw a very experienced man utterly fail.

Dr. Herman's conclusions are as follows:

(1) The operation of opening an extra-uterine gestation sac by the vagina early in pregnancy, before rupture has taken place, by the cautery knife or otherwise, is a dangerous and unscientific proceeding. Abdominal section ought always to be preferred to this.

(2) Soon after rupture has taken place, when interference is called for to arrest hemorrhage, abdominal section is more likely to succeed than vaginal.

(3) When rupture has taken place, and the effusion of blood is followed by pyrexia, the indications for incision of the vagina are the same as those in hæmatocele from any other cause.

(4) At, or soon after, full term, before suppuration has taken place, there may be conditions which indicate delivery by the vagina as preferable to abdominal section. These are—

(5) When the fœtus is presenting with the head, breech, or feet, so that it can be extracted without altering its condition, and

(6) When it is quite certain, from the thinness of the structures separating the presenting part from the vaginal canal, that the placenta is not implanted on this side of the sac, and it is not certain that the placenta is not implanted on the anterior abdominal wall.

(7) If the child cannot be delivered by the vagina without being turned, abdominal section should be performed.

These conclusions may be taken as practically fatal to vaginal section.

Parry has collected a number of cases from which he draws the conclusion that about seventy-five per cent of the cases which go toward full term (that is, according to my views, of the cases which survive primary rupture, and are developed extra-peritoneally), arrive at that term, and die at or shortly after it (if not destroyed by surgical interference), the minority dying at various periods in the progress. I have not tested the evidence on which he bases his conclusions, for I do not think they are of much moment. I am inclined to think that most of the women will not present themselves till they begin to believe that, having gone past their time and the child having ceased to move, something has gone wrong. Then it will simply be a matter of relieving the mother of a risky burden. If the child is living, an effort ought, in my opinion, to be made to save it. But whether the child be living or dead, the steps of the operation will be practically the same, and the early part of the proceedings will not vary very much from the ordinary processes of any abdominal section, save in one particular—that the opening should not be made in the middle line, so as to avoid opening the peritoneum. In fact, the operation should not be an abdominal section at all, in the strict sense of the definition I have adopted. This fact has been the cause of much confusion on the part of one perverse critic, whose diatribes require no further notice or explanation.

To understand the motive of this avoidance of the ordinary incision in dealing with a case of ectopic pregnancy we must revert to the explanations already given of the process at the time of rupture, and to the views I have advanced, that all the full-term ectopic pregnancies are those which have grown in the broad ligament—extra-peritoneally. As they grow they separate the folds of the broad ligament, and finally lift the peritoneum slowly out of Douglas' pouch, off the rectum, sides and brim of the pelvis, off the posterior surface of the uterus, and off the back and sides of the lower abdominal walls as far round as a point corresponding to the cornu of the uterus on each side. The result of this is that the posterior and lateral levels of the reflections of the peritoneum are raised very

materially, while the utero-vesical pouch is uninterfered with, and it remains as a long process, like the finger of a huge glove running down in front of the gestation sac, to its normal ending on the base of the bladder. This curious re-arrangement of the peritoneum is similar to what we constantly find in cysts of the broad ligament, only the arrangement in them is less systematic, and the explanation of both is simple. The peritoneum is very easily lifted off any of the organs round which it is wrapped, if the process is slowly carried on. The growth of the ovum, therefore, easily lifts the peritoneum everywhere if the pull is direct; but when the pull comes to be indirect, as it must be the moment the top of the fundus is reached, the lifting of the peritoneum ceases, and the long tubular process is formed. As the growth of the ovum is not quite symmetrical, this tube is sometimes on one or other side and sometimes in the middle, and therefore it is that some of my operations for ectopic gestation at the full time have been abdominal sections, and some have not been. Therefore it is also that the opening in this case should be made, not central, but well to one side.

This curious lifting of the peritoneum may of course be interrupted by a secondary rupture of the sac into the peritoneum, and we may find—probably shall—that many of the minor variations which are quite well established, such as invasions of the intestines by the placenta, are due to the same cause. We may also find, what I have already indicated as a probability, that direct primary rupture into the peritoneum of a tubal pregnancy of the twelfth week, may end neither in the death of the mother nor in that of the child, but that it may go on developing in the peritoneum. I regard this as very unlikely, and as yet wholly unproven.

The lifting of the peritoneum also explains the intimate association which the fœtal sac always has with the posterior wall of the uterus.

What was, on my part, originally a pure speculation concerning the methods of origin of the relations of the peritoneum and their details, has been elevated into a series of indisputable facts by the fortunate experience by Dr. Berry Hart, of Edinburgh, of two bodies which contained ectopic pregnancies. The bodies were frozen and sections made, and these have been so carefully and elaborately described by Dr. Hart and Mr.

Carter that I cannot do better than reproduce their original observations. I must acknowledge at the same time my indebtedness to these gentlemen, and to the proprietors of the "Edinburgh Medical Journal" for permission to reproduce an admirable illustration which will assist my readers greatly in understanding the description of the parts.

"The first specimen had advanced to between the fourth and fifth month. Dr. Hart saw the patient for the first time in the Buchanan Ward of the Royal Infirmary, and found her with a tumor the size of a cocoanut in the site of the right broad ligament, and reaching from the right iliac margin to the region of the recto-vaginal space, which bulged down markedly. The uterus was displaced to the left side of a two months' pregnancy. From the history of five months' amenorrhœa, and the occasional attacks of fainting and pain during that time, there was no difficulty in coming to the conclusion that we had here to deal with an extra-uterine gestation developing between the layers of the broad ligament. Two days after, the patient collapsed markedly, evidently from rupture of the sac and loss of blood. Eight hours afterward, when she had somewhat rallied, an exploratory abdominal incision was made to see if anything could be done. Blood poured out whenever the peritoneum was opened, and on passing the fingers in, rupture deep down through the posterior lamina of the broad ligament was found, a condition which did not admit of removal of the sac, inasmuch as it had developed down between the rectum and the vagina. The incision was therefore closed, and the patient sank in about ten hours.

"At the post-mortem, which was performed by Dr. Bruce, the bony pelvis and contents were removed and frozen, and in this way the relations were preserved—an impossibility if the parts are scooped out from the pelvis in the usual way.

"The pelvis when frozen was sawn in the mesial, right sagittal lateral, and left sagittal lateral planes, so as to cut sac and uterus.

"The following points are noteworthy:

"In the mesial line the fœtus and placenta are contained in a space bounded above by the laminæ of the broad ligament, and below by the paraproctal tissue and that at the base of the broad ligament. The placenta is attached to the inner aspect of the tube and broad ligament, the fœtus lying be-

low. The vertical measurement is 4.10 cm., the transverse 8.7 cm.

"A similar section to the left of the middle line shows the enlarged uterus, and hamætoma between the peritoneum and the rectum. The rupture had occurred through the posterior lamina, and low down.

"The uterus measures 10 cm. vertically, has a well-marked decidua, and the dip of the vesico-uterine pouch is only 5 cm. from the fundus. The left Fallopian tube and ovary are intact. This specimen, therefore, shows that the gestation, primarily Fallopian, had developed between the layers of the broad ligament and into the connective tissue between the peritoneum and the rectum. It was thus, prior to its intra-peritoneal rupture, entirely extra-peritoneal (v. Plate I., Figs. 1 and 2)."

The description here given by the authors conclusively establishes the process of primary rupture into the cavity of the broad ligament, for which I have already advanced very many arguments, as the explanation of the occurrence of the sous-peritoneo-pelvienne variety of Dezeimeris. The rupture, which was the immediate cause of death, was the secondary rupture into the peritoneal cavity which I have already described as having occurred in Nonat's case, and I think that if the operator had been bold enough to carry on his proceedings, had opened the sac, and sponged it out with a styptic in the fashion that I have described (p. 498), a more satisfactory ending of the case would have been arrived at. But the unfortunate termination is, at least to some extent, compensated for by the brilliant contribution to the elucidation of the pathology of ectopic gestations, of which it has been the immediate cause.

The second specimen "was the unopened body of a female, aged 33, small and very emaciated, who was supposed to have gone a little beyond the term of normal pregnancy; but little information of any kind could be obtained, as she was destitute, with no friends. The usual appearance of a multiparous pregnancy were present, without any varicosity of the venous system.

"On delivery into the dissecting room the extremities were cut off; and the head and trunk, after a process of freezing by means of ice and salt, were cut into a series of sagittal, mesial, and lateral slabs, six in all, of about 1½ inches in thickness.

These slabs may be for convenience mentioned as 1R, 2R, 3R, and 1L, 2L, 3L, viz., the first slab on the right side, and so on.

"In the sagittal mesial section the saw passed almost exactly in the mesial plane of the body. There is nothing particular to remark about the brain and head and neck, the specimen presenting the usual appearances exhibited in sections made in this manner.

"In describing the gestation sac and its contents we shall try to avoid too minute details. The first great point to settle is the relations of the peritoneum to the sac, and it will simplify matters if we state the one broad fact brought out in the sections, viz., that the gestation is entirely extra-peritoneal, and that fœtus and placenta lie in extra-peritoneal connective tissue. The fœtal capsule and its contents, which occupy a great portion of the abdominal cavity, rise up to the upper margin of the second lumbar vertebra and extend well into the right half of the sections, pushing the intestines up and to the left. In front the tumor is separated above from the abdominal wall by the great omentum, while below its wall is formed by the uterus, behind, it is separated from the posterior abdominal wall by a double layer of peritoneum. The uterus is much enlarged, the upper surface of the fundus being on a level with the upper border of the first sacral vertebra. It was pushed over to the left side, none being found in the right outer lateral section. The peritoneum has been entirely stripped away from its posterior and the upper part of its anterior surfaces, and from the fundus bands of tissue connect it with the upper and inner surface of the fœtal sac. On the left side of the fundus a small fold of peritoneum inclosed the left Fallopion tube and left ovary.

"The left Fallopian tube passed obliquely downward from the left side of the fundus to the left iliac fossa, its fimbriated end being attached to the tumor.

"The left ovary was found below the Fallopian tube and left under surface of the capsule, and is seen in the left lateral section 3″ from the median line in the angle between the abdominal wall and the left iliac fossa. (v. Plate III., Fig. 6.) It measured $1'' \times 1\frac{3}{8} \times \frac{3}{4}$ in thickness, and was inclosed in the same fold of peritoneum with the Fallopian tube. The ovarian vessels were greatly increased in calibre. *The right Fallopian tube and ovary cannot be identified, being taken up*

with the sac. Owing to the surfaces of the peritoneum being more or less adherent, it required great care to trace its general relation. In the mesial section it will be seen to be reflected from the inner surfcae of the anterior abdominal wall on the front of the uterus at the upper level of the pubes; the bladder lies below the lines of reflection, and is deficient of a serous covering. The front of the enlarged uterus is covered for a short distance, and the peritoneum is there reflected on the fœtal capsule, this portion of its surface being rough and deficient of any serous investment. The fœtal capsule is seen to be enveloped in front and above, and behind the membrane is reflected on to the rectum at about the level of the fourth sacral vertebra. On the left side the peritoneum passes from the left iliac fossa and covers a small portion of the upper part of the body of the uterus, and from this is reflected on to the capsule, forming a fold in which the left Fallopian tube and ovary are inclosed. The relations of the peritoneum to the other organs do not require any special remark. On the right side the peritoneum is lifted up. The fœtus has thus developed beneath the peritoneum, elevating the folds of the broad ligament after distending them, and in its upward growth stripping the peritoneum up from the right side of the anterior abdominal wall for a distance of 7⅝ in. above the pelvic brim. Posteriorly the deepest portions of the pouch of Douglas lie at the level of the fourth and fifth sacral vertebræ. The fœtal capsule and its contents are found to extend into the hypogastric, umbilical, lumbar, and right inguinal regions.

"The sac can be studied in all its relations in the sections. Microscopical examination of its walls were made at various points, viz., at its uppermost portion, and also at the anterior abdominal wall below the peritoneal reflection (Plate II., Fig. 4).

"In the former part there was peritoneum and unstriped muscle, showing the Fallopian-tube origin; in the latter, connective tissue. The capsule was thus formed by connective tissue, bounded outside by the special structures displayed, viz., either by muscular abdominal wall or by peritoneum. On the right side of the body a deep dissection was made from the skin, and the cæcum and peritoneum found displaced up.

"*The Uterus.*—The cervix contained a plug of mucus, and in the flattened cavity of the uterus was found a small amount

PLATES.

To face page 568.

DESCRIPTION OF PLATES.

Plate I.

Fig. 1.—Sagittal lateral section (right) of pelvis, with extra-uterine gestation in right broad ligament.

Fig. 2.—Sagittal mesial section of same pelvis, showing uterus with decidua. This section demonstrates, inter alia, that what is termed clinically retro-uterine hæmatocele may be hæmatoma.

Plate II.

Fig. 3.—Sagittal mesial section of cadaver, with advanced extra-uterine gestation — subperitoneo-abdominal (1R).

Plate III.

Fig. 5.—Sagittal lateral (2L) of same.

Fig. 6.—Sagittal lateral (3L) of same.

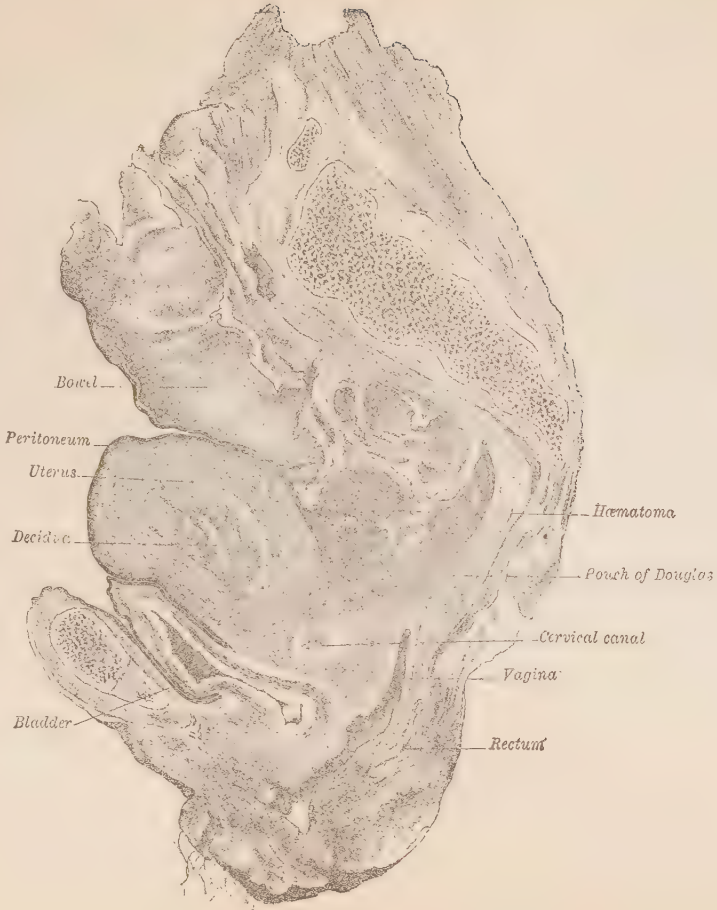

Bowel
Peritoneum
Uterus
Decidua
Bladder
Hæmatoma
Pouch of Douglas
Cervical canal
Vagina
Rectum

FIG. 2.

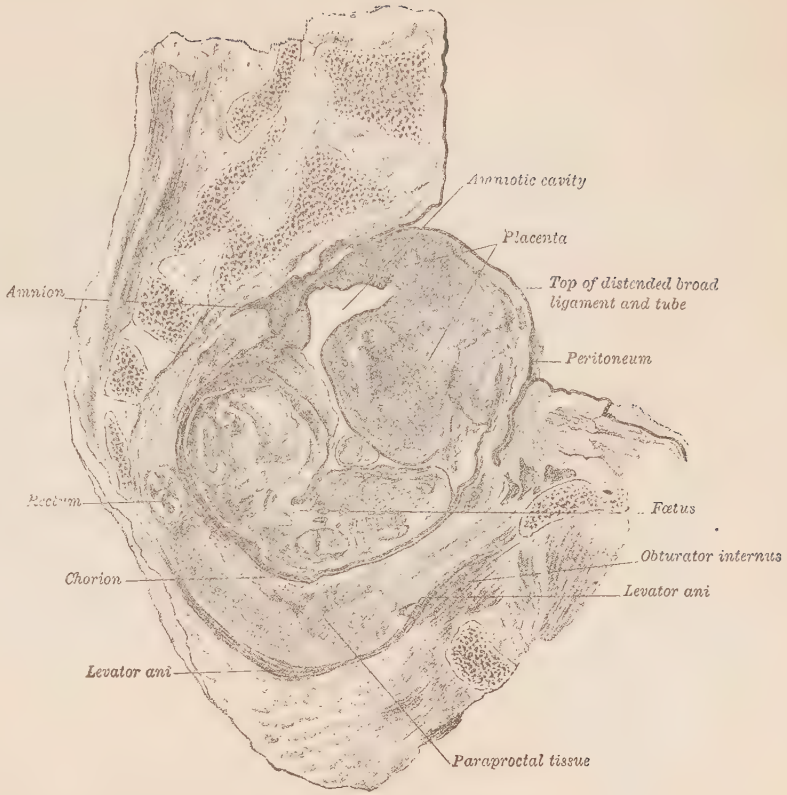

Amniotic cavity
Placenta
Amnion
Top of distended broad ligament and tube
Peritoneum
Rectum
Fœtus
Obturator internus
Chorion
Levator ani
Levator ani
Paraproctal tissue

FIG. 1.

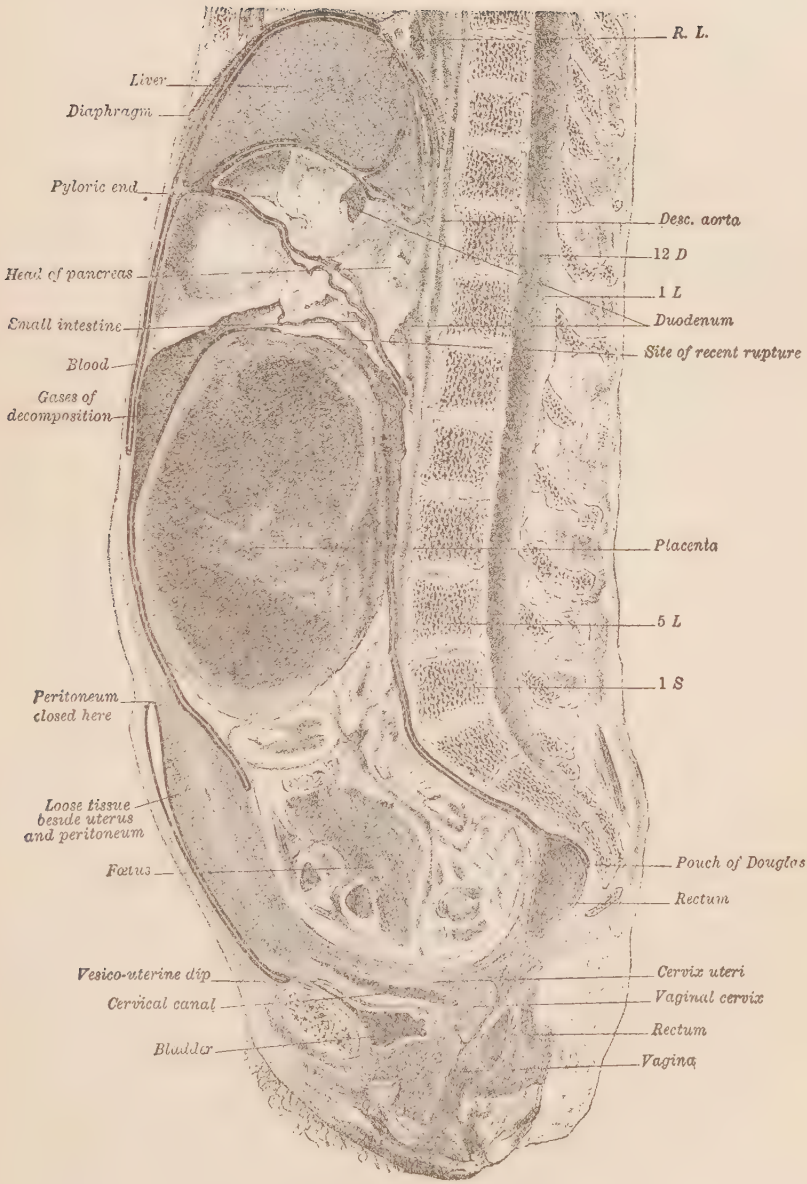

R. L.
Liver
Diaphragm
Pyloric end
Desc. aorta
12 D
Head of pancreas
1 L
Small intestine
Duodenum
Site of recent rupture
Blood
Gases of decomposition
Placenta
5 L
1 S
Peritoneum closed here
Loose tissue beside uterus and peritoneum
Pouch of Douglas
Fœtus
Rectum
Vesico-uterine dip
Cervix uteri
Cervical canal
Vaginal cervix
Rectum
Bladder
Vagina

FIG. 3. (1 R.)

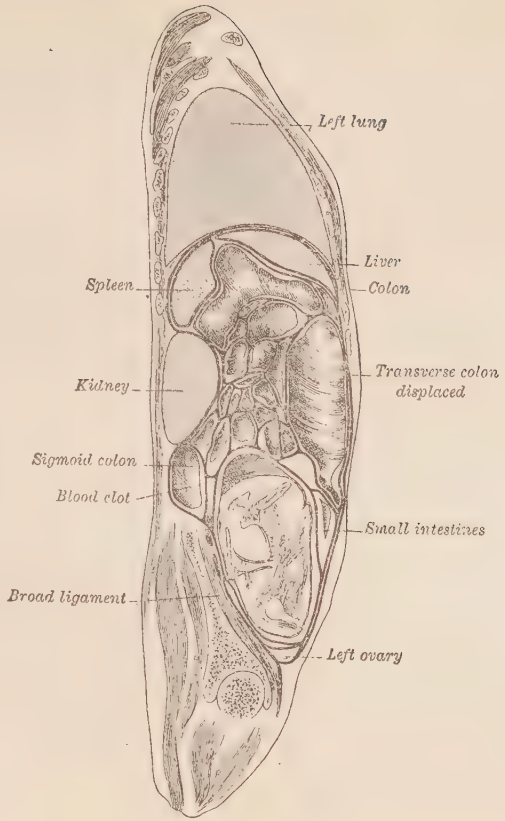


FIG. 6. (3 L.)

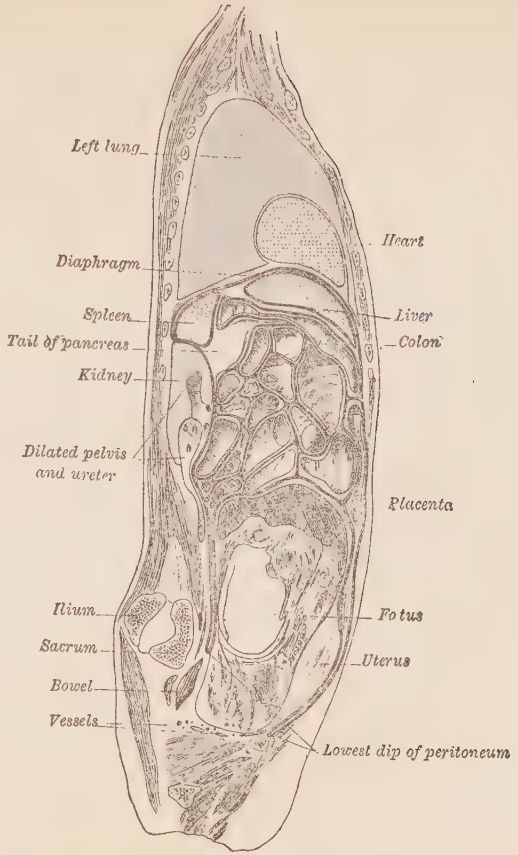


FIG. 5. (2 L.)

of disintegrated tissue. The fœtus is situated below the placenta and between the uterus in front and the abdominal wall behind. Together with the placenta it is seen to be inclosed in a distinct capsule.

"The placenta consists of an oval-shaped and flattened mass of tissue situated in the abdominal cavity and extra-peritoneally, and lying above the fœtus. Its long axis is directed up and down, and in the mesial section is seen to extend from the upper margin of the second lumbar vertebra to a little below the upper border of the first sacral vertebra. It is attached to the posterior aspect of the anterior abdominal wall and outer surface of peritoneum. Where attached to the anterior abdominal wall, the veins there are enlarged.

"The diameter of its long axis is 13.5 cm., and its average antero-posterior measurement is 7.5 cm. Around it is a thin investment of connective tissue, and it is firmly attached at points, especially in front and above, to the surrounding capsule by bands of vascularized tissue. In the right sections a cavity is seen between the capsule and the placenta, which was filled with a mass of grumous blood, and gases of decomposition, the position of which corresponds to a well-defined darkening of the skin of the anterior abdominal wall, as if the patient had suffered from a severe blow or fall. The fœtus weighed 2 lb. 4 oz. without the umbilical cord. It was fairly well nourished, *but decomposition had commenced, especially at the lower part of the abdomen.*

"The consideration of these two sections shows, therefore, a special phase in the development of extra-uterine gestation. They demonstrate that a Fallopian-tube pregnancy may develop between the layers of the broad ligament, and may continue this extra-peritoneal mode of growth, stripping off the peritoneum from the uterus, bladder, and pelvic floor until it becomes in great part surrounded by a peritoneal capsule derived from these organs. All this is done without any actual intra-peritoneal invasion. The placenta in the advanced gestation case is attached in front to the extra-peritoneal connective tissue, the veins there enlarging and acting like uterine veins. In this special cadaver, therefore, the gestation began probably in the right Fallopian tube, developed into the layers of the broad ligament, and grew extra-peritoneally, lifting up the peritoneum on the right side of the middle line, both anteri-

orly and posteriorly, and also stripping the posterior uterine wall and upper part of the anterior uterine wall. The extra-peritoneal tissue, with its blood-vessels, is therefore not only capable of forming anastomoses in abdominal aneurism, as Turner and Chiene have shown, but may attempt to carry on the functions of the maternal portion of the placenta.

"We have here what may be termed slow displacement of the placenta. At first it lay in the Fallopian tube, but the growing ovum has slowly pushed it up (a process attended with blood extravasation) from pelvis to abdominal cavity, until at last its upper edge is about ten inches from its original site. Part of this is due to growth, of course. The uterus also had its cervical portions elongated in the same way to three inches. These sections have an important bearing on the classification of extra-uterine gestation. Much has been written, and little really demonstrated on this point. The tubal variety is undoubted; the tubo-ovarian has also been demonstrated; but the ovarian is a very doubtful form. The sub-peritoneo-pelvic or intra-ligamentous variety of Dezeimeris, Tait, and Werth, is demonstrated in the second specimen, which also shows the ovary thinned out on the posterior lamina of the broad ligament. The presence of the ovarian structure in the cyst wall of an extra-uterine gestation has been brought forward as evidence of its being the ovarian variety; it more probably shows that it is sub-peritoneo-pelvic.

"The chief interest centres on the anatomical nature of abdominal gestation. The second case shows that this can be extra-peritoneal, a fact never hitherto demonstrated, although strongly contended for by Tait. We do not deny that we may have either a partial extra-peritoneal and intra-peritoneal variety, or an entirely intra-peritoneal variety, but we ask for actual proof of such. If it be urged that a purely intra-peritoneal form must exist because placenta has been found attached to the uterus and intestine, we answer that in the cadaver shown (Plate II.) the placenta has been attached to the portion of uterine wall where the peritoneum is stripped off; or it might have been attached to the other abdominal viscera, but yet carrying a layer of peritoneum before it, be still extra-peritoneal. We, therefore, hold that the following varieties have been demonstrated, viz.: tubal, tubo-ovarian, sub-peritoneo-pelvic, sub-peritoneo-abdominal. An abdominal vari-

ety, partly intra-peritoneal and partly extra-peritoneal, is probable; a purely intra-peritoneal variety has yet to be demonstrated, and the same holds good as to the ovarian variety. Hitherto we have always regarded the peritoneal cavity as the site specially chosen by extra-uterine gestation for its development, but we must now more closely scrutinize such in the light of this and similar cases."

I have placed in the italics two sentences in this record. The first to the effect that the right Fallopian tube and ovary could not be identified, having been taken up by the sac. This clearly shows, as Dr. Berry Hart concludes, and as I have for years argued, that such a pregnancy as this is originally tubal, and that the tube is carried up to form the upper part of the cyst is due to the fact that the rupture through which the ovum escapes into the broad ligament takes place at that part of the tube which lies at the junction of the two laminæ. The second point worthy of note is that even in this case decomposition had commenced, and that, therefore, had the woman been received in the clinical ward instead of the dissecting room, a surgical operation would have been demanded.

Dr. James Braithwaite, of Leeds, records two cases in which he operated successfully, and he has given such interesting details ("British Medical Journal," Jan. 3d, 1885), all of which directly support the views I have advanced in the preceding pages, that I need offer no apology for quoting them at length.

The first case had symptoms of primary rupture at the third month of gestation, and was operated upon about a fortnight after a spurious labor at the full time, the operation taking place on May 5th, 1883.

"The incision was central; no peritoneum was met with, and the sac was closely adherent to the abdominal walls. The child was lifted out by its feet, but it proved so large that it was necessary to extend the incision upward another inch. This unfortunately detached the cyst from the abdominal wall, and a coil of bowel protruded into view at the upper part of the wound. The cyst was carefully stitched to the lower surface of the wound with a continuous catgut suture. The placenta was deeply situated, but to what part it was

attached was not positively ascertained. The cyst was of considerable thickness, already black from decomposition and lined with a smooth shining membrane (the amnion), which readily peeled off. After washing out the cavity with warm carbolic water the wound was closed with silver-wire sutures, the cord being left hanging out at the lower end of wound. A large glass drainage tube was also inserted. During the next three weeks the whole of the placenta came away through the lower part of the wound. The cyst came with it; and I recognized the catgut which had been used at the upper margin of the abdominal wall incision. Much of the black and putrid mass was removed by daily traction upon the projecting parts, but unless great care was used hemorrhage occurred. When the whole of the placenta and cyst had come away the wound healed up rapidly, and the patient made a good though slow recovery, and she is at the present time as well as she was before her illness."

The extension of the incision probably opened the "finger glove process" of the peritoneum, rather than separated the cyst wall.

"Case II.—Mrs. W——, of Holbeck, a patient of Dr. Dodson's, with whom I saw her in September last year, aged thirty-five, has been married ten years, but never pregnant until the present case occurred. Menstruation all her life quite regular and natural; the last period was about October 15th, 1883. On December 3d, having missed exactly seven weeks, she was slightly unwell, and had at the same time, to use her own words, "a very violent pain" in the body. The symptoms were such as would be produced by rupture of an early tubal gestation—viz., pain and collapse. She recovered from this, but the body went on increasing in size just as in normal pregnancy. At the end of August a sanguinolent discharge occurred, and this therefore may be taken as the time when labor would have taken place had gestation been uterine. The movements of the child, however, ceased to be felt about the end of the first week in August. By examination of the abdomen the outlines of the child were not perceptible as in the last case, but some thick substance intervened, which subsequently turned out to be the placenta. This much increased our difficulty in the diagnosis. There was a projection outward of the abdominal walls in the right inguinal re-

gion, which felt not unlike a foot. There were no fœtal or placental sounds audible. The uterus measured only two inches and three-quarters, and the cervix was well open, so that the finger could be passed up to, but not through, the os internum. The pulse was weak, and the condition of the patient such that, being quite certain it was a case of abdominal extra-uterine gestation, we decided to remove the child at once. This was done on September 11th, 1884, at the Women and Children's Hospital. The incision was central, and we came, as expected, directly upon the placenta, the edge of which, however, was found about two inches to the right of the incision. Careful separation of the placenta in this direction did not produce hemorrhage. An incision at right angles to the first was now made, and the edge of the placenta being pushed back the feet of the child were seized, and it was removed without much difficulty, the placenta yielding without being torn or separated from its attachment to the abdominal walls. Whether the child was inclosed in a cyst or not we were not quite certain at the time of the operation; but, as proved afterward, this was the case. There were no veins in the abdominal walls at the seat of the placental attachment except just at the lowest angle of the central incision. These I was careful to avoid wounding; they were, however, only of small size. The wound was closed with silver wire, the funis being left out, and a drainage tube inserted, both at the extreme right of the lateral incision. An attempt to separate the placenta with the finger and traction in about two weeks set up hemorrhage, and it was not attempted again until the discharge became very decidedly purulent at the end of six weeks. The whole of the placenta which had not been removed, for some small portions had been, was at the end of six weeks separated by the finger and removed without much difficulty. It weighed thirteen ounces. The patient is still in the hospital, but is nearly well. In introducing the finger for removal of the placenta I felt the cyst walls, which appeared to be pretty firm and thick.

"The first thing worth remarking in the history of these cases is the occurrence of severe pain early in gestation, attended with some degree of collapse. This indicated rupture of the Fallopian tube, in which, up to that period, the fœtus had resided, and its escape in the first case into the interior

of the broad ligament, and in the second into the peritoneal cavity. I assume that the explanation of these cases given by Mr. Lawson Tait is the correct one, and I believe it to be so—namely, that all cases are originally tubal, that rupture always occurs, but that this rupture may be in different parts of the tube; and if on the lower surface of the tube the fœtus is let down between the folds of the broad ligament, and then develops, the placenta retaining its original hold upon the interior of the tubal cyst; if the tube ruptures on its upper surface the fœtus escapes into the peritoneal cavity; and if the mother survives it develops there just as it would have done in the uterus. It seems pretty clear that in my second case the placenta was detached from its original position and took root again in a fresh one, and that the interior of the abdominal walls. This situation of the placenta is rare, and I think it may without much difficulty be diagnosed by the thickness of the structures intervening between the fœtus and the examining hand."

Dr. R. B. Maury, of Memphis, has recently published the details of the post-mortem examination of a case which completely confirms the conclusions of Hart and Carter; and now that the facts are known, doubtless, information will be abundant, and the old confusion will speedily end.

"The pelvic organs were carefully removed, and it was then seen, beyond all possibility of doubt, that the fœtal sac was entirely extra-peritoneal, that the gestation had originated in the right Fallopian tube, and had developed between the folds of the broad ligament, downward to the pelvic floor, laterally to the pelvic wall, and upward into the abdomen.

"The ovum in its development had lifted the peritoneum off from the bladder and the anterior surface of the uterus, while the relations of the peritoneum to the posterior uterine wall and to Douglas' pouch were not altered.

"The sac extended quite to the pelvic and abdominal wall on the right side, but did not go beyond the left cornu of the uterus on the left.

"At the time of the operation it was observed to be covered by peritoneum, and this was clearly shown after death.

"The gestation was therefore entirely extra-peritoneal, and belonged to the variety intra-ligamentous of Werth, or sub-peritoneo-pelvic of Dezeimeris.

"No trace of the ovary was discoverable in the structures belonging to the sac, but on the left side the ovary was found much shrivelled and otherwise changed in appearance.

"This autopsy corroborates the view taught by Mr. Lawson Tait, that in extra-uterine pregnancy, no matter where the fœtus may be found, its development begins in the Fallopian tube, 'and that it may become intra-peritoneal or extra-peritoneal, just as the tube happens to burst.'"—"Memphis Medical Monthly," March, 1888.

We come now to consider the further details of the operation of removing a fœtus developed in the broad ligament at, or near, or past the full time of gestation; and no variation on this point will make any difference in the essential details of the proceeding. The opening of the abdomen and sac should be, as I have said, to one side of the middle line, and the history, together with the physical signs, will probably enable us to decide on which side of the middle line the incision should be made. As the purpose is to avoid opening the uterine process of peritoneum, the incision should be made two or three inches away from the middle line and toward that side in which the pregnancy has been developed—if this point can be determined. When the sac is opened the fœtus is to be removed carefully, so as to avoid tearing as much as possible; and if it is alive, it should be handed over to those specially detailed for this duty. The umbilical cord should be divided close to its placental origin, and the placenta should be emptied, as far as possible, of blood. The interior of the sac should then be carefully cleansed of all dirt and loose membrane, and then filled and washed thoroughly with clean water, and the stitches carefully placed in the wound so that when they are drawn tight the sac shall be hermetically closed.

By means of my siphon trocar the sac should again be washed out with warm water, and then the stitches drawn tight with the trocar (small-sized) still in the sac. The siphon action should then be reversed and the sac emptied of water as much as possible and the trocar removed—in so doing care should be taken that no air enters, and that the wound is hermetically closed.

I recommend this proceeding from the splendid results I have obtained by it in dealing with congenital cysts (Trans. Gynæcological Society, 1887), strikingly different from those

arrived at by drainage. It seems to me that the conditions of the two cases are very similar, and that the success in one may justify the same means being tried for success in the other. The crux of the discussion is, of course, the removal of the placenta, and I have tried all ways with it, and I am disposed to think that leaving it will be the best. I have already detailed a disastrous case where I removed it by vaginal section. I have twice removed it, arresting easily what hemorrhage there was by the application of perchloride of iron. Both children were alive and still live, and both mothers survived, but in both cases I was able to tie a big pedicle—the remains of the tube and broad ligament—which doubtless contained the bulk of the blood supply to the placenta. This proceeding I certainly should recommend in all cases where it is practicable, and from my own experience alone it seems certain that it will be possible in a considerable number of cases. But there are others, and I have published these, where such a proceeding was not possible, where the placenta was plastered flat on various structures to which it was intimately adherent, and from which it would have been removed only with great difficulty and much hemorrhage. I confess under such circumstances I should hesitate before commencing its removal, but if I did begin it I should rush rapidly through with it and follow separation with a sponge soaked either in strong vinegar, or a solution of perchloride of iron. Such a process would be very risky, and I confess I should not like to face it, and for the further reason that I do not think it will prove to be necessary.

The alternative proceeding which I have adopted in these cases—all three mothers surviving—was to close the sac (closing the peritoneum in one case, when it had been opened as in Dr. Braithwaite's first case) all save an aperture through which I brought the umbilical cord and a drainage tube. These three women all survived, but they survived a process of offensive suppuration lasting for months, and which nearly killed them all. One of them—as result of this profuse suppuration and of her own carelessness—has a ventral hernia, which contains most of her intestines.

I certainly, therefore, am not in love with this method of dealing with the placenta—for it deliberately induces the process of necrosis, which I do not in the least believe is necessary.

We must bear in mind that when the placenta has acquired adhesions outside the uterus it is in a condition altogether different from that in which it is placed when in contact with the endometrium. In both cases it is of course essentially a fœtal structure, but it is far less so when it has its relations in ectopic pregnancy. When intra-uterine it is separated by a maternal layer of cells easily destroyed and being constantly replaced, which are not present when its columnar villi invade intestines, muscles, and other maternal structures. Again, when the process of labor is going on in the uterus, every contraction of the organ tends to disturb the connections between the fœtal and maternal tissues, so that when finally the complete contraction of the uterus is effected on the expulsion of the child, the placental relations are completely disconnected by the mere pressure of uterine contraction. No such disconnection occurs to an ectopic placenta. The histories of all the cases where an extra-uterine pregnancy has gone on for an indefinite period after the term of gestation without disturbance, show conclusively that all the tissues except the bones are capable of being digested and absorbed, and even the bones to a large extent yield to this powerful influence. The placenta, as a rule, is the first of the tissues to disappear, even despite the somewhat numerous instances to which I have already alluded, where the placenta at first seems inclined to grow. For the majority of cases such a tendency at this period might, for a time at least, be disregarded, but even if it became from subsequent observation evident in any particular instance that the placenta was growing after the removal of the fœtus, we should have the advantage at least of having gained time in the treatment of the case; for nothing has so strongly impressed itself upon me in my experience of abdominal surgery, as that we may deal safely by secondary steps with conditions which, had they occurred to us in a primary stage, would certainly have led to unfortunate issues. I am therefore disposed, for the present at least, and until I am corrected by future experience, to advise that in dealing with an ectopic gestation in the advanced stages, we should deal with the fœtus only, should empty the placenta of blood and close the wound hermetically upon it. The only exception would be where it can be dealt with largely by tying the broad ligament only to a relatively small extent

requiring separation from the tissues with which it is associated. Campbell has to some extent anticipated my argument on this point in the following passage: "As the placenta, when long retained, is destroyed during the suppurative process, except in some rare instances, and removed from the abdominal cavity with the other decomposed structures, or cannot be discovered, this discloses to us the important fact that the retention of the mass may be permitted without any detriment to the patient; while it can scarcely be doubted that the irritation, which could not fail to be produced by groping for it among the abdominal viscera, or the hemorrhage arising from its detachment, might be succeeded by formidable effects. At one period it was supposed that the placenta could not be suffered to remain in the abdominal cavity with impunity; but it may be asked, can the retention of the mass be more injurious to the patient than that of a full-grown fœtus, which, as we are now aware, may remain in the abdominal cavity for a long series of years without any injurious effect?" (p. 152.)

A case of great interest in the consideration of this point is narrated in the "Obstetrical Transactions" of 1887, by Dr. Champneys, in which the proceeding which I now recommend was more nearly carried out than in any other I have seen. The placenta was emptied of blood, but unfortunately the cord was not cut short, but was allowed to hang out of the wound, dependence having been most unfortunately placed upon the so-called antiseptic system to prevent decomposition.

The operation was performed on the 19th of October, and upon the 19th of November the progress of the case is noted to the effect that "the incision was completely healed, but the lower abdomen markedly distended, and a swelling which was supposed to be the placenta considerably smaller." Subsequent events made it perfectly clear, however, that the patient was suffering from placental decomposition and resulting peritonitis, and she went on from bad to worse, with a pulse of 114 and temperature 104°, and as high even as 106°, to the 7th of January, when she died. On post-mortem examination the placenta was seen to be lying in the sac like a round ball, as large as a fœtal head, and of a dark maroon color. On passing the fingers round it a few bands and one or two adhesions were found between the placenta and the sac, but otherwise the placenta was detached. The blunder, of course, in this case

was that the fœtal sac was not opened a second time, and the placenta removed immediately at the outset of serious symptoms—that is to say, within five weeks of the original operation. It is perfectly astonishing that the patient should have been allowed to go on for very nearly six weeks after this, in a condition of subacute blood-poisoning, without any effort being made to save her. The lesson of the case I feel strongly is, that we ought to make a preliminary effort, by leaving the placenta alone and closing the sac over it, to permit of its absorption. Should that not occur, we may then, by a secondary operation at such time after the first as may be indicated by the course of events, remove the placenta. This proceeding would then be rendered far less hazardous, at least in the matter of hemorrhage, by nature's own process of the inflammatory occlusion of the blood-vessels. Certainly this is the reasonable method, as it seems to me, of dealing with this important question, the only one yet awaiting its proper solution, and its solution is forced upon me not only by my experience in ectopic gestations, but by my experience in a large number of other operations in abdominal surgery. Certainly it is not a question which will be settled by the tabulation of a number of cases mostly dissimilar in the extremest degree from one another, and incapable of leading to anything but confusion when paraded in the form of statistical evidence.

Campbell gives a very interesting list, and withal a very ghastly one, of instances which he has unearthed where there have been multiple extra-uterine gestations, and of instances also where they have been retained for very many years. As a mere matter of curiosity I reproduce it: "Two patients had the product of three extra-uterine gestations in their abdomen at one time; in both individuals all the decomposed structures were evacuated through the abdominal parietes, and each recovered. Nine women conceived once during the retention of the extra-uterine fœtus; two, twice; one, three times; one, four times; one, six times; and one seven times. There were two cases of contemporaneous intra- and extra-uterine gestation. In this variety two single woman only are particularized. In seventy-five cases the fœtus was retained for the following periods, viz.: three months in two instances, four months in one, five months in one, nine months in two, fifteen months in three, sixteen months in two, two years in eight, three years

in seven, four years in four, five years in one, six years in two, seven years in three, nine years in one, ten years in three, eleven years in two, thirteen years in one, fourteen years in two, sixteen years in one, twenty-one years in one, twenty-two years in one, twenty-six years in two, twenty-eight years in one, thirty-one years in one, thirty-two years in one, thirty-three years in one, thirty-five years in two, forty-eight years in one, fifty years in one, fifty-two years in one, fifty-five years in one, and fifty-six years in one. In twenty-six patients the decomposed structures were evacuated through the rectum, and of this number six died. The fœtal structures passed through the abdominal parietes in twenty-nine cases, and three of the number died. In eight instances the remains of the fœtus were discharged per vaginum, and three of the patients died."

True lithopædion—that is to say, where the fœtal sac has been incrusted, after more or less digestion and absorption, with a layer of the salts of lime, and has remained quiescent—is of remarkably rare occurrence. I have only once in my lifetime seen a case where it was suspected to have occurred. Dr. Fales, of Boston, has spent much labor in examining the literature on the subject, and he has found only eleven cases where the condition has been verified by post-mortem examination, and he adds a twelfth occurring in his own experience. As his paper is in a journal, the "Annals of Gynæcology," not very easy of access, and as the subject certainly has a considerable amount of interest, I venture here to reproduce his record.

"Case I. is reported by Dr. Brandt, in the 'Edinburgh Medical Journal' for 1862:

Miss A.	was born, . . .	1778.			
"	was married, . .	1795,	at the	age of	17.
"	first child, . . .	1796	"	"	18.
"	second child, . .	1801	"	"	23.
"	pregnant, . . .	1804	"	"	26.
"	third child, . . .	1808	"	"	30.
"	fourth child, . .	1815	"	"	37.
"	died, ...	1858	"	"	80.

No history of the third pregnancy. The autposy was performed September, 1858. The tumor weighed 1.8 kilos, 20.32

cm. in length, 13.33 cm. in diameter, 40.64 cm. in circumference. It was a bony cyst containing a fœtus, head uppermost, looking to the left and downward. The spine and back were in apposition with the right side of the cavity; the head was decidedly compressed; the cord could be distinguished passing round the neck; the whole body was twisted in its long axis.

"CASE II. is reported by Dr. Conant, in 'New York Medical Journal,' May 10th, 1865, p. 140:

"So far as known, the pregnancy, which was the first, was normal, labor pains came on at the usual time, lasted a few days, and subsided. Subsequently she was afflicted with profuse and most offensive perspiration, which was almost unbearable to her attendants. After a time this disappeared, and slow recovery ensued, attended by a hard tumor in her side, which caused her no inconvenience other than a sense of weight. Subsequently she gave birth to three children. In June, 1863, thirty-five years after the accident, she died. The autopsy revealed a calcified fœtus, extra-uterine, seemingly not enveloped with or in, membranes; another hard mass, said to have been the uterus, was found in the abdomen, this, however, contained the remains of the placenta, in the opinion of Dr. Conant.

"CASE III. is reported by Dr. Parkhurst in 'Medical Times and Gazette,' Vol. I., 72, p. 655:

"She became pregnant in 1802; nothing unusual about the pregnancy was noticed; the catamenia ceased entirely; fœtal movements appeared at the usual time. Premature labor was begun at eight and a half months, as the effect of a fright. The pains gradually subsided, and for two or three weeks she was comfortable. Her health then began to decline, and for one and a half years she was an invalid. After this period there was a gradual restoration to a condition of comparative health, though she was subject to attacks of severe abdominal pains at irregular intervals. She died in 1852, at the age of seventy-seven. The autopsy disclosed a tumor, the external surface of which was smooth and white, and composed of fibro-cartilage. Its weight was 3.6 kilos. There was no connection with the Fallopian tubes or omentum. The external surface of the fœtus was incrusted with an earthy substance.

"CASE IV. is reported by Dr. Hans Chiari, 'Wiener Med. Presse,' Vol. 17, No. 38, p. 1,092:

"In this case symptoms of pregnancy were observed in 1827; but no birth followed them; the patient died at the age of eighty-two, of pneumonia. At the autopsy the tumor was found to be attached to the walls of the uterus. It was about the size of a man's head, and here and there over its surface points of calcification could be detected. The uterus, right tube, and ovary were normal; the left ovary was wanting. The fœtus was enveloped in a capsule, and was in a remarkably well-preserved state; the face, internal organs, and even the striæ of the muscles being recognizable. The placenta was found, but its position is not stated.

"CASE V. is reported by Dr. Galli, in 'Lo Sperimentale,' xxxix., 2, p. 135:

"In this case, two children having been born, pregnancy occurred, for the third time, at the age of thirty. Fœtal movements ceased after the eighth month. No birth followed. Subsequently, for a long period, she suffered from severe abdominal pain. Became pregnant again, and was delivered of a healthy male child. The product of the third pregnancy was carried for thirty-seven years. In her sixty-seventh year she fell, and probably disturbed the lithopædion, as a violent peritonitis intervened, from which she died. The autopsy revealed a well-formed lithopædion; but nothing further is stated.

"CASE VI. is reported by Dr. Plexa, 'Monatsschr. f. Geburtsh.,' xxix., 4, p. 242:

"In this case symptoms were manifest which caused the diagnosis of extra-uterine pregnancy to be made. There were repeated attacks of abdominal pain, accompanied by fever. These gradually subsided, and strong hopes were entertained that this case would eventuate in a lithopædion. After one and a quarter years, however, a peritonitis ensued, from compression of the intestines between the tumor and the abdominal walls, which caused the patient's death at the age of forty. At the autopsy it was found that the fœtus had entered the abdominal cavity by the bursting of the left Fallopian tube. The right ovary and tube were normal. The color of the fœtus was dark brown and calcification had begun.

"CASE VII. is reported by Professor J. Van Grau and Dr.

Schrant in 'Genees. en Heilkunde te Amsterdam,' ii., 1, pp. 17–96:

"The patient was married at twenty years of age. Had seven children, and three miscarriages. Twelve years before her death she noticed a gradually increasing swelling of the abdomen. The tumor was distinctly movable, and appeared to be adherent at the umbilicus. A diagnosis of lithopædion was made; and, at her death, at the age of forty-two, in the Amsterdam Hospital, this was confirmed. The tumor was free, except at the front, where it was attached to the abdominal walls. The fœtus was enveloped in a calcified membrane; its head was situated at the umbilicus, the back toward the left hypochondrium; arms and legs drawn toward each other, and to the right. The uterus was in the lower pelvis, and was normal. The left ovary and tube were also normal. In the place of the right ovary there seemed to be a cyst, filled with a brownish substance, attached to the tube. After the covering was stripped off the fœtus was seen with the head, legs, and arms drawn toward each other. The internal organs, muscles, and other structures were easily recognized.

"Case VIII. is reported by Dr. Wagner, 'Arch. der Heilk.,' vi., No. 2, p. 174:

"The patient was a widow, sixty-eight years old. At the age of twenty-four she had given birth to five children. In her thirty-seventh year she again became pregnant, but was never delivered of the child. Labor-pains were not present. For a long period the abdominal enlargement remained constant in size, and Cæsarean section was advised. Finally, the tumor began to grow smaller; her menses returned, and fair health was experienced, the only complaint being of a feeling of weight in the abdomen. At the autopsy the tumor was found to fill the lower pelvis, and to be attached to the bladder, rectum, and uterus. The tumor weighed three-quarters of a pound, and was about the size of a man's head. It was covered by a yellowish membrane. The left tube and ovary seemed to be growing from the tumor, the uterus being pushed from the right. The fœtus was of female sex; the head was much drawn to the right, and bent upon the thorax. The skull was markedly compressed, the bones overlapping; calcification was present, but not uniformly. The various organs

and muscles were not distinguishable, being changed to a fatty mass, which contained hæmatoidin crystals.

"CASE IX. is reported by Dr. Bossi, 'Sitzungsber. d. Vereins d. Aerzte in Steirmark,' xi., p. 37:

"In this case a lithopædion was diagnosed in 1868. During the years 1869 and 1870 abortion was induced several times. The operation was repeated in 1872, with a fatal result, peritonitis following. The autopsy revealed a pear-shaped tumor about the size of a man's head, covered with a capsule, which was very thick and hard (calcified). Portions of the fœtus were in a natural condition, and portions were changed to adipocere, some of the bones being entirely denuded. The tumor communicated with the rectum by a small opening. The uterus and tubes were normal. Right ovary atrophied, left one adherent to tumor.

"CASE X.—'Tübingen Inaugural-Abhandlung,' von Wilhelm Keiser.

"The lithopædion was found in a woman ninety years of age, in 1720. In 1674 she had all the symptoms of pregnancy, fœtal movements being very noticeable. At the expiration of nine months labor-pains started up; the membranes ruptured. Pains continued for two weeks, and then gradually disappeared; the fœtus having apparently escaped into the abdominal cavity, after rupture of the uterus. Two children were subsequently born. The autopsy revealed a large tumor, 13.5 cm. in diameter, covered with a capsule so hard that a knife could not cut it. The stroma contained an exudation in which lime-salts were deposited. The skin of the fœtus was well preserved, covered by epidermis more or less calcified. The muscles could not be recognized, having been changed to a 'soft substance' (adipocere). The brain was a blackish-brown mass, which was pulverulent and easily melted; the membranes were of a leathery consistence. A citron color was diffused throughout the entire structure. The reports concerning the position of the tumor are not trustworthy.

"CASE XI. is reported by Smellie in his "Collection of Cases and Observations in Midwifery,' vol. ii., p. 65:

"The patient was pregnant in 1731, with the usual signs. At the sixth month fœtal movements ceased, as the result of a fright. Under treatment she discharged a mass, which was thought to be a part of the placenta, as well as a small

amount of fluid. There was no decrease in the size of the abdomen. In July, 1733, two years and two months from her first pregnancy, labor-pains returned, with an apparent rupture of membranes. At this time the child was found in the abdomen. In January, 1734, she became pregnant, and was delivered October 28th. She was again delivered October 22d, 1735, also October 9th, 1738, and June 17th, 1741. She was admitted to Guy's Hospital October 14th, 1747. She died November 7th, 1747. The autopsy showed the abdominal contents to be nearly in their natural state. In the right pelvis was a child, attached to the ileum and neighboring membranes by the peritoneum, in which the tube and fimbriæ were apparently lost. The fœtal integument had become partially calcified.

"Case XII.—In giving the history of this case I hoped to quote from the record books of the physician in attendance at the time of the accident, who, as I understand, took extensive notes, but I am unable to do so owing to his death a few years ago, and the subsequent destruction of his records. I am fortunate, though, inasmuch as such information as I have of the case comes from a twin sister, who is still a remarkably vigorous woman, both mentally and physically, and whose statements, as far as they go, are undoubtedly correct. Mrs. A—— was married September 24th, 1844. She never had any miscarriages. She was delivered of a perfectly healthy child, January 29th, 1848. Early in January, 1856, she became, as events proved, pregnant again, though her condition at the time was merely surmised, as menstruation continued to be present, and, in fact, existed, with more or less regularity, throughout her entire pregnancy. It was not until the middle of May that the attending physician made a positive diagnosis of pregnancy, basing his opinion on fœtal movements, which became manifest at that time. Early in March, while visiting friends, she fainted, vomited, and complained of epigastric pain. There was no flowing at this time. The following day she rode home, a distance of four miles. Directly after this she had three 'inflammatory fevers,' characterized by abdominal pain, excessive tympanites and uncontrollable nausea and vomiting. During one of these attacks an abscess formed just above the pubes, which opened, but did not discharge much, if any. Counting from the middle of May, when fœtal

movements began, October 1st would be the probable date of confinement. About that time the physician was summoned, not on account of labor pains, as she never had them, but on account of excessive and painful movements of the child. These were always very marked, and caused her the utmost inconvenience. As she expressed it, she felt more life with this child in two hours than during her entire previous pregnancy. October 13th the physician was again summoned for the same reason as before. At this time 'something was rubbed on the abdomen,' after which the movements grew less and less, and finally ceased. For the following ten years she was an invalid, though nothing very explicit could be obtained as to her condition. She was generally miserable, and had a number of attacks of abdominal pain at irregular intervals, sometimes accompanied by icterus. During this period the tumor very gradually decreased in size, finally remaining stationary, and causing no trouble other than a feeling of weight when standing or walking too long. Her health was fair until 1883, when a malignant growth attacked her larynx, which eventuated in her death December 24th, 1886. The autopsy was performed December 26th, 1886, Drs. Bill and Metcalf assisting. The body was very much emaciated. The tumor was apparently situated in the median line, with its most prominent point at the umbilicus, but on palpation it was found to extend downward and to the left. On making the incision it was found to be adherent to the abdominal walls, and it seemed as though it would have soon made its way through, either from pressure or ulceration, so thinned had the structures become at the point of its adherence. The position of the tumor may be best described by borrowing the obstetric expression, sacrum, left anterior, though it was entirely out of the pelvic cavity, the base of the skull being on a level with the umbilicus. It was almost lying loose in the abdominal cavity, the only points of attachment being the one just referred to, to the abdominal wall, what was probably the umbilical cord, and some small adhesions to the intestines. These were ranged round the tumor, none in front of it, and were one mass of adhesions, forming, with the abdominal wall, a cavity, as it were, containing the tumor. The umbilical cord (?) passed directly downward, inclosing the uterus, and then gradually fading out into the peritoneum. Nothing that would

answer for a placenta, or the remains of one even, could be found. Roughly speaking, the parts of the fœtus were normally disposed, the thighs and arms being flexed on the abdomen and chest respectively. The left leg was rotated slightly outward, as well as extended, and the forearms, instead of being crossed, were more or less parallel with the long axis of the body, the hands being placed well up beside the head. The tumor weighed $2\frac{3}{4}$ lbs., was $8\frac{1}{2}$ inches long, and $12\frac{1}{4}$ inches in circumference. The cross section showed it to consist of a fœtus and its envelopes, the process of calcification being especially marked in the membranes. The uterus, Fallopian tubes, and ovaries were also removed, but furnished no points of importance. The autopsy suggested an extra-uterine pregnancy of the abdominal variety; but the history points rather to one of the tubal variety, primarily. To epitomize the various dates:

Mrs. A—— was married in, . . .	1844.
First child, ...	4 years later.
Second pregnancy, ...	8 " "
Probable rupture of cyst and peritonitis,	at the third month.
Death of fœtus, ...	at the ninth "
Period of ill health, ...	10 years.
Period of health, ...	27 "

Death from cancer of larynx invading the lung at the age of 67."

We are quite justified in concluding from such records that Campbell and Parry are correct in their belief that a "quiescent lithopædion" is a very rare occurrence, and that a woman with the remains of an ectopic gestation sac in her abdomen or pelvis had far better have them removed.

BIBLIOGRAPHY.

1. Lancet, October 30th, 1886.—2. Bernutz and Goupil: Diseases of Women, translated by Meadows, New Sydenham Society, 1866.—3. Principles of Midwifery, by John Burns, edited by James, vol. i., p. 168, Philadelphia, 1817.—4. Principles and Practice of Obstetrics, p. 442, Washington, 1834.—5. Lancet, October 26th, 1886; also, Diseases of Ovaries, p. 348.—6. Handbuch der Krankh. der weibl. Geschlechtsorgane, 7 Aufl., Leipsic, 1886.—7. Zeitschrift für Geburt. und Gynæk., 1884.—8. Quoted by Lusk: Midwifery, p. 289.—9. Fritsch: Dis. of Women, p. 289, New York, 1883.—10. Diseases of Women, p. 202, New Sydenham Society.

VOLUME V. NUMBER 3.

WOOD'S Medical and Surgical MONOGRAPHS

Consisting of Original Treatises and Reproductions, in English, of Books and Monographs selected from the latest literature of foreign countries, with all illustrations, etc.

CONTENTS

THE ARREST OF GROWTH IN CANCER BY THE INTERRUPTED VOLTAIC CURRENT (ELECTRO-ATROPHY). By J. INGLIS PARSONS, M.D.
THE DREADFUL REVIVAL OF LEPROSY. By SIR MORELL MACKENZIE, M.D.
DISEASES OF OLD AGE. By DR. A. SEIDEL, Berlin.
URINARY NEUROSES OF CHILDHOOD. By DR. LOUIS-J. GUINON, Paris.
VARICOSE VEINS OF THE LOWER EXTREMITIES. By WILLIAM H. BENNETT, F.R.C.S.
THE USES OF ELECTROLYSIS IN SURGERY. By W. E. STEAVENSON, M.D.

PUBLISHED MONTHLY
PRICE, $10.00 A YEAR SINGLE COPIES, $1.00
MARCH, 1890

NEW YORK
WILLIAM WOOD AND COMPANY
56 AND 58 LAFAYETTE PLACE
1890

2025

Entered at the Post Office at New York, as second-class matter.

Copyright by Wm. Wood & Co., 1889.

PRESS OF
THE PUBLISHERS' PRINTING COMPANY,
30 & 32 WEST 13TH STREET,
NEW YORK.

CONTENTS.

THE ARREST OF GROWTH IN CANCER BY THE INTERRUPTED VOLTAIC CURRENT.

PAGE

Electro-Atrophy, ... 587

THE DREADFUL REVIVAL OF LEPROSY.

The Extension of Leprosy, and Measures to be taken to Arrest its Spread, ... 603

DISEASES OF OLD AGE.

CHAPTER I.

Senile Changes in Different Organs, ... 631

CHAPTER II.

Diseases Affecting the Aged, ... 648

CHAPTER III.

Hygiene of the Aged, ... 657

URINARY NEUROSES OF CHILDHOOD.

CHAPTER I.

Introduction, ... 669
Classification, ... 671
Physical and Chemical Properties of the Urine, ... 676
Spasm of the Bladder, ... 679

CHAPTER II.

Nocturnal Incontinence of Urine, ... 681

THE

ARREST OF GROWTH IN CANCER

BY THE

INTERRUPTED VOLTAIC CURRENT

(ELECTRO-ATROPHY).

ALTHOUGH considerable progress has been made during the last quarter of a century by Althaus, Newman, Cutler, Apostoli and others, in the treatment by electricity of simple and benign tumors, very little, if any, success has followed similar attempts by others to treat cancer with this agent. Neftel, Prince, Beard, Rockwell, and others, who have worked at the subject, have apparently achieved but little success. Very probably the chief cause of this was to be found in the fact that these observers had an erroneous theory of the action of the constant current. They appeared to be still under the influence of Morgan, who thought that, because decomposition of molecules took place at the poles when a constant current was passed through living tissues, therefore it also took place along the whole course or channel of the current. It was a very natural inference to draw, and the treatment of cancer would be very much simplified if it did occur. But as a matter of fact, it does not, as the experiment about to be described appears conclusively to prove. So far as I have been able to ascertain, I was the first to practically demonstrate this fact and disprove what could only be called a theory without foundation.

When a constant current is passed through living tissues and electrolysis occurs, there are three distinct actions which take place: firstly, a decomposition of molecules in the neigh-

borhood of the poles; secondly, a local caustic action by the acids and alkalies at their respective poles, produced by this decomposition of molecules; thirdly, the transport of elements from one pole to the other. By using a small electrode the caustic action is intensified. Even with a strong current of 200 milliampères continued for half an hour, the amount of tissue destroyed is not much bigger than a walnut, and is confined to the neighborhood of the needle. A case of fibroid tumor beyond operation which I treated in this way, and published in the "British Medical Journal" of April, 1889, proves this statement. Twenty-eight applications were necessary to the treatment, extended over three months. The patient's life was saved. But then such a method, which merely produces a small slough, would not answer with cancer, which grows rapidly and infiltrates the surrounding tissues. It was for this reason that former attempts failed; all that happened was a slough in the neighborhood of each pole, and to produce which any other caustic would in most cases have answered just as well, while the tissues between the poles remained unaffected. A still stronger objection to electrolysis, as it has hitherto been applied with the constant current, is that the caustic action destroys healthy and diseased tissues alike. In order, therefore, to kill all the outlying cells of a cancer, it would be necessary to destroy an enormous amount of healthy tissue, or else leave some cells behind. So that in this respect electrolysis applied with a constant current, as has hitherto been done, offers little if any advantage over other forms of treatment, such as the knife and caustics.

With my own method this is not the case; there is little or no destruction of healthy tissue, the cancer cells alone are destroyed. The reversal of the current at each interruption, causes the alkalies and acids formed at the poles to neutralize each other; by this means the caustic action is reduced to a minimum. While my discovery of the destructive action of the voltaic current in the inter-polar region, when suddenly interrupted, furnishes us with a weapon of offence against morbid growths, and capable of adjustment to the varying necessities of different cases; with a continuous current, the destructive action is almost entirely confined to the poles, whereas with the interrupted voltaic current it extends the whole distance between them. By this means, very extensive areas can be

acted upon, and cases can be saved where the use of the knife would be entirely out of the question.

The only patients that I have had an opportunity of treating have been those who were beyond operative interference, or too nervous to undergo an operation with the knife.

The method of treatment about to be described is based upon the following considerations:

First, with regard to the nature of cancer. The earliest pathologists regarded it as a dyscrasia, or altered constitution of the blood; then followed the embryonic cell theory; finally, the microbe was made to bear the burden of further misdeeds.

Virchow, with his dogma, "*Omnis cellula e cellulâ,*" demolished the first. The second, propounded chiefly by Cohnheim, will not account for all tumors, even if it be admitted as possible that embryonic tissue can remain for a quarter to half a century in the human tissues, without partaking in the changes undergone by other cells in the body. Thirdly, all attempts to find the microbe have failed.

The hypothesis now brought forward is founded on the fact that all the cells of the body possess a latent capacity which enables them, under various stimuli, to proliferate and form new tissue. Under normal conditions, this capacity remains dormant, or is only exercised to the extent of keeping pre-existing structures in a living condition. But let an injury or irritation to some part of the body occur, and this latent capacity ceases to be dormant, and an active formation of cells takes place, until the injury is repaired or the irritation removed. Now this formation of new tissue is essentially a new growth, although not a tumor. The capacity for forming it is common to all the tissues of the body, and it ceases when the necessity for its continuance has disappeared. This appears to show some controlling influence, probably by the nervous system. Is it not, then, possible that all tumors are in the first instance composed of cells formed in the normal process of repair, but that, having escaped from the control of the nervous system, they take an independent life, and continue to proliferate and develop without check, just as all living cells are capable of doing *ad infinitum* under favorable conditions?

The frequent association of cancer with a depressed condition of the nervous system is explained by this hypothesis. In

other words, a tumor consists of cells which have sprung from the pre-existing cells of the body, but have escaped from the control of the system at large. The failure of even Virchow to find any reliable histological evidence in the early stages of a recent celebrated case points to the probability that the boundary line between the tumor new growth in its early stages and a normal new growth produced in the repair of an injury to the tissues is very indefinite. The close connection between the two is shown by some statistics in a recent paper by Dr. Gross ("International Journal of Medical Science," March and April, 1888), who found that out of some 907 cases of cancer of the breast there had been antecedent inflammation in 20 per cent, and a history of injury in 13 per cent.

Dr. John Williams found microscopical evidence of the gradual transition from healthy tissue into cancer in a case described by him in the Harveian Lectures (1886; Case XV., p. 51). At the same time, although devoid of nerves, all tumors have some sort of vascular arrangement, and continue to receive nourishment from the circulation. In this respect they are on an equal footing with the rest of the body. This appears to form a formidable barrier to any measures taken to stop growth through the internal administration of drugs. Whatever would affect the tumor cells would also affect all the cells in the body. Considering their origin and the similarity in composition between the cells of tumors and those of the rest of the body, it is unlikely that any drug will be found to exercise a selective influence on the former and leave the latter untouched. Unless this could be done, it would be necessary to damage more or less the whole of the body in order to get at the cancer cells.

There is one point, however, in which the normal cells of the body essentially differ from tumor cells. Most of the latter show a decided tendency to degenerate, while, in marked contrast to this, the healthy tissues possess wonderful recuperative powers, due probably to the presence of trophic nerves. It therefore seemed to me that, if a cure were to be found for cancer, it would be based upon this essential difference between healthy tissue and tumors, and that on account of this difference some agent might be found capable of inflicting an irreparable damage to a tumor, and yet stop short of destroying the normal cells of the body, on account of the higher re-

cuperative powers possessed by the latter. And further, that, although the normal cells of the body are unable to resist the invasion of the cancer cells, the latter are actually of lower vitality. If such be the case, it may be asked why should the cancer cells devour the healthy?

The probability is, that the difference in composition and structure between the malignant and healthy cells is not sufficient to stimulate the latter to resistance. Consequently, the proliferation of the cancer cells, resulting from their independent existence, enables them to overcome the healthy cells, since these, for want of sufficient stimulus, remain passive. In a paper read before the British Gynæcological Society in 1888 "On the Action of the Constant Current on Fibroid Tumors of the Uterus," I advanced the following hypothesis: "That while the normal tissues of the body have the power of recuperating any effect produced on them by the transport of elements, the cells of tumors being of lower vitality might be checked in their growth, and perhaps in time destroyed."

The following experiments were tried in 1887 to ascertain whether, when a current is passed through a tumor, any decomposition takes place between the poles. Three glasses were connected together with a stout lamp wick. Into each of these a standard solution of iodide of potash was placed. A current was then passed through the solution with an intensity of 200 milliampères until the whole of the iodide of potash in the two outer glasses was decomposed. On testing the centre glass, it was found to contain as much iodide of potash as before, although in the two outer glasses it was all decomposed. The resistance of this apparatus was so great that I was obliged to go to the Electric Apparatus Company, and do the experiment in the vaults of the Junior United Service Club, where an unlimited supply of volts could be obtained. A second experiment was then tried with water in the outer glasses and a solution of iodide of potash in the centre. The current was passed as before, but no decomposition took place in the centre glass. A third experiment was tried on the web of the frog's foot, and watched through the microscope. At the positive pole a coagulation took place, with exudation of blood from the capillaries. At the negative pole a collection of hydrogen bubbles could be seen. Between the poles no change could be observed, and the circulation went on as be-

fore. Finally, a strong current was passed through a fibroid tumor, soon after removal from the body, for a period of several hours. On making sections of the portion between the poles, no change could be detected. All these experiments demonstrated in a conclusive way that the passage of a constant current does not cause any decomposition between the poles, although there must of necessity be an exchange of atoms between the molecules. Recently, I have had an opportunity of examining sections of a fibroid tumor that I had thoroughly treated with a constant current, and which was subsequently removed by Dr. Travers on account of its size. No change could be found between the poles, showing that the conclusions drawn from the previous experiments were correct. Subsequent experience has shown me that repeated applications of the constant current, with an intensity of 200 milliampères, are unable to control the increase of a quickly-growing fibro-cystic tumor. But I noticed, in treating cases of fibro-myomata of the uterus by a constant current, that when, as occasionally happens, through an imperfection of the circuit, the current was suddenly interrupted, marked signs of disturbance followed, such as rise of temperature, considerable pain, and malaise. Rightly or wrongly, I put this down to some destruction of tissue occurring in the tumor, and, if this conclusion were correct, the means required for the treatment of cancer on the hypothesis advanced has been found.

The most probable explanation of this phenomenon is that the impact of a powerful current, when suddenly flashed through, causes death to those tumor cells which lie in the nearest path between the poles where the lines of force are most concentrated, or else they are sufficiently injured to lead to their gradual atrophy. The latter explanation is more probably the true one, as no rise of temperature or other symptom follows the applications when applied to cancer.

Further than this, it has been ascertained by experiments on animals that even moderate voltaic currents suddenly interrupted could, when applied to the nervous system, cause death; while very much stronger currents applied to other parts of the body were found to be quite harmless in their action. If the normal cells of the body then differed so widely in their resisting power to electricity, it seemed more than likely that some difference would also be found between normal

cells and those of tumors, although at first one might think the excess of resistance would be on the wrong side. For the treatment of fibroid tumors, anything likely to cause their wholesale destruction is to be deprecated. For this reason interruptions of the constant current have been carefully avoided, and precautions taken to gradually increase it as smoothly as possible. Being surrounded by a capsule, and having but few vessels in their interior, a gangrene *en masse* might follow, with all its attendant consequences. But this argument would not apply to cancer. Those cells especially that most require to be destroyed are scattered through the healthy tissues, and each little islet or bird's nest is surrounded by this tissue, furnished with vessels and lymphatics capable of absorbing it so soon as loss of vitality and degeneration occur. I therefore felt no hesitation in trying the destructive effects of a powerful interrupted voltaic current in the first case of cancer that objected to be operated upon in the usual way. After waiting a year or more the case presented itself. There is one point that, for want of space, I have not entered upon: Is cancer a local or a general disease? So far the accumulation of evidence appears strongly in favor of its being, at any rate, a local disease at first.

The *modus operandi* is as follows: The patient is anæsthetized; the current is then passed through the tumor and all the tissues for some inches round it by means of fine insulated needles, so as not to injure the skin. A battery of 70 cells, with an electro-motive force of 105 volts, is used; the intensity of the current to commence with is 10 milliampères, gradually increased to 600 milliampères, and flashed through the growth in every direction from 50 to 100 times, according to circumstances. The pulse and respiration are carefully watched. One out of the four cases treated, a woman aged sixty-three, with extensive carcinoma of the left breast, a presystolic bruit, and weak, intermittent pulse, was unable to stand more than 250 milliampères, and for this reason: when the current is applied to the left breast electrical stimulation of the heart occurs, and if this organ is healthy an increase in the strength of its contraction appears to take place after its passage; but with the patient who had cardiac disease the improvement only continued up to a certain point, and then the intermittency increased, and great irregularity occurred.

These observations made last year on the human subject corroborate exactly those made in the laboratory on mammals by Dr. John McWilliam, and described in a recent paper in the Journal. He called the condition of the heart fibrillar contraction, that is to say, a widely inco-ordinated arhythmic contraction of the ventricular muscle, attended by a great and rapid fall of blood-pressure. It is only, however, in the immediate neighborhood of the heart, and when it is diseased, that this takes place. When the current was passed through a secondary growth in the axilla of the same patient, it could be increased in 600 milliampères with perfect safety.

The effects produced by the action of the electricity consist in a cessation of growth, gradual disappearance of pain, some shrinking and hardening of the tumor and enlarged glands, followed by improved nutrition and better state of the general health. The growth as a whole does not disappear, but remains as an inert mass, composed in all probability of fibrous tissue alone. Before treating these cases I felt almost sure that this would be the case, because I had found by a number of experiments, detailed in the paper before referred to, and also from treating a large pelvic fibroma by galvano-puncture in 1887, that neither could electricity decompose the fibrous tissue of new growths (whatever it might do with cicatricial tissue), nor could the system cause its absorption after it had been rendered inert. However, it will always be open to patients to have the remains of the growth removed subsequently by the knife. The advantages claimed for this method of treatment are as follows:

1. There is no destruction to the normal tissues of the body, and if recurrence should at any time occur, its progress can be immediately stopped, and the treatment repeated as many times as necessary. Life would by this means be prolonged indefinitely, provided that metastatic deposits had not occurred before the commencement of the treatment. So far, cases able to bear the full strength required have shown no signs of recurrence.

2. Patients are not obliged to lie up, but are able to get about on the day following the application.

3. The current can be passed through almost any part of the body, and thus arrest growths which could not by any possibility be otherwise treated.

CASE I.—August 15th, 1888. A. L., aged 38, married. Family: four children; three miscarriages; last, three years ago.

Family History: Mother died from asthma; father, alive and well, aged 64. No history of cancer in the family.

Previous History: Has never at any time had any abscess or swelling in the breast, nor had any difficulty in nursing. Seven months ago she suffered from shooting pains in the left breast, and then noticed a lump there for the first time.

Present Condition: On the inner side of the left breast is a hard nodule, the size of a walnut, and a second one three times the size on the outer side, infiltrating the surrounding tissue. On the skin of the areola a distinct tuber can be seen; the nipple is also retracted. In the axilla are three enlarged glands. She complains of shooting pains in the breast and down the arm.

Diagnosis: Carcinoma of the breast. Mr. Bryant very kindly saw this case for me, and confirmed the diagnosis.

August 23d. Operation: Ether was given, and an interrupted voltaic current, slowly raised to 400 milliampères, was applied to the breast and tissue round in every direction. During the application the pulse became stronger, while respiration was not affected. At each make and break, the arm and shoulder were jerked up.

August 25th. No rise of temperature or discomfort followed beyond a slight feeling of malaise. The pain has all gone.

August 30th. Operation repeated.

September 15th. The glands in the axilla treated in a similar way, and she was sent home.

October 18th. No return of pain. The mass in the breast and the glands in the axilla are smaller and harder.

February 6th, 1889. She still has no pain, and the condition of the growth remains unaltered. General condition excellent.

March 12th. Patient continues to be quite well.

CASE II.—September 26th, 1888. S. H., aged 62, married. Family: three children.

Previous History: She never had any trouble or pain in her breasts until twelve months ago, when she noticed two small lumps on the left side. These gradually enlarged, and

caused severe shooting pains. About four months ago she began to feel a dull aching pain in the shoulder, and extending down the left arm.

Present Condition: There is a large, hard mass involving the whole of the left breast, and extending on the subcutaneous tissue on the outer side, and toward the axilla. On the outer side of the breast the skin is adherent and thrown in wrinkles. The nipple is markedly retracted, and two tubers can be seen on the skin. The whole mass is firmly adherent to the pectoralis major, but not to the ribs. High up on the inner side of the axilla is an enlarged gland the size of a walnut. There is loss of sensation on the outer side of the arm; one of the supraclavicular glands is slightly enlarged. The pulse is irregular, weak, and intermittent. A presystolic bruit can be heard.

Diagnosis: Carcinoma of the breast, also confirmed by Mr. Bryant.

October 4th. Under ether, interrupted voltaic current, gradually raised to 300 milliampères, was applied to the under part of growth. Any attempt to exceed this strength caused great irregularity of the cardiac beat. After thirty interruptions of the current it was necessary to desist, on account of the weak action of the heart.

October 10th. Same treatment was repeated to the rest of the growth, with the same limitations as before.

October 25th. The gland in the axilla was treated, and, as the pulse was not affected, the current was raised to 500 milliampères. The patient went home next day.

November 7th. Pain in the shoulder and arm have quite disappeared, while sensation in the other part of the arm, which she had previously lost, has now come back; but on the outer side of the breast, where considerable shrinkage has taken place, she still feels some pain.

December 19th. Condition unchanged. No pain, no sign of recurrence.

February 1st, 1889. The last few weeks she has had a return of the old pain. There is some redness and further infiltration of the skin near the nipple, and also some puffiness and œdematous swelling on the inner side of the growth. The mass as a whole is still movable, and does not appear larger, except for the puffiness before mentioned. But the swelling

in the axilla, where the whole force of the battery was used, remains unchanged, nor is there any return of pain in the shoulder or arm.

February 2d. Interrupted voltaic current, slowly raised to 400 milliampères, was applied to the whole of the upper and outer parts of the breast. The patient was then sent home.

Case III.—November 12th, 1888. (Kindly sent to me by Dr. Gerald Harper.) E. E., aged 36, married. Family: eleven children, no miscarriages; last child seven months. Menstruation always regular, quantity moderate, duration four days.

Previous History: Enjoyed very good health until after birth of last child seven months ago, when metrorrhagia began and has continued up to the present date. During this time it has never stopped for more than two days. Three months ago slight pains began, chiefly in the groins and back, extending down the thighs. These have recently become very much worse, and often keep her awake at night. The bladder during the same time has become very irritable; she is unable to hold her water for more than an hour.

Family History: No history of cancer; one sister died of consumption.

Present Condition: She is rather a pale, delicate-looking woman. Examination per vaginam: In place of the cervix there is a large excavated ulcer, in the shape of a hollow cone, extending for a short distance all round into the vaginal wall. The surface feels soft and irregular and bleeds profusely. The discharge is most offensive, and the odor can be detected some yards off. The broad ligaments on both sides are infiltrated, and the uterus is partially fixed.

Diagnosis: cancer, confirmed by Drs. Arthur W. Edis, Travers, and Gerald Harper. On consultation, the case is considered beyond operation.

November 18th. Ether administered. Interrupted voltaic current applied to the growth. Intensity 400 milliampères.

December 10th. She had no discomfort from the application. Ulcer is harder; still bleeds easily.

Ether administered. Interrupted voltaic current, 600 milliampères, applied to every part of the growth and tissues round. To return home.

March 9th. After going home she was for some weeks in bed; the hemorrhage, however, gradually went off, and she has had none for a fortnight. The last occasion she thinks was her period. She now has no trouble with the bladder nor has she lost flesh, and she is able to get about her house and do some work. She still feels some pain in the groins, but it is very much less.

On examination, the uterus is movable, and there is no infiltration of the surrounding tissues. The cup-shaped depression formed by the remains of the growth is very hard, but does not bleed at all on being touched.

Case IV.—January 15th, 1889. M. W., aged 61 (kindly sent to me by Dr. Potter, of Kensington Infirmary). Family: five children, no miscarriages; last child 14 years.

Past History: Two years ago she had a lump taken out of the upper part of the right breast at the Cancer Hospital. It recurred in twelve months' time and has continued to grow since. She suffered no pain, or very little, from it.

Present Condition: At the upper part of the right breast there is a small, hard, nodular mass, about 2 inches long and 1 inch broad. The skin over it is movable, but not freely so; it is attached to the pectoralis major and surrounding tissues, but it is not adherent to the ribs. There is no enlargement of the axillary or other glands.

January 18th, 1889. Interrupted voltaic current was applied to the growth, strength 600 milliampères.

January 20th. Patient has experienced no pain or other ill effect from the current, and can go home.

March 12th. The growth appears to have shrunk, and she has no pain and no enlarged glands.

April 9th. Patient continues quite well; no signs of growth recurring.

Remarks.—Case I. Although this patient up to the beginning of March, eight months after the first application, showed no signs of recurrence, it is, of course, possible that this may occur, because I did not at that time feel sufficiently sure of my ground to increase the current beyond 400 milliampères. With the second patient, who has heart disease, the quiescent state of the nodule in the axilla ever since it was first treated, six months ago, with full force of 600 milliampères, compared

with the renewal of activity in the older portion of the growth in the breast, where it was only possible to use 300 milliampères, is very striking, because, as a rule, at this stage of the disease the growth is usually most virulent and active in the secondary formations.

The advanced stage in which Case III. was taken in hand gave very little hope of a successful result. Notwithstanding this, the patient at the end of three months is wonderfully improved, and able to go about and do her work. It seemed more than probable that, as the patient was beyond surgical interference, there would be some involvement of the lumbar glands which would be most difficult to get at, and, therefore, the possibility of metastatic deposits in this case must not be overlooked.

I feel very little doubt that some patients will be able to stand even a stronger current than 600 milliampères, which is the maximum at present obtained. If recurrence occurs, I should feel no hesitation in increasing the intensity beyond that point. It may be said that cases of scirrhus are only slow growing, but with Cases I. and II. the previous history of so short a time as six months and twelve months respectively shows that, on the contrary, the growth has been remarkably active.

Since the publication of my first paper, I have treated other cases which I intend to publish at a future date. The observation of these has fully confirmed the efficacy of the treatment.

The cases which may be said to defy surgical aid most, are sometimes the best for treatment.

For instance, cases of recurrence where there is no main mass, but a diffused infiltration without involvement of the skin, are most satisfactory, and the growth can be stopped without pain and with very little disturbance. On the other hand, a circumscribed mass such as sometimes results in the neck after removal of the tongue, from early involvement of a lymphatic gland, creates far more disturbance and after treatment is apt to break down and form an abscess. As it is not diffused through the healthy tissues, it forms too large a mass in the aggregate for the system to absorb. This, however, does not apply to a primary tumor of the same size, for then there appears to be sufficient healthy tissue remaining in the

growth to absorb the débris, unless, as in one of my cases, it is of enormous size, and has existed for a considerable time; in that case the balance between absorption on the one hand, expulsion by suppuration on the other is altered, and the latter turns the scale. With enlarged glands the treatment is very satisfactory so long as they are not much larger than a walnut. There is, however, one very important point to bear in mind, and it is this, that the electrical resistance of these and of all hard growths is often from 100 to 150 ohms higher than the surrounding tissues. Unless the electrode, therefore, actually pierces the gland, the current will choose the better conductor, namely the healthy tissues, and pass round the gland instead of going through it, and thus leave the growth untouched. Any one not aware of this fact might easily fail to obtain good results and be inclined to condemn the treatment through ignorance.

The same principle applies to different growths. The electrical resistance of a soft growth is very much less than that of a hard growth. Among the unsatisfactory cases are those where the skin is extensively involved, as it is the worst conductor of all the tissues in the body; it is difficult to pass a current across so as to act on an extensive area. This, after all, is a minor point, because patients usually seek relief before this has occurred. When the skin is merely adherent, there is no difficulty in the way. A year and four months has now passed since I operated on my first case, the details of which I append and were published in the "British Medical Journal" of April 27th, 1889, and I am happy to say she still remains perfectly well. I omitted to mention that the diagnosis of this case was also confirmed by Mr. Jacobson, Dr. Edis, Dr. Harper, and others, in addition to Mr. Bryant, and I have recently shown this case to several members of the profession and offered to show her at the Clinical and Medical Societies of London.

THE DREADFUL REVIVAL OF

LEPROSY.

BY

SIR MORELL MACKENZIE M.D.

THE DREADFUL REVIVAL OF LEPROSY.

INTRODUCTORY NOTE.

An article upon any subject from the pen of so able and brilliant a writer as Sir Morell Mackenzie, needs no prefatory remarks to assure for it an abundance of readers. Though the question discussed in the following pages will strike many as being a little out of the author's line, still the views advanced respecting the wide prevalence of leprosy in various quarters of the globe and the great harm which has resulted from the ill-advised "Report on Leprosy" made by the Royal College of Physicians in 1867, are so forcibly put and so evidently true, that one must reject the temptation to read between the lines and recalling some recent personal unpleasantnesses, to attribute to the author any animus in explanation of his present zeal.

Circumstances are such that the present is an auspicious time for the agitation of this most important question, and we must welcome every effort to put a check upon what is so aptly termed a "Dreadful revival of leprosy." The question is one of greater importance to our own country than is generally known or acknowledged by those who should know, and he who attempts to aid in its solution is too apt to be put down as an alarmist.

Honest opinion is always to be respected; but how men of recognized ability in the profession, who are fully cognizant of the history of the disease, can believe it safe or wise to admit lepers into our country, and harbor them here, where the chances of escaping the scourge are so great if proper precautions are taken, is beyond my comprehension.

It is most encouraging to note that the efforts which have of late been made to secure some legislation bearing upon the leper question in the United States are already rewarded by some practical results.

Secretary Windom has just issued an order that "no vessel shall be admitted to entry by any officer of the Customs until the master, owner, or authorized agent, shall produce a certificate from the health officer or quarantine officer at the port of entry or nearest United States quarantine officer, that no person affected with leprosy was on board the vessel when admitted to free pratique, or, in case a leper was on board such vessel, that he or she, with their baggage, had been removed and detained at quarantine. Second.—Medical officers in command of United States quarantines are hereby instructed to detain any person affected with leprosy found on board any vessel, but such officer will permit the departure on outgoing vessels of persons detained at quarantine in pursuance of this regulation, provided such vessel shall be bound to the foreign country from which the said leper shall have sailed."

Hard as this may appear when it affects unfortunates who, having contracted leprosy in a foreign land, are returning home, it is none the less wise and just.

I can only emphasize my view, expressed two years ago ("The New York Medical Journal," March 24th–31st, 1888), that "It is a question of national interest and must be settled on the basis of what means will best prevent a national calamity. If there is no cause for fear, well and good. If there is any danger, let us not hesitate because the individual would suffer. *Salus populi suprema lex* is the motto which must guide us."

The report of forty-two cases seen by one observer in New Orleans since this time, many of whom were native-born citizens, would surely seem to indicate an increase sufficient to cause alarm.

Let the authorities go one step further and make the report of leper cases to local boards of health compulsory. This will cause the detection of such cases as may have escaped quarantine, owing to difficulties of diagnosis in the early stages of the disease, and will effectively do away with foreign lepers seeking treatment here.

Then let the government establish one or more central leper

hospitals or isolated settlements for the segregation, care, and scientific treatment of the afflicted now within our bounds. This seems a harsh measure, but each succeeding year's delay only makes its execution more difficult. Foreigners could be given the option of returning to their own country, while the discomfort of the few natives would be as nothing compared with the benefit conferred upon generations to come.

CHARLES W. ALLEN, M.D.
696 MADISON AVE., New York.

In order to explain what may at first sight appear to be an intrusion into a region altogether foreign to my line of professional work, I may perhaps be allowed to say that from a very early period of my career I have taken a particular interest in leprosy. Next to the skin, the throat is the part most often attacked by the worst form of the disease; and for this reason I have sought every opportunity of seeing it at close quarters. At the risk of falling into the "autobiographical" vein so deprecated by Mr. Balfour, I may add that I have made special investigations on leprosy in most of its European haunts, and also in Madeira; I may therefore claim the right to speak of it with some amount of personal knowledge. My attention was first directed to the subject nearly thirty years ago, when I was studying diseases of the skin under the celebrated Hebra at Vienna. In his wards I saw several cases of leprosy, which I understood came from the "Danubian Principalities" of those days. In 1880 I examined a number of lepers in the Hospital de San Lazaro at Seville, in 1881 I saw several cases in the lazaretto at Funchal, and in 1884 I made extensive investigations in Norway, at Molde and Bergen, where I had the advantage of the assistance of Dr. Danielssen and Dr. Armauer Hansen, whose names are familiar as household words to the medical profession throughout the world in connection with leprosy. In Danielssen, who has watched the course of the disease among several generations of his countrymen, the doctrine of heredity finds its most uncompromising champion; in his son-in-law Hansen, the discoverer of the *bacillus lepræ*, contagion has naturally enough one of its most thorough-going supporters. One could hardly be in a better position for hearing both sides of this most important question than between those two distinguished men. In 1888 I saw a

few cases in Italy in the Civil Hospital at San Remo. (Invalids visiting this charming health resort need not be afraid of coming in contact with lepers. The few unfortunate victims of the disease are kept under close supervision in the Civil Hospital, which is situated on a high rock, and is separated even from the old town, to which the building is adjacent. The part of San Remo which is frequented by those seeking health or sunshine in that delightful spot is as free from lepers as Brighton or Eastbourne.) I have also had a few opportunities during the last twenty-five years of examining cases of leprosy here in London, in my own practice and that of others.

In this country most people, I imagine, were till lately in blissful ignorance of the fact that leprosy still walks the earth in all its original hideousness. Vague notions, derived partly from the Bible and partly from casual references in historical works, made up the sum of popular knowledge on the subject, and to the "general reader" leprosy was but a name, an extinct *deinotherium* of the palæontology of disease. Very few English doctors were better informed. The disease was either not referred to at all, or was dealt with in the most perfunctory way in lectures and text-books of medicine. As Dr. Munro, whose writings on leprosy have done so much to diffuse a knowledge of the disease among medical men, points out, students a very few years ago might have gone out to fulfil their mission of healing in various parts of the world without knowing that such a disease as leprosy was to be met with. Their first introduction to it was often when its existence was forced on them as a strange and disconcerting phenomenon in actual practice.

Space will not permit me to trace the early history of leprosy in ancient times, nor even to chronicle its course in Europe in the dark ages. My regret at being obliged to leave out some historical details which might prove interesting is lessened by the fact that an excellent summary of the researches of Hirsch,[1] Munro,[2] and others, was published five years ago in "The Nineteenth Century" by Miss Agnes Lambert.

Judging from the long intervals of time which often elapsed without any mention of the disease, and the frequent notices of it by writers at particular periods, it would appear that between the twelfth and the fifteenth centuries the disease underwent considerable vicissitudes, becoming at times more

prevalent and then again being much less common. These changes probably corresponded with alternating periods of want and prosperity, the disease becoming general when the vitality of the nation was lowered by long wars, pestilences, and famine. The extraordinary spread of the disease at the time of the Crusades led to the belief that it had again been imported into Europe from the East, and Voltaire characteristically says that this was the only permanent result achieved by these expeditions. There is, however, abundant proof that even if leprosy was reimported, it had really never left Europe. In the early part of the sixteenth century the scourge suddenly began to abate, and in a relatively short time it became nearly extinct in most of the countries of Europe.

There are, however, a few strongholds from which leprosy has never been driven. Spain supplies many centres of infection, but it is impossible to obtain exact statistics on the subject. We have, however, the testimony of Dr. Roman Viscarro to the fact that "from time immemorial lepers swarm in Spain, especially in the provinces of Asturias, Tarragona, Valencia, and Castellon."[3] Dr. John Webster, who visited the leper hospital at Granada about thirty years ago, found it tenanted by fifty-three inmates. He was informed that in 1851 the number of lepers in nine provinces of Spain was 284; this was probably far below the real number, as the natural tendency of lepers and their friends to hide their affliction is in Spain intensified by religious superstition, and the supineness of the authorities must lead to perfunctoriness in the difficult task of collecting statistics on the subject. Dr. Webster was informed that leprosy was believed to be spreading in Spain at the time of his visit. At Seville, in 1880, I found thirty-nine sufferers in the Hospital de San Lazaro. During the five years 1875–80, the total number of lepers admitted was eighty-four, the greatest number in any year having been twenty-one (1879–80). Seville itself supplied the largest contingent; then came Cadiz, Huelva, Almeria, Badajoz, and Pontevedra. The figures, however, give an altogether inadequate idea of the prevalence of leprosy in these districts. As a high authority says: "In addition to the sufferers from those provinces who enter the hospital, there are many others who remain at home with their families, some maintained by them, others dependent on public charity; and probably only those

seek shelter in the hospital who are destitute of all resource."[4] The late Dr. Jelly[5] showed how extraordinarily prevalent leprosy is in the district known as La Marina, which takes in the sea-board of the two provinces of Valentia and Alicante; and he also brought forward proofs of the spread of the disease in the south of Spain in recent years.

Portugal has more lepers than any other European country, except Norway; but want of space prevents my showing its distribution. In Italy leprosy is met with on the Genoese Riviera; it was also found till quite recently at Comacchio, in the Ferrara marshes. In Sicily the disease has been steadily spreading for the last thirty or forty years. In annexing Nice, France took over with it a considerable number of Italian lepers belonging to Le Turbie and neighboring places, but the disease is now almost extinct in these localities. Small foci of leprosy still exist in Thessaly and Macedonia; the affection is not rare in some of the Ægean islands, *e.g.*, Samos, Rhodes, Chios, and Mitylene, and it is extraordinarily prevalent in Crete. It is spreading to an alarming degree in Russia, especially in the Baltic Provinces, and it has lately been found necessary to establish a special hospital at Riga. In St. Petersburg cases are occasionally, though very rarely, met with; at least half of them are imported from outlying provinces. "Sporadic" cases are said to occur in some parts of Hungary and Roumania. In Sweden, where the disease was extremely prevalent up to the beginning of the present century, it seems now to have almost died out. Norway is unquestionably the most considerable leprosy centre in Europe at the present day, but the disease is curiously limited to particular regions, such as the districts round Bergen, Molde, and Trondhjem.

In almost every other quarter of the globe leprosy is rife at present, and wherever it exists it seems to be slowly, but surely, extending its ravages. It is impossible to estimate even approximately the total number of lepers now dying by inches throughout the world, but it is certain that they must be counted by millions. It cannot be comforting to the pride of England, "the august mother of nations," to reflect that a very large proportion of these wretched sufferers is to be found among her own subjects.

That leprosy has spread considerably in recent times there

can be no manner of doubt. Within the last fifty years the seeds of the disease have been sown in several districts where it was previously unknown, and already the accursed crop has begun to show itself. As has been shown by Dr. Munro,[6] the seeds of leprosy take something like half a century to mature, and there is every prospect that unless the natural evolution of the scourge can in some way be prevented, a terrible harvest will be reaped before many years are past. To say nothing of the notorious case of the Sandwich Islands, where leprosy, imported about the year 1850, either by whaling ships manned by sailors from leprous regions or by Chinese immigrants, has since made such fearful progress, we have the case of Australia, where it has been carried by the Chinese, and of America where several distinct centres of infection have appeared within living memory. Thus there is California, where it was imported by the "Heathen Chinee"; some of the North-Western States, where it was brought from Norway by Scandinavian emigrants; and Salt Lake City, to which it was conveyed by Mormon converts from the Sandwich Islands. (This fact was communicated to the New York Academy of Medicine by Dr. P. A. Morrow on the 6th of June, 1889.) In Louisiana, where last century leprosy prevailed so extensively that a hospital for it was founded in 1785, it again showed itself in 1866, in a woman whose father was a native of the South of France. From this fresh centre the disease has spread to such an extent that Dr. Blanc recently saw forty-two cases in New Orleans alone. The disease also appeared in Oregon among the Chinese, but was promptly checked, and in South Carolina a limited outbreak occurred between 1847 and 1882. Sixteen cases were reported, the first victims being Jews belonging to families which had emigrated to the United States early in the century; several of the rest were Jews, but there were also some native Americans and at least one Irishman among them.

In France the disease is also extending, as we learn from a communication made to the Académie de Médecine of Paris on the 14th of October, 1887, by Dr. Besnier, the distinguished physician of the Hôpital St.-Louis. This authority stated that, since France had extended her colonial possessions, French soldiers, sailors, traders, and missionaries have fallen victims to leprosy in large numbers.

In the United Kingdom we have at present no leprosy of home growth, but we are probably never without a few cases among those who have lived for some time in countries where the disease is common. From an unofficial return, recently published in one of the medical journals, it appears that in the early part of the present year there were several patients in London suffering from leprosy. There is, or was quite lately, a boy in a large public school in whom there are the strongest grounds for suspecting the existence of leprosy in the early stage; the disease is supposed to have been communicated by vaccination in the West Indies. It is beyond question also that there are many other cases in this country at the present moment which are carefully concealed from the knowledge of every one but the medical adviser. Nearly every skin specialist must be able to attest this fact.

But the most striking extension of the disease has been witnessed in the Sandwich Islands. Here Dr. W. Hillebrand saw the first case—the first spark of the conflagration—in 1853 "in a thinly-populated district of Oahu, about twenty miles from Honolulu, in a small village near the sea."[7] The disease, however, was not officially recognized till 1859. At that time "only a few cases became known, but with every subsequent year the leprous patients presenting themselves at the public dispensary increased in number, until during 1864 and 1865 it was considered of quite ordinary occurrence that lepers should apply for relief."[8] A census taken by the Hawaiian government about that time gave the number of known lepers, suffering from the tubercular form of the disease, as 230 out of a population of 67,000. In 1866 the segregation settlement at Molokai was opened, and since that time more than 3,000 cases have been received there. The last report of the Hawaiian Board of Health gave the total number of lepers in the settlement on the 1st of April, 1888, as 749, but Dr. Prince A. Morrow, of New York, who visited Molokai in the early part of this year, puts the present number at nearly 1,100.

In the West Indies the disease has been steadily extending its ravages for many years back. Thus at Trinidad, where in 1805 the three first victims could still be pointed out, an investigation ordered by Governor Woodford in 1813 revealed seventy-three lepers in a total population of about 32,000. Two years later there were seventy-seven.[9] The evil was unfortu-

nately not considered of sufficient magnitude to need state interference. Ten years later, however, a half-hearted attempt at segregation was made, but in 1840 it was found that the number of sufferers had so much increased that a proposal to establish a settlement on a little island a few miles off had to be abandoned, because it was too small to hold them. In 1878 the numbers of lepers was officially stated to be 860 in a population of 120,000. These figures have an eloquence of their own which requires no comment from me to emphasize the startling truth which they convey—namely, that at Trinidad in the years between 1813 and 1878 leprosy increased nearly four times as rapidly as the population! We shall see presently to what cause it was undoubtedly attributable.

In British Guiana the increase of leprosy in recent years has been not less remarkable. In 1858 an asylum for lepers was established at the mouth of the Mahaica Creek. "On the 31st of December, 1859, there were only 105 inmates at the asylum. In 1869 they had increased to 300, and the place could hold no more. Increased space was provided, and in 1889 we find from the official reports that over 500 were dealt with. Around this leper asylum, outside its boundaries, there are large numbers of lepers not included in these returns." [10] Dr. Hillis, the best living authority on the subject, informs me that the increase of leprosy in British Guiana during the past ten years has been very great, although during that period the general sanitary condition of the colony has improved. He estimates that at the present time there must be more than a thousand lepers in British Guiana, a number equal to 1 in 250 of the whole population. Considering the very high death-rate of the disease—16 per cent annually—it is clear that it is spreading with great rapidity.

When New Zealand was first taken possession of by the English, a peculiar form of leprosy akin to the anæsthetic form was found to exist. Dr. John Myles, of Taranaki, who recently paid a visit to this country, informed me that the disease is most common in a zone of about twenty-five miles round Lake Taupo. The whites, it appears, do not become leprous, except the "Pakeha Maoris," as the whites are called who live much with the Maoris or intermarry with them. No system of restraint exists now, but when the Maoris possessed the country they killed those who became leprous. Fear of

contracting the disease prevented them indulging their cannibal propensities in relation to the poor lepers. It has been noticed in recent years that there has been considerable increase in the amount of leprosy, though it is still principally confined to the neighborhood of Lake Taupo.

In India, the last official report gives 135,000 as the number of lepers, but Mr. E. Clifford says [11] that there can be little doubt that they already exceed 250,000, and that their numbers are still growing. In Canada it has recently been discovered that the cases in the lazaretto at Tracadie do not comprise all the lepers in New Brunswick, and a considerable number of other cases are believed to exist, especially among the French residents in the northern part of the province.[12] At the Cape, though it was recently denied officially that leprosy is increasing, a strong impression prevails that the disease is extending. The same may be said of our Australian colonies, and the statement to that effect made by the Prince of Wales at the first meeting of the committee of the "Father Damien Memorial Fund" at Marlborough House has not, so far as I know, been called in question. Such a state of things may well cause anxiety, not only to those responsible for the welfare of our colonies, but to all lovers of humanity.

The facts above set forth represent an unspeakable amount of suffering to many races whose destinies we have taken into our keeping, but besides this they indicate a possible danger to ourselves. Leprosy has before now overrun Europe and invaded England, without respecting the "silver streak" which keeps off other enemies; and it is perfectly conceivable that, under certain circumstances, it might do so again. It is well known that, in recent years, our countrymen whose lot is cast in places where the disease is indigenous have ceased to show the immunity from its attacks which was once thought to be their privilege. Can all this misery be prevented? I do not hesitate to answer, Yes. It may be doubtful whether the disease can be stamped out, but it is certain that its advance can be checked, and that its ravages can be confined within comparatively narrow limits. How is this to be done? Only in one of two ways—by eliminating the cause of the disease, or by preventing its transmission.

The former of these methods is unfortunately impossible, for we are at present entirely in the dark as to the cause of

leprosy. The subject has for centuries been a favorite playground for the "scientific imagination," and, as Cicero said of the philosophers, there is no absurdity which has not found defenders among the various scientific pundits who have wrestled with the problem. Climate, soil, and race have each been tried and found wanting, for the disease exists in every kind of climate and on every variety of soil, and no race is exempt from it, although certainly some divisions of the great human family appear to be more open to its attacks than others. Almost since the dawn of medical speculation food has been regarded by many as the *fons et origo mali,* and there has been a remarkable "stream of tendency" toward fixing on fish as the particular esculent at fault. As far back as the days of Aretæus, the eating of fish and milk at the same meal was reputed to be an infallible cause of leprosy; the old proverb of Provence, "Le poisson fait devenir ladre," represented a popular belief which has for centuries been almost universally prevalent in European haunts of leprosy; and even at the present day, when the theory has completely "deliquesced" under the solvent action of what Magendie called *le fait brut,* a distinguished surgeon still tenderly hugs his old *mumpsimus,* and sees putrid fish (Raymond ["Histoire de l'Eléphantiasis," Lausanne, 1767, p. 23] tells us that in the South of France it was not putridity, but the opposite condition, that was dreaded. "Les gens du pays attribuent la cause occasionnelle du mal . . . à l'usage du poisson mangé trop frais") at the bottom of every case of leprosy, though the patient may have never had a chance of eating fish of any kind. The "exquisite reason" appears to be that, if the leper himself did not, some of his ancestors may have done so. It is, in fact, a case of original dietetic sin, fish being the pathological apple

> whose mortal taste
> Brought death into the world and all our woe.

It is interesting, however, to observe that the Maoris in New Zealand attribute the peculiar form of leprosy from which they suffer to eating a small carp which exist in large numbers in a diseased state in the lake of Taupo. White, of Selborne, attributed the disappearance of leprosy from this country to "improved agriculture and an abundant supply of fresh food and vegetables." Beyond all question good food is an essential factor in the preservation of health, but it does not follow

that bad food is the cause of leprosy. The same may be said as to hygienic conditions. Malaria may conceivably predispose to leprosy, as to other diseases, by weakening the constitution—or, as modern pathological *illuminati* phrase it, "lessening the resistance of the tissues to the attacks of microbes"—but there is nothing to show that it has any more direct effect. The gratuitous "primary dyscrasia" of Danielssen and Boeck; the less mysterious but not less gratuitous "defect in development of certain elements in the skin" of Vandyke Carter; the "absence of potash in the blood" of Hjaltelin (of Iceland), and the "absence of salt in the food" of Munro, are all very pretty theories as they stand, but they are rather to be admired than adopted. At present the bacillus holds the field; but that "fearful wild fowl" must be known in all its ten "categories" before our acquaintance with it is likely to prove of practical service. Especially one would wish to know whence it comes. At the present time there is a tendency to trace disease germs to our "poor relations," but animals can hardly be held accountable for the manufacture of leprosy, as it is very doubtful whether they are themselves susceptible of it. It would be curious, however, if after all the bacillus of leprosy should be traced to fish; I am not aware that it has ever been looked for in them, or that any attempts have been made to inoculate them with the disease.

If, however, the origin of leprosy is still one of the great unanswered riddles of the universe, there is not the same mystery as to the mode of its transmission. The evidence in favor of contagion is to my mind quite overwhelming. The contagiousness of the disease was never doubted till it had nearly died out; men ceased to believe in contagion when they no longer saw daily instances of it. The whole system of medical police by which leprosy was finally driven out of Europe was based on the notion that it was contagious, and no measures not based on that principle have ever had the slightest effect in checking its ravages. The alarming spread of this loathsome pest in recent years is in my opinion due to the fact that for some time the opposite doctrine gained the ascendency, and held captive the minds of men. For this pernicious error, and for all the disastrous consequences that have flowed and continue to flow from it, the Royal College of Physicians of London is chiefly responsible.

Though the miserable story is too well known to those who are interested in the subject of leprosy, it may be well briefly to recall the facts. In 1862, some alarm being felt as to the spread of leprosy in Barbadoes, the Colonial Office requested the College of Physicians to draw up a series of questions relative to the nature, causes, and prevalence of the disease. A form containing these questions was circulated throughout the colonies, and upward of 250 replies were received from medical men in different parts of the world, exclusive of those from Her Majesty's Consuls and of communications from the governors of British colonies. The mass of information thus obtained was then submitted to the college, which undertook, apparently with a lightness of heart worthy of M. Émile Ollivier, "to collate, digest, and report upon" it.

A committee was chosen by the college to discharge this important public duty, but there is every reason to believe that only two of the physicians on the committee had the smallest practical acquaintance with leprosy. One of them, the late Dr. Owen Rees, had met with one remarkable case, and another, Dr. Gavin Milroy, paid a hurried visit to Demerara, where he was egregiously hoaxed even by dull-witted lepers. (A striking instance related by Dr. Hillis ["Timehri," June, 1889, p. 79] will suffice: "One man stated to Dr. Gavin Milroy when he was in Demerara, at the penal settlement, that he believed his leprosy arose from the salt diet the prisoners are accustomed to, whereas in fact his wife had suffered from leprosy previous to his being sentenced to penal servitude, and a child of this same man and woman died at the leper hospital at Mahaica." But what must always discredit Dr. Milroy's judgment is the incredulity with which he received Dr. Hillebrand's account of the outbreak of leprosy in the Sandwich Islands: the subsequent course of events is a terrible commentary on the inability to appreciate facts "gross as a mountain, open, palpable," which inspired the unlucky manifesto of 1867.) The mental condition of the other members of the committee must have been *tabulæ rasæ* of ideal blankness on everything connected with leprosy. One can only marvel at the sublime self-confidence with which such a body—seeing as it did with only one eye, and that, as will presently be shown, a dim or distorted one—undertook to guide the footsteps of Government over ground so difficult even for the

clear-sighted. The results of their labors was the "Report on Leprosy" published in 1867, an ill-starred document which has probably done more to propagate the disease than any other single agency since the crusades. In that report the "Patres Conscripti" of English medicine expressed the following opinion, each individual sentence and clause of which is absolutely and demonstrably erroneous:

"The all-but unanimous conviction of the most experienced observers in different parts of the world is quite opposed to the belief that leprosy is contagious or communicable by proximity or contact with the diseased. The evidence derived from the experience of the attendants in leper asylums is especially conclusive on this point.

"The few instances that have been reported in a contrary sense either rest on imperfect observation, or they are recorded with so little attention to the necessary details as not to affect the above conclusion.

"That leprosy is rarely, if ever, transmissible [in married life], when one of the parties has no tendency whatever to the disease, is the opinion of the great majority of the respondents who have had the largest opportunities of observation."[13]

If this deliverance had been merely a theoretical opinion promulgated by the College of Physicians for the edification of the few medical men in this country who concern themselves with its utterances, no one would have had any right to complain. But in this case the decision that leprosy was not contagious led at once to practical consequences of the most far-reaching importance. The measures devised by humane and enlightened statesmen for the mitigation of the scourge were abandoned; the leper-houses throughout Her Majesty's dominions were thrown open, each discharging its measure of pollution into the stream of healthy life near it; and a general relaxation of sanitary discipline with regard to leprosy supervened. (As evidence of this I need only cite two instances, not more flagrant than hundreds of others, but which happened to be the first that come to hand. Dr. Munro says ["Edin. Med. Journ.," vol. xxv., p. 424]: "It is sad to think that in any colony of England a leper should be allowed to keep a school, as I have seen to my horror in St. Kitt's. In misgoverned Crete such things might be, but done in an English colony, with the tacit sanction of the government, instructed by the

Royal College of Physicians of London as to the non-contagious nature of the disease, the latter acting on utterly worthless negative evidence—so done, such an affair is a disgrace to humanity!" Again, take the following plain unvarnished tale from Dr. Hillis ["Timehri," June, 1889 p. 80]: "A respectable young lad became leprous through, as I believe, playing with a boy who had leprosy and who lived further down the street. When seen he was in an advanced stage of tubercular leprosy, covered with sores, and he was sent home and treated by the late Sir Erasmus Wilson, and the family left the house. I subsequently learnt that another family shortly took over the same house without its having undergone any purification or disinfecting, and it must be remembered the boy had been confined for months to one room and was covered with these sores. Had he died of some endemic disease considered contagious, but questionably so, how much money would have been spent on painting, papering, etc., by the authorities! But in the case mentioned no such steps were considered necessary. I am not finding fault with disinfection as carried out in Georgetown—far from it; it is merely contended that, owing to the opinions that the Executive have to guide them" [*i.e.*, the Report of the Royal College of Physicians], "they would not be justified under the present rulings on the subject in going to any expense under the Public Health Ordinance, as leprosy is not considered a contagious disease.) It may without much exaggeration be said that if leprosy slew its thousands before, it has slain its tens of thousands within the confines of the British Empire since 1867. Even outside the limits of Her Majesty's sway the evil effect of this decision has been felt, for the authority of an institution which was supposed to be the concrete embodiment of medical science in this country necessarily had great weight on the minds of some foreign practitioners. That this unfortunate "Report on Leprosy" did not do still more harm is only due to the fact that the dangerous doctrine which it was intended to enforce was not universally acted upon; the practical common sense of mankind in many places where leprosy has its home refusing to be led astray by theoretical opinion. Of course I do not blame those responsible for the report for not having known better; they acted according to their lights, and it can hardly be imputed to them as a crime that these "lights" proved

ignes fatui to those who looked to them for guidance. What they cannot, however, be readily absolved from is the having undertaken to decide a question with which they were quite incompetent to deal.

But if the judges were incompetent, it is no less certain that many of the witnesses were untrustworthy. The answers received to the questions in the circular afford ample proof that many of the respondents knew little and cared less about the disease. The truth is that the mystery still surrounding leprosy is in great measure due to the want of careful study by a sufficiently large number of observers. The subject is so repulsive that it has been, and still is to some extent, "segregated" by medical men from their mental purview as a pariah among diseases. But while positive evidence of contagion of the most striking kind is contained in the very Report[14] which denies its existence, the college, forgetting the cardinal principle that even one well-attested fact outweighs any amount of negative statements, seems to have settled the matter by the simple expedient of counting rather than weighing the opinions submitted to them. One is reminded of the story of the Frenchman accused of stealing a horse, who, when the judge said, "Three witnesses saw you do it," promptly replied, "Ah, sir! three thousand could easily be found who did not see me."

Let us now consider the conclusions of the report somewhat more closely. The first statement, that "the all-but unanimous conviction of the most experienced observers in different parts of the world is quite opposed to the belief that leprosy is contagious or communicable by proximity or contact with the diseased," is to-day exactly the reverse of the truth. For this revolution in opinion we are indebted chiefly to the report itself. The enormous increase of leprosy consequent on the free trade in the disease which followed the appearance of that document opened the eyes even of fanatical non-contagionists. It is a fact of the greatest significance that almost without an exception the men who know most of leprosy, who have lived in the midst of it, are those who believe most firmly in its contagiousness. This is true not only of medical men, but of the missionaries and others who tend the unfortunate sufferers. Such a consensus of opinion is not to be lightly set aside. *Securus judicat orbis terrarum;* the

instinct of mankind has more than once been right in the domain of medicine, when science was at fault.

It was not long before the theoretical conclusions enunciated with such self-satisfied optimism by the framers of the report were rudely shaken by the stern logic of facts. The tide of leprosy, as if in mockery of these medical Canutes, rose ever higher; proofs of the contagious character of the disease were forthcoming on all sides;[15] and it became only too evident that in seeking counsel from the College of Physicians the Government had placed its trust in a hopelessly futile oracle.

The "pity of it" seems all the greater when it is remembered that the medical profession in the British Empire could at that time have furnished plenty of distinguished men who had given much attention to leprosy, and had seen it in its worst haunts. A thoroughly competent tribunal of experts could have been formed, from whom a report of permanent value might have been obtained.

Proceeding to the next point—viz., that "the evidence derived from the experience of the attendants in leper asylums is especially conclusive on this point" (non-contagion)—I may remark that, granting for a moment that negative evidence on a matter of this kind is worth anything at all, the argument proves far too much. Exactly the same thing may be said of the attendants in Lock hospitals. The same argument was used to controvert Koch's view as to the contagiousness of consumption, but was rightly held to have no weight. It is not true, however, that attendants on lepers are never attacked. The college might profitably have recalled the historical problem submitted to the Royal Society by Charles the Second before building an argument on an imaginary foundation. Its own report contains several instances in which persons who had the care of lepers contracted the disease. The most striking of these is the case of Dr. Robertson, medical superintendent of the Curieuse leper establishment, Seychelles,[16] but there are others equally remarkable.[17] Several cases of the same kind have also been recorded by Hansen,[18] and Father Étienne.[19] One of those mentioned by the latter observer is that of Dr. Goddard, a young French physician, who, in order to demonstrate in his own person the non-contagiousness of leprosy, went to Palestine and took up his

abode in a lazar-house. The poor fellow fell a victim to his scientific enthusiasm, and died of leprosy in a few years. Other cases are reported from South America.[20] But what need is there for the laborious collection of such facts when there is the grand object-lesson of Father Damien's life and death before the whole world? Dr. Hoffmann, the medical officer of Molokai, has recently fallen a victim to his devotion, and it is said, though I know not with what truth, that one of Damien's colleagues is now also a leper. In addition to all this we have the crushing fact that, out of sixty-six kokuas, or helpers, on the island in 1888, twenty-three were known to have contracted the disease, while in eleven more its existence was suspected. I have taken some trouble to show that the supposed immunity of attendants is a pure myth, for it is really the corner-stone of the vast edifice of error erected by the College of Physicians in 1867.

The argument from married life has just as little foundation. Several illustrations of contagion between husband and wife are given in the report itself, and others are brought forward by Frère Étienne[21] and Hillis. The former relates the case of a Venezuelan lady whose husband died of leprosy. Six years afterward she herself became a leper. One of Hillis's cases is particularly instructive. A shopkeeper whose business often took him to the asylum became leprous. For ten years his wife remained free from the complaint. The case was well known, and Dr. Hillis was frequently asked to explain how it was, if leprosy was contagious, that she had escaped so long. In the course of time, however, she became an undoubted leper.[22] The moral of this striking case is that contagion would be more often noticed if suspected persons could be kept sufficiently long under observation. In precisely the same way the case of Keanu, the Hawaiian convict inoculated by Dr. Arning on the 5th of November, 1885, used to be cited as a proof that the disease could not be transmitted in that way. The poison took its own time, however, and the man is now an undoubted leper.

But leaving all other questions out of consideration, the crucial question remains: If leprosy is not contagious, how is it that it spreads? Granting that heredity plays a certain part in its transmission, the sudden outbreak of the disease in races previously altogether free from it can hardly be ex-

plained by ancestral proclivity. The College of Physicians had before their eyes the great fact of the invasion of the virgin soil of Hawaii by leprosy, but, like their prototypes in "Tristram Shandy," "they concerned themselves not with facts—they reasoned." We may suppose them to have argued that, as leprosy is not contagious, therefore it cannot spread from one nation to another. All evidence to the contrary was dismissed as impertinent. Sterne must have foreseen these learned Thebans when he described the disputations of the Strasburg doctors: "'It happens otherwise,' replied the opponents. 'It ought not,' said they."

Nothing which the College of Physicians may do in the future can wipe out the effects of their report, or the incalculable misery which it has caused. When the public mind at last was aroused on the subject, just twenty years after the issue of the first report, a second one appeared (July 15th, 1887), in which it is admitted that there is a case for inquiry! I venture to suggest that, by way of making some reparation for the past, they should send some of the men of light and leading among them to see for themselves what leprosy is, and to study it, not vicariously through the eyes of others, but face to face in its native haunts. What is wanted is that the full light of modern medical science should be thrown on the dark places where the monster lurks. If some of the eminent scientific men who were invited to join the Marlborough House Committee could study leprosy in its home, great results might be obtained. If an experienced pathologist like Sir James Paget could spend some time in India, important facts as to the nature of leprosy might be discovered; or if a distinguished physician like Sir Andrew Clark could have five years at Molokai, he might perhaps slay the sphinx of leprosy, or at any rate evolve a system of diet calculated to be of service to those subjected to the contagion of that dire disease. To send out from time to time a young doctor on a scientific "Wanderjahr" among the lepers, as proposed by the Marlborough House Committee, seems to me mere trifling. Is it likely that a fledgling just escaped from the academic nest should "come, see, and conquer" where Danielssen and Boeck, Vandyke Carter, Hansen, Munro, Arning, and Hillis have failed?

Another plan which might lead to some discovery of real

importance would be the offer of a prize sufficiently large to tempt men of the highest eminence to compete. Mr. Macnamara suggests[23] that the Damien Memorial Fund should offer "a prize of £500, open to all comers, for the best essay and original research regarding the bacillus lepræ." This is a step in the right direction, but it is not enough. M. Pasteur received something like £20,000 for his discoveries in the silkworm disease. If he could only be induced to grapple with the leprosy problem, there might be a chance of a "protective virus" being discovered which should make people exposed to the contagion of leprosy invulnerable to its attack.

In the mean time the only way of coping with leprosy is to deal with it as a thing dangerous to mankind. It would be criminal to allow the scourge free play because academic pedantry is not satisfied as to the exact mode of its transmission. No half measures will suffice: *écrasez l'infâme* in a new sense must be the motto of those intrusted with the task. Our forefathers did not allow themselves to be disturbed by "philosophic doubt," but stamped out the pest by a system of "Thorough" which Strafford might have envied. The sick must be kept strictly apart from the healthy, and all suspicious cases should be detained in quarantine and jealously watched. For this purpose special medical inspectors should be appointed, each with a defined district under his constant personal supervision. I should not be disposed to insist on the separation of married couples; lepers are not prolific, and hereditary contamination has now been shown to be almost a *quantité négligeable.* (Dr. Armauer Hansen, who recently visited the Norwegian lepers in Wisconsin, Minnesota, and Dacota, found that the offspring of 160 lepers who had emigrated to America had remained free to the third generation. ["Archiv für Dermatologie," 1889, Heft iii.] Again, there is the fact that, although during eighteen years 2,864 persons were consigned to Kalawao [Molokai], only twenty-six children were born during that period, and of these only two have become leprous.) There is, of course, the risk of contagion; but if husbands or wives are willing to encounter it, I do not see why they should be prevented. We have the testimony of Father Damien to the good effect of allowing married people to remain together.[24] There must always be a certain amount of hardship in segregation, but if lunatics can be made com-

fortable and even happy in confinement, so may lepers. A vast amount of nonsense has been talked about the horrors of segregation; if there is any discomfort beyond the separation from friends and the suffering caused by the disease, it is either due to neglect on the part of the authorities or to want of funds. The lepers I saw in Norway were, with the exception of those in the very last stages of the disease, clean, cheerful, and busy—the men in the workshops, the women at domestic work, and the children in their classes. The last was certainly a painful sight, but the little patients themselves were not at all gloomy. Those I saw at Seville were less cheerful, but that may have been due to the quasi-monastic atmosphere which surrounded them. Dr. Webster, however, says[25] that the lepers whom he saw at Grenada were quite a "merry family," dancing, twanging the guitar with their crooked stumps of fingers, and warbling ditties with such remnants of voice as were left them. Frère Étienne's testimony as to Trinidad is not less striking (op. cit., pp. 254–55. "La tristesse n'est pas du tout, comme ja l'ai lu plus d'une fois, un effet inévitable de la maladie. Je puis bien attester qu'il n'y a pas au monde école ou collège plus gai, plus bruyant, où l'on joue et babille plus, qu'à Cocorite. Tout est oublié; certainement on n'est pas un lépreux, on est un pensionnaire; et la reine est bien honorée d'héberger de tels personnages."), and Mr. Clifford tells us that even at Molokai there is little sign of unhappiness.

Apart altogether from the *salus populi*, which must ever be the supreme law in such matters, there can be no doubt that segregation, if properly carried out, is the best thing for the sufferers themselves. If I had the misfortune to be a leper myself, I should prefer to be where one touch of nature makes the whole world kin, though it were only by followship in suffering, to being an object of horror to all around me. Medicine, though it cannot cure, can do much to mitigate the incidental miseries of leprosy, and nowhere can treatment be so well applied or the necessary nursing so intelligently carried out as in places where special experience has engendered special skill. Sanitary arrangements must be of the highest attainable perfection, as the concourse of foul smells in leper-houses is especially apt to breed disease, not only among the inmates but among those in charge of them. The food must

be abundant and nutritious, and plenty of occupation should be provided for such as can work. Nor should amusements be neglected. It was by attention to all these things that Father Damien was able to humanize the poor outcasts for whom he gave his life; by these means he transformed a sink of moral as well as physical corruption into a peaceful and happy community. There is no reason why this memorable example should not bear fruit wherever lepers are to be found. There is no lack of self-sacrificing men whose hearts are filled by the enthusiasm of humanity; what the Catholic Church beautifully calls the "devout female sex" will always be ready wherever nurses are needed; and of doctors we shall never fall short when there is such a field for scientific discovery. The only thing wanting is money. The stamping out of the most grievous disease which flesh is heir to will no doubt be an expensive undertaking; but is this great and wealthy country to count the cost when poor States like Norway and Hawaii are lavishing their scanty treasure in furthering the good work?

It is to be feared that the wave of philanthropic sentiment which, under the combined influences of pity, fear, and fashion, rose so high a few months ago, is already subsiding. De Quincy gauged the English character correctly when he said that, so far from being phlegmatic, we are the most excitable people on earth; unhappily our excitement, which is so easily fanned into a blaze, burns itself out all the more quickly. As Macaulay observed, our virtue goes to sleep for several years after one of our periodical outbreaks of morality. Let not the same be said of our philanthropy! It will be deplorable, and indeed disgraceful, if our desire to do something to check the advance of leprosy is allowed to die out with the "scare" which engendered it.

BIBLIOGRAPHY.

1. Handbook of Geographical and Historical Pathology. By Dr. August Hirsch. Translated from the second German edition by Chas. Creighton, M.D.—2. Edinburgh Medical Journal, vols. xxii., xxiii., xxiv., xxv.—3. El Siglo Medico, Oct. 21st, 1883.—4. Dr. Ph. Hauser, Estudios Medio-Sociales de Sevilla. Madrid, 1884, p. 319.—5. Brit. Med. Journ., July 23d, 1887.—6. Loc. cit.—7. Letter quoted by C. N. Macnamara, Leprosy a Communicable Disease, 2d ed., London, 1889, p. 61.—8.

Ibid.—9. For the information above given relative to Trinidad, I am indebted to the work already cited, La Lèpre est Contagieuse, Trinidad, 1879, p. 264 et seq.—10. John D. Hillis, F.R.C.S.I., late Medical Superintendent of the Leper Asylum, British Guiana, Timehri, June, 1889, pp. 77, 78.—11. Father Damien, London, Macmillan & Co., 1889, p. 153.—12. British Medical Journal, Aug. 3d, 1889.—13. Report on Leprosy by the Royal College of Physicians, prepared for Her Majesty's Secretary of State for the Colonies, London, 1867, p. lxix.—14. I need only cite the evidence of Drs. Goding and Stevenson, of Barbadoes (Report, p. 32), Dr. Aquart, of Grenada (ibid., p. 36), Drs. Manget, Reed, Pollard, Duffey, Carney, and Van Holst, of British Guiana (ibid., pp. 45–46), the Proto-medico of Corfu (ibid., p. 67), Drs. Regnaud and Bolton, of Mauritius (ibid., p. 86), the Civil Commissioner of Seychelles (ibid., p. 90), and Mr. Macnamara, then of Mozufferpore (ibid., p. 141).—15. In 1869 Drognat-Landré supported the contagionist doctrine in a powerful work (De la Contagion seule Cause de la Lèpre). In 1873 the "conqueror worm" which carries the infection, was discovered by Hansen. In 1874 Dr. Vandyke Carter, whose investigations in nearly every part of the world where leprosy is found entitle him to rank as the foremost living authority on the disease, was driven by the facts which he himself had collected to "find salvation" in the contagionist fold. A few years later the *coup de grâce* was given to the Report by Dr. W. Munro, formerly medical officer of St. Kitt's, in the West Indies, in a series of papers (Edin. Med. Journal, vols. xxii., xxiii., xxiv., and xxv.) which form a storehouse of facts from which every subsequent writer has freely drawn. I need only mention two other works which have been several times referred to in the course of this article, and in both of which the same conclusion is enforced—Frère Étienne's little book (La Lèpre est Contagieuse, Trinidad, 1879) and the magnificent monograph of Mr. J. D. Hillis (Leprosy in British Guiana, London, 1881). In 1884, Miss Agnes Lambert published an excellent article (already referred to) in The Nineteenth Century (August and September, 1884). In 1887, Dr. Besnier brought additional proofs of the contagiousness of leprosy before the Académie de Médecine in Paris, while in this country Archdeacon Wright sounded the alarm as to its increasing prevalence throughout the world (Times, Nov. 8th, 1887).—16. Report on Leprosy, p. 90.—17. See replies by Drs. Goding and Stevenson, p. 32; Dr. Mantell, p. 41; Mr. N. C. Macnamara, ibid.; Mohamed Naeem, p. 177; and Surgeon-Major J. Rose, p. 199.—18. British and Foreign Medico-Chirurgical Review, April, 1875.—19. La Lèpre est Contagieuse, p. 122 et seq.—20. Boletin de Medicina del Cauca, No. 1, p. 13 (Dr. Tenorio), and p. 16 (Dr. Escobar).—21. Op. cit., p. 95.—22. British Medical Journal, Nov. 5th, 1887.—23. Leprosy a Communicable Disease, 2d ed., London, 1889, p. 61.—24. E. Clifford: Father Damien, p. 82.—25. Medico-Chirurgical Transactions, vol xliii., 1860, p. 29.

Diseases of Old Age.

BY

Dr. A. SEIDEL,

Berlin.

DISEASES OF OLD AGE.

CHAPTER I.

THE life of the individual forms a complicated chain of events, the harmonious working together of which we indicate by the term, "health." In the "struggle for existence" this harmony is often disturbed without the continuity of life being broken by death. In so far as the disturbances affect the constituent parts of the organism, depress the organic functions below the norm, and stand in a peculiar causal relation to each other, we call them "disease."[1] According to this explanation, pathology is simply a part of physiology. Normal life and disease often mutually assimilate each other; disease is taken up into life itself and becomes one with it; life is mutilated.[2] The intensity of this mutilation, *i.e.*, the reaction of the tissues, depends not alone upon the intensity of the "irritant," but especially upon the condition of the organism—upon its "irritability" or "predisposition." One of the most weighty of the predispositions to disease is old age,[3] which, however, must be looked upon as much as a condition for the predisposition to disease as a cause itself of disease. The organic changes and the anomalies of function can no longer be regarded as foreign to, but as essential conditions of, the senile organism (Canstatt). The process of change becomes a morbid process, which Canstatt has termed "senile involution." We can speak of a normal, physiological old age only so long as the retrograde changes, which the tissues always undergo in advanced life, have not yet become apparent. In the following pages we shall consider first the diseases peculiar to this period of involution, and then the modifications which the ordinary diseases undergo at this time, and finally formulate certain hygienic and therapeutic indications.

We employ the term "senile marasmus" or "senile ca-

chexia" to denote the picture presented by the sum total of all these peculiar marks of old age.[4] The progress of involution in the different organs is an irregular one, it begins now in one system, now in another, while the other organs are still in a normal condition. The chief characteristic of the diseases peculiar to old age is, says Canstatt, their isolation; they are, as it were, entirely cut off from the rest of the organism. At the end of a long life the individual dies piecemeal.[5] Senile involution may set in early in life (senium præcox), and is then due to any one of a great variety of causes, such as sex, temperament, constitution, food, occupation, excessive bodily or mental strain, depressing external circumstances, etc. General paralysis, indeed, is a precocious senility of the psychomotor brain-life, modified by a complicating neurosis of the sympathetic (Schüle). We ought not to measure age by the number of years, but rather by the condition of the vital powers. These organic changes declare themselves gradually and consist in a retreat of life from the periphery toward the centre. They are known as involution in the aged and as evolution in the child.

The question now presents itself whether a law is discoverable by which this process may be explained. Canstatt regards the cause of the senile changes as resident in the nature of the cell, which holds in itself the conditions of its life and of its destruction. But those cells which go to make up the body at its birth are not all present in adult life and have probably entirely disappeared in old age. There is in man an almost constant molecular renovation, the cessation of which in old age cannot be explained by any inherent quality in the cell.

Durand-Fardel[6] believes that the plastic force begins to grow weaker at the climacteric period, and that it changes its object in the generation of pseudoplasma (heteroplasie). This view is, however, open to criticism, since sexual activity is not necessary to the life of the individual. We must therefore, in addition to these internal conditions of vitality of the organic cells, admit certain external causes, dependent, according to Demange,[7] upon interstitial nutrition. This author asserts in proof of his theory that the local and general changes are of the same character, a fact which would point to a common cause. According to him, the essential feature of senility is a change in the quantity and quality of the inter-

stitial nutritive material, the cause of which change he finds in an alteration in the conditions of the circulation induced by the atheromatous process.

"Atheroma results from a vital use of the organs; repeated frictions give rise to the milky patches on the endocardium; repeated pressure on an organ, as the liver or the spleen, leads to a thickening of its capsule; it is then not impossible that the repeated rubbing of the blood against the inner surface of the small vessels may excite finally an irritation of the lining membrane, whence comes endarteritis, and then follows atheroma with its consequences upon the partial nutrition of the organs and the general nutrition of the individual" (Demange).

In 500 autopsies on old people, in which not only the gross changes were noted, but even the finest arterial twigs were studied under the microscope, Demange was able to discover in every instance the presence of endarteritis. We see atheroma, indeed, as a result of chronic alcoholism, diabetes, gout, rheumatism, syphilis, and poisoning by lead, mercury, or tobacco; and it is also produced by the acute infectious diseases, such as typhoid fever, measles, and scarlatina; but the chief and most frequent cause is old age. In aged animals also atheromatous arteries are always found. The above-mentioned influences act in the same way as old age, and thus may give rise to a precocious senility. In his investigations, Demange excluded all those cases in which any complications were present, in order that he might determine the conditions dependent upon advanced age alone.

The primary change which is produced by the "functional wear and tear" consists in an obliterating endarteritis of the vasa vasorum, and this in turn produces a sort of necrobiotic infarct in the wall of one of the larger vessels.[8] The endothelium of the smallest vessels proliferates and the intima becomes considerably thickened, in consequence of which the lumen of the vessel is narrowed. Sometimes a fibrinous thrombus is formed. Following upon the endarteritis a swelling of the muscular layer takes place, and finally the external tunic also becomes inflamed, so that a periarteritis is added to the previous condition. The entire process is called arteriosclerosis. The necrobiotic cells in the area supplied by such a degenerated nutrient artery excite a secondary condition of irritation, the result of which is fatty, atheromatous, or cal-

careous degeneration. We give to the entire process the name of atheroma, although atheromatous degeneration is only a part of the disease.

Added to these circulatory disturbances in a certain vascular area are similar disturbances in an entire organ or system of organs, which are brought about through the agency of the capillary vessels that are filled with fatty granules.[9] Interstitial nutrition is interfered with in consequence of the capillary lesions and a speedy death of the elements results. These disturbances of nutrition destroy the power of reproduction of the cells of the parenchyma, the cells themselves break up and lose their plastic force. This atrophy is shown by loss in weight, decrease in size, and other external changes. The degenerative processes include within themselves alterations in structure and in chemical composition. We have to do with fatty, granulo-fatty, and amyloid degeneration and with pigmentary and calcareous infiltration.

At the same time with the atrophy and degeneration of the cells of the parenchyma we find deep-seated changes in the connective tissue. The plastic force becomes once more active, but the newly-formed cells assume the embryonic form. Charcot[10] speaks of a connective-tissue hyperplasia of dystrophic and epithelial origin, and regards the process as a sclerosing one. This sclerosis extends more and more widely, includes all the elements, and so gives a new impetus to atrophy and degeneration. The sclerosis is an irritative process which probably is excited by the presence of the atrophied and degenerated cells.

Demange looks upon the sclerosis as of perivascular origin, and believes that its starting-point is the periarteritis. Frequently also the sclerosis seems to start from the inner side of the capsular investment of the glands and to extend more or less widely through the glandular tissue.

The nature of the senile alteration is, therefore, an hypoplasia of the elements and a hyperplasia of the connective tissue. Seiler[11] says justly, "Rigiditas senectutis perpetua comes." In proportion as age advances the power of reaction diminishes, and nature's reparative force grows daily weaker. Here art must interfere actively, for a passive and expectant therapy is no longer in place. There is certainly no need to dwell upon the necessity to the practical physician of an exact

knowledge of the period of involution. Mistakes are made daily in the treatment of the aged, and the normal mortality of advanced life is considerably increased as a consequence of the hitherto neglected study of the anatomical and physiological peculiarities of the senile organism. Old age is properly the age of infirmity, and at that period it is often difficult to define accurately the limits between health and disease. The prolongation of life will receive the most thankful recognition at this time, for we learn to prize most those possessions the speedy loss of which we have cause to apprehend.

We have seen in this general review of the nature of old age that senile atheroma is, in the wider sense, the primary cause of the degenerative changes, including sclerosis. The anatomical conditions which we meet with, more or less clearly defined, in the various organs and organic systems are then· atrophy, degeneration, and sclerosis, endoperiarteritis of the vasa vasorum, and atrophy and degeneration of the capillaries.

Atheroma does not always give rise to manifest symptoms; its progress is slow and unmarked until something occurs to bring it prominently to view, or until it gives rise directly to some other pathological condition, such as thrombosis, hemorrhage, or gangrene. After each attack the affected organs recover incompletely, and their weakened state and the depression of their functions become ever more apparent.

We will now pass briefly in review the senile changes as they are manifest in the individual organs.

The Heart.

The senile heart combines in itself the arterial and venous changes. The right, or arterial heart, suffers from disorders of nutrition in the endocardium and muscular walls[12] following upon arterio-sclerosis of the coronary arteries. The lesions so caused are analogous to atheroma of the larger arteries, and consist in colloid thickening of the intima, *i.e.*, of the endocardium, yellow or fatty plaques, and spots of atheromatous softening or of calcareous deposit. The deeper layers of the endocardium (intima) are always first affected, and the changes are distinctly localized in definite areas corresponding to the nutrient arteries. The disturbance of nutrition leads to a loss in elasticity, to dilatation (aneurism), embolism,

and thrombosis. The fatty degeneration of the myocardium and the infarcts in the ventricular wall lead sometimes to rupture of the heart. So-called angina pectoris stands also in relation with sclerosis of the coronary arteries. In addition to these changes, which correspond to atheroma of the larger arteries, we have also the compensatory disturbances dependent upon the mechanical conditions present in the heart. In consequence of the impeded circulation, caused by the shutting off of large capillary areas, there arises a secondary hypertrophy of the heart,[13] in which, however, only embryonic cells are formed ("sclerotic myocarditis"). Sclerosis affects with preference the left heart, while granulo-fatty degeneration is more marked in the right.[14] At the same time milky patches are formed on the pericardium as a result of the repeated friction of the heart against neighboring organs. Finally the mitral and aortic, and less often the tricuspid and pulmonary, valves become thickened and deformed. All these changes are then the product of two factors, an irritation of moderate intensity and the constant repetition of the same through a long series of years. As this process becomes intensified disease must of necessity follow.

In autopsies on old people the increase in weight and volume of the heart, in which both ventricles participate equally, is almost constantly found. The atrophic heart, which is sometimes seen, is not due to senility but to some cachexia resulting from cancer, tuberculosis, dementia, chronic diarrhœa, etc.

The common characteristic of the circulation in the aged is a retardation in the venous system, which Canstatt has denominated "venosity." The cardiac hypertrophy by itself is insufficient to maintain the relative equality of the two circulatory systems, and the added assistance of an increased pulsation is needed. The pulse rate in the aged averages 77 per minute.

Some of the clinical symptoms of the senile heart may give rise to error. The wasting of the chest wall, which is usually present, causes the apex beat to be more apparent on palpation and it seems to be stronger and also somewhat longer. The apex is almost always lower, being felt in the sixth intercostal space. The increase in the vertical and transverse diameters is readily made out on percussion, and a dilatation of the arch of the aorta is recognized by the same means. The

first sound of the heart is frequently somewhat rough, dry, and prolonged; the second is stronger, more distinct, and sonorous. These point to a beginning hypertrophy and to a certain rigidity of the valves. Irregularity of the contractions and intermittence indicate sclerosis and commencing degeneration of the myocardium. Reduplication of the first sound is dependent as well upon simple senile atrophy of the kidneys as upon interstitial nephritis. Its causation is to be referred either to altered muscular tonus, the contractions of the two ventricles not occurring synchronously, or to a want of synchronism of the other sounds, resulting from relaxation and stretching of the mitral and tricuspid valves (Peter, Demange). The reduplication of the second sound, which is also observed in the aged, is accounted for by the rigidity of the aortic valves (Potain). A systolic murmur at the aortic orifice is often not the result of stenosis of the aortic valves, but is produced in the dilated aorta, which forms a cavity in which the arterial tension is lowered (Peter, Demange). The roughened first aortic sound is caused by the roughening of the aortic orifice.

During a considerable period the patient feels a slight oppression only on rapid walking or while ascending stairs, but often, especially in the evening, he notices a moderate œdema of the ankles, which will be found to have disappeared the following morning after rest in bed. The heart is still able to cope with its difficulties, which, however, increase day by day. This period of tolerance has, nevertheless, its limits, and the heart is finally incapable of bearing up under its load; then comes the stage of intolerance. The danger of asystolie ever menaces the heart of the aged. Any interference of the general nutrition hastens the progress of the fatty degeneration. A simple febrile affection, a prolonged period of inactivity whereby the nutritive functions are depressed, a long confinement to bed necessitated by the slow healing of some wound, a simple fracture, some strong emotion, a moderate catarrhal jaundice, or any other slight disturbance may hasten the degeneration of the heart muscle and lead to asystolie. Sometimes a bronchitis (a very common affection in the aged) or pneumonia (usually in its latent form, one of the most frequent causes of death in the aged), which overburdens the pulmonary circulation, furnishes the exciting cause of asystolie. But even without these exciting causes death will occur from ex-

haustion so soon as the vital energy sinks below the point where it can effect the molecular changes of matter with the outer world.[15] More frequently there are actual valvular lesions which sooner or later lead to heart failure. The heart struggles on the longest and ceases to beat only when it no longer finds in the organism the conditions necessary for its contraction.[16] These conditions cease to be active when the irritability of the respiratory centre becomes so depressed that no dyspnœa can be felt although the supply of oxygen is deficient. The old man dies then without a struggle, he dies from "exhaustion" (Stricker).

Respiratory Organs.

The first of the senile changes in the respiratory system to be considered are those relating to the thorax. The separate pieces of the sternum become ankylosed, the costal cartilages become ossified, and the ribs lose their elasticity, they become flattened, and their posterior angle is increased. The result of this is that the sternum, attached to the ribs by less resilient cartilages, is thrown forward, and the chest assumes the appearance of the rachitic thorax. In consequence of the atrophy of the intervertebral disks the spinal column sinks and a kyphotic curvature is formed. As a result of these changes the thorax loses its elasticity and mobility, and its capacity is diminished.

The muscles of the chest participate in the general atrophy of the muscular system, the pleuræ become opaque and thickened, and adhesions are formed which interfere with the gliding of the lungs over the chest wall. The inspiratory power is thereby diminished, the thorax moves as one piece, and the type of respiration approaches that seen in emphysema.

The alveolar walls (elastic fibres and capillary vessels) atrophy, and communications are formed between the alveoli, giving rise to "senile emphysema."[17] Anthracosis is almost always present in the lungs of the aged. The bronchi show calcareous incrustations, and the cartilages of the trachea and larynx become ossified.

Through the narrowing of the respiratory field the aëration of the blood is less perfectly performed, and the old man breathes like one with emphysema in an atmosphere which is in a certain sense confined. The vital capacity of the lungs is

diminished, and the amount of residual air is increased. This serves to explain the shortness of breath, the easily excited dyspnœa, and the increased frequency of respiration in the aged.[18] The atrophy of the lungs furthermore offers a very marked impediment to the lesser circulation, and thereby increases the dilatation of the right, or venous, heart. We see here an explanation of the frequent congestive disturbances in the aged, of the transient or permanent dyspnœa, and of the hypostatic pneumonia which so often leads to a fatal termination.

Digestive Organs.

The falling of the teeth leads to atrophy of the alveoli, the angle of the jaw becomes more obtuse, and the chemical composition of the teeth approaches that in children, the organic constituents becoming relatively increased while the inorganic are diminished.

The *gastro-intestinal canal* is the seat of important changes.[19] The mucous membrane becomes paler and thinner, the cells and glands atrophy, the capillary network is in part atrophied and fatty degenerated, and the vessels of the stomach and intestine are specially prone to atheroma. The muscular fibres of the middle coat lose their tone and permit of dilatation of the stomach and intestine, particularly of the large intestine.

The *salivary glands* are often found atrophied and dry, but the composition of the saliva remains normal.

The *liver* is notably diminished in size and weight, it is more firm and more yellow in color, and Glisson's capsule becomes cloudy and thickened. The surface of the liver is somewhat granular, and on section the tissue appears paler. This is the gross appearance of the senile liver unaffected by actual disease. But in consequence of the frequency of heart and lung troubles in the aged we often find superadded the lesions of engorgement of the liver. At the same time with the atrophy we see atheromatous changes, the connective tissue is sclerosed (especially at the porta hepatis and on the inner surface of Glisson's capsule), and the liver-cells are reduced in size.

The *gall-bladder* is thickened and adherent to the neighboring parts of the liver, it often contains gall-stones, and the bile itself is thick and contains a greater amount of cholesterin.

The *pancreas* is atrophied and sometimes fatty degenerated.

The *spleen* is reduced in volume and in weight, its tissue is more firm, and the capsule is thickened. The parenchyma is coarser than normal, and the sclerotic changes are pronounced. Very many of the Malpighian corpuscles are atrophied so that the whole organ is reduced to a mass of connective tissue traversed by obliterated blood-vessels.

The *lymphatic glands*, especially those of the mesentery, are atrophied sclerosed, and degenerate[20]

Kidneys.

The senile kidney [21] is diminished in weight and volume. It resembles the contracted kidney (nephritis interstitialis), the changes in which are the same, only in greater degree, as those in the senile kidney; it is granular, pale, and anæmic; its color is red only when, in consequence of heart or lung trouble, it is congested (engorged). There is seldom, during life, any polyuria, albuminuria, or dropsy, the two latter being seen only as a result of engorgement of the organ. The senile kidney has a lobulated appearance, the capsule is thickened and adherent, the parenchyma is hard, and the renal arteries are atheromatous. The glomeruli are sclerosed, the convoluted tubules are also in part atrophied and sclerotic, and similar changes are seen in Henle's loops. In addition to the lesions which these parts themselves present we see sclerosis of the connective tissue advancing in bands and islands, compressing the tubules and glomeruli.

The urine is diminished in quantity, and there is also relative decrease in the amount of the urea, uric acid, chlorides, and phosphoric acid. The causes of these changes are found in the anatomical condition of the senile kidney, in the altered constitution of the blood, and in the products of disassimilation. The renal excretion, which is one of the most important means of disposing of the bodily waste, is therefore retarded in old age.

Urinary Organs.

The alterations in the urinary tract and its adnexa were first demonstrated in their relation to arterio-sclerosis by the Guyon school in Paris.[22] Throughout the urinary system

a fibrous induration takes place which is in relation with the atheromatous changes in the vascular system.

The *ureters* are thickened in consequence of a fibrous degeneration of the muscular layer, and the capillaries show the usual senile lesions. There is often a notable dilatation of the ureter—large enough to admit the finger—which is the result of the obstruction to the flow of the urine, so common in old men, and which may even lead to hydronephrosis.

The *bladder* loses its propulsive force, and in spite of the secondary hypertrophy the contractile power is insufficient to overcome the increasing opposition, for the apparently hypertrophied muscular bands are composed in part of fibrous connective tissue. The prominence of these muscular bundles and the sinking between them of the mucous membrane gives to the inner surface of the bladder the appearance which has been denominated "vessie à colonnes." The depressions between the bands frequently form pockets or diverticula which are never completely emptied of urine. When the muscular fibres become exhausted by the constant strain and undergo fatty degeneration, a passive distention of the bladder occurs. The vesical arteries, like the others in the body, become the seat of endoperiarteritis.

The *prostate* becomes lobulated in consequence of the formation of fibrous rings and bands. The small vessels become encircled by a thick fibrous ring, whereby their calibre is narrowed and sometimes completely obliterated. The organ is notably vascular. The hypertrophy of the prostate (leiomyoma of Virchow), which is present in from 16 to 22 per cent of all old men, is a secondary affection similar to the hypertrophy of the heart and bladder. But there is also a senile atrophy of the prostate which, according to Thompson, is present in 5 per cent, according to Dittel, in 30 per cent of all cases. The incomplete evacuation of the bladder is not, however, due to purely mechanical causes, dependent upon hypertrophy of the prostate, especially of the third, or Home's, lobe, for cases of very marked hypertrophy are occasionally met with in which there are no urinary troubles whatever. The atony of the muscular coat of the bladder, resulting from senile involution, plays the principal rôle in its production. Retention of urine is therefore not a direct symptom of hypertrophy of the prostate, but the latter is rather to be regarded as a complication of the former.

Genital Organs.

Marked changes are seen in the genital apparatus of the aged. The testicle is the seat of a sclerotic process;[23] it is decreased in weight and volume, and is of a firmer consistence. The tunica vaginalis is thickened. Small fibro-cysts and varicose dilatations of the veins are found in the epididymis. The tunica albuginea is thickened, and the connective tissue between the seminal tubules is proliferated and degenerated, the seminal tubules being thereby compressed. The membrana propria of the seminiferous canals is thickened and fibrous, the epithelium is fatty degenerated, and the arteries of the intertubular connective tissue are atheromatous. In about one-half of the cases the spermatozoa are absent, but this is due more to obstruction in the efferent ducts than to any deficiency in their formation.

In women[24] the expulsion of the ova ceases usually at the menopause. The ovaries decrease rapidly in weight and volume, their stroma becomes hardened, the cortical zone atrophies, the Graafian follicles disappear, and the corporea lutea become converted into hard cysts. The uterus loses in weight and becomes more spherical and firmer. The arteries are atheromatous. The Fallopian tubes, and also the cervical canal, are often obliterated. The mammary glands also atrophy.

Skin.

The first changes in the skin and its appendages are manifested by wrinkles and gray hair. Later the skin loses its fresh tints and becomes often dry and lustreless, while the subcutaneous cellular tissue disappears. The cutis[25] undergoes an atrophy of all its elements, the connective-tissue fibres become loose and separate from each other, and the bundles of elastic fibres become fissured, the fibres separating from each other and those nearest the surface becoming granular. In many places pigment granules are found. The papillæ atrophy, as do also the smooth muscular fibrils of the hair follicles and sudoriparous glands. The arteries of the skin are atheromatous and the veins are varicose. It is to the endoperiarteritis that is to be referred the production of the ecchymotic spots so often seen on the backs of the hands and on other exposed portions of the skin in the aged.

The epidermis is thinned, and in the rete Malpighii pigment collects and often leads to the melanoderma of the aged. The epidermis is not infrequently covered with blackish crusts, seborrhœic warts.[26]

The tactile papillæ take part in the general sclerosis of the skin. The sweat glands show no special change. The hairs fall out in consequence of atrophy of the bulbs, and the nails become dry and fissured.

Sclerosis of the drum-membrane causes deafness. The diameters of the crystalline lens become altered, and presbyopia results, the power of accommodation grows less, and the sight is weakened. The transparency of the crystalline grows less and its consistency increases. A faint amber-colored cloudiness often precedes the formation of senile cataract.

A heaping up of fatty granules in the cells of the cornea forms the arcus senilis,[27] which is seen as a light gray ring at the periphery of the cornea. Virchow and others trace a relation between the arcus senilis and fatty degeneration of the ophthalmic artery, of the general arterial system, and of the muscular fibres of the heart.

Locomotor Apparatus.[28]

The osseous changes consist in a senile osteoporosis (not to be confounded with senile osteomalacia) of the spongy tissue of the short bones and of the epiphyses of the long bones. All the spaces in the osseous tissue, and the medullary spaces in particular, are increased in size, while the compact tissue becomes more spongy, the consequence of which is an abnormal fragility of the bones. The periosteum of the long bones becomes dryer, the nutrient foramina are obliterated, and the marrow contains more fat than in adult life. The flat bones of the skull participate in the general senile atrophy (Geist, Kölliker, Virchow). The diploë and both the internal and external laminæ are thinned, and sometimes there is a loss of substance, forming an opening of an oval shape, the edges of which are raised somewhat by reason of an increase of the cells of the diploë. The middle meningeal artery is tortuous and atheromatous.

The sutures of the skull become obliterated. The coccyx becomes fused into one bone with the sacrum, the manubrium

and ensiform process of the sternum unite with the body, and the cornua of the hyoid bone with its body. The intervertebral disks are compressed and the height of the body is lessened. The bodies of the vertebræ may even become fused together. The cotyloid cavity of the hip is enlarged.

The costal cartilages ossify, as do also the cartilages of the larynx and the tracheal rings. The epiglottis becomes fibrous.

The cartilaginous changes consist in a fatty infiltration and calcification of the cartilage cells and also a calcareous infiltration of the perichondrium and basement substance.

The senile joint changes [29] accompany the alterations in the cartilages, bones, and capsular ligament. The articular cartilage becomes fibrillated and worn away, and the capsular ligament is sclerosed and thickened, in consequence of which a partial anchylosis takes place and motion of the joint is accompanied by a grating and creaking sound. The senile alterations in the joints should not be confounded with arthritis deformans. The latter occurs commonly in adult life and is characterized by a proliferation of the cartilage and bone.[30]

The voluntary muscles take part in the general atrophy, and there is sometimes a true fatty infiltration which may lead to complete paraplegia. Fatty infiltration occurs also in the involuntary muscles, in the muscular coat of the arteries, and in the smooth muscular fibres of the intestine and bladder. The muscle and tendon reflexes are less marked and there is a general muscular debility.

Brain.

The senile alterations in the brain [31] result from the circulatory disturbances which cause an atrophy and degeneration of the elements. There is a loss in weight of the cerebral hemispheres, while the cerebellum remains nearly stationary. The brain is also diminished in volume so that it does not completely fill the cranial cavity. The convolutions atrophy, the sulci are widened, and the cerebro-spinal fluid, which fills the subarachnoid space and the ventricles, increases in amount as the volume of the brain grows less.

The meninges become cloudy, thickened, and adherent. The pineal sand is more abundant, and there is ossification of

the corpuscula choroidea and of the Pacchionian bodies. Osseous plates are also found at times in the arachnoid and pia mater.

The amount of fat is diminished and that of water increased; the fluid is found especially in the cortical substance, the fat in the white substance. The absolute quantity of phosphorus is diminished, though relatively it is increased, the loss that takes place being the result of the atrophy of the brain.

Fatty degeneration of the capillaries and atheroma of the arterial vessels are constantly present. Durand-Fardel has given the name of "état criblé" to a condition characterized by the presence of numerous small holes, up to the size of a pin-head, in the white and gray matter which arise from the retraction of the brain substance around the dilated lymph-sheaths. The remains of spots of miliary softening (small spaces filled with a serous fluid) are often seen. The atheromatous process in the cerebral vessels varies from a simple induration and fatty infiltration of the arteries up to a more or less complete calcification of the vessels (Charcot, Bouchard). The endarteritis gives rise to circulatory disturbances and leads to thrombosis and softening.[32] Periarteritis, on the other hand, is more frequently the cause of the formation of miliary aneurisms, the rupture of which produces cerebral hemorrhages. When these accidents do not happen, the presence of atheroma is revealed only by the occurrence of vertigo, sudden fainting spells, unconsciousness, somnolence, and physical and intellectual weakness, conditions which are the clinical manifestations of weakness of the brain following an impeded cerebral circulation.

Spinal Cord.

The senile changes in the spinal cord were first described accurately by Leyden. As in the brain we find also here the decrease in volume of the nervous matter and the corresponding increase in the amount of the cerebro-spinal fluid (hydrorhachis ex vacuo). The consistence of the cord is increased and its color is duller or more gray. The pia mater is cloudy, grayish in color, and thickened, and not infrequently contains little osseous plates.

Leyden has described the frequent presence of amyloid

bodies in the cord analogous to those found by Virchow in the brain. The cells of the anterior horns are so extensively atrophied that the pathological picture presented resembles that seen in progressive muscular atrophy. The arteries are the seat of very marked structural alterations. Frequently we see a zone of sclerosis surrounding the vessels which involves and destroys the nerve fibres. When this disseminated sclerosis becomes more pronounced, a condition arises which Demange has described under the name of "contracture tabétique des atheromateux." The vessels affected with periarteritis assume a beaded form and become the seat of miliary aneurisms and ampullar dilatations. A condition recalling the "état criblé" of the brain is also seen here. Demange describes a sort of dissecting aneurism in the lymph sheaths, resulting from miliary hemorrhages within the sheaths. Leyden has also described minute areas of softening, arising from small arterial thrombi, the size of a pin-head.

Nerves.

We know but little of the senile changes occurring in the nerves and their terminations, but they are probably analogous to those found in the spinal cord. The spinal alterations appear at a much later period than those in the brain, and when they do occur lead to a progressive decadence in the functions of the central nervous system.

The motor functions suffer first a weakening, there is less strength than there was. The gait is slower and less sure, and the old man tires easily. The tendon reflexes are weaker.[33] In consequence of the general debility there is often observed a slight and temporary trembling of the limbs. This is not to be confounded with the so-called tremor senilis, which is a disease by itself, not confined exclusively to old age, and which would better be called tremor essentialis (Leyden). In true senile trembling there is probably difficulty of conduction of the motor impulses in a spinal cord presenting some of the alterations just described. We speak of "senile paraplegia" when the lower extremities are more particularly the seat of the muscular debility. This condition should not be confounded with incomplete bilateral hemiplegia following symmetrical cerebral lesions.

The general sensibility is obtunded in old age, and various paræsthesias, such as numbness of the extremities and formication, are complained of. The special senses are less acute. The pupil does not respond readily to light,[34] but remains rigid and contracted. The intellectual and moral attributes of the aged participate in the general decadence. The moral perversion sometimes takes a sexual form ("exhibitionists," Lasègue), and senile dementia[35] often closes the scene.

Finally the debility increases more and more, all the organs take part in the decay, and marasmus and at last death bring to a natural termination the movement of animal life which depends upon the combined action of the individual forces residing each in its special organ. This is natural death, death from old age.

CHAPTER II.

We have learned to regard old age as an inevitable, incurable disease, which comes on gradually, progresses slowly, not attacking all the organs at once, but often seizing them one by one, and which at last terminates with the death of a vital organ or with general marasmus. The morbid picture of senility is a many-sided one, and depends not always upon the number of years. Our age is rich in those premature old men, who, weakened by a rapidly consuming life, wander about like animated corpses, and hasten on toward the grave. On the other hand we often gaze with wonder upon the venerable form of one advanced in years who has fulfilled his allotted task with wisdom and strength up to the end of his long life. Seldom, however, does the old man endure until removed by a natural death, for these retrograde tissue changes which we have been considering excite in the senile organism almost as great a predisposition to disease as we find existent in childhood. When we consider that at least ninety per cent of mankind never reaches the age of seventy-five years we can better appreciate how great the mortality really is. But the course of the individual diseases in old age is so different from what we are accustomed to see in younger people, that it seems necessary to point out briefly the numerous modifications which old age impresses upon the ordinary diseases of adult life.

When we begin to study the way in which the aged are affected by different diseases we are met by the fact that they are more prone to suffer from some than from others, and that they seem to be partially or wholly exempt from the attacks of many of the affections of adult life.

The life of the aged can, of course, be destroyed by the same causes which snap the thread of a younger life, yet old people are less susceptible, as a rule, to the action of contagia,

and yield to them only when present in overwhelming quantity. This is to be explained by the fact that, notwithstanding the weakened powers of resistance of the senile organism, the contagion finds here an unsuitable soil for its development. The senile organism, by reason of the less rapid change of matter, is loaded with excrementitious products (ptomaïnes?), which probably are inimical to the development of a large number of disease germs.[36] For this reason we never see a typical inflammatory condition, accompanied by erethitic or dynamic fever, but rather the nervous, asthenic, torpid forms, in which the symptoms recall those of poisoning (by ptomaïnes) of the central nervous system much more than of an infection. But the infrequency of infectious diseases in the aged is also due to the fact that these individuals are less exposed to the external causes which produce lesions of the tissues whereby the infecting agents gain an entrance into the system. Without the presence of oxygen most of the pathogenic microbes (with the exception of what Pasteur has called the anaërobes in contradistinction to the aërobes) cannot develop, and remain innocuous to the organism.

The following are the modifications which the acute infectious diseases always undergo in the aged. We miss from the very beginning any strong reaction of the organism against the invading disease; the former remains strictly passive. The diseases terminate fatally in a short time or they assume a more chronic course, ending by lysis and being frequently characterized by metastases. Perfect recovery is the exception, for the plastic activity is reduced to a minimum, and the elimination of the morbid products by means of the atrophied secretory organs is seldom complete. Among the infectious diseases the exanthemata are never seen in the aged. Typhus and typhoid fever are very rarely encountered, acute articular rheumatism approaches more the nature of gout, and the liability to acute erysipelas is almost wholly lost. The liability to contract tuberculosis is also lost, the tuberculous affections of old people having had their beginning at an earlier period of life. The aged usually enjoy an immunity to epidemic parotitis, and the liability to contract malarial fevers is also much reduced. The syphilitic virus will, however, live and develop in the organism even in extreme old age.

Pneumonia.

One of the most frequent of the acute infectious diseases is pneumonia. Cruveilhier calls it the scourge of old age. The symptoms of the disease in the aged are so different from those in adult life that we may profitably devote a little time to their consideration.

Pneumonia presents in childhood a much more serious aspect than it does in old age, but the aid to recovery which nature gives at the former age is far greater. The reciprocal echoing back of functional or material disturbances from one organ to another does not take place in old age, and for this reason the symptoms and course of pneumonia at this period undergo many and various modifications. The disease is often very deceptive in its symptoms, and is not infrequently latent.

Senile pneumonia is usually a primary affection, and one reason why it occurs so seldom as a secondary condition is that the acute infectious diseases, to which pneumonia is such a common sequel, are of very infrequent occurrence in old age. But since we have discovered a specific cause for pneumonia we must regard most of even these so-called secondary forms as primary, and are brought to a recognition of mixed infections.[37]

We very seldom see croupous pneumonia in the aged, but almost always lobular or broncho-pneumonia and the hypostatic and metastatic forms. Among the predisposing causes we recognize chronic bronchorrhœa, permanent passive congestion of the lungs, and paralytic œdema of the dependent portions of the lungs, which offer an inviting field for the lodgment and development of the specific organisms.

The lobular form of pneumonia is as greatly to be dreaded in the aged as in children, as well on account of its frequency as of its insidious onset. The disease develops in the aged only in portions of the lung affected with hemorrhagic hypostatic infiltration, while in children areas of lobular pneumonia are found in the midst of healthy lung tissue.

We ought not to confound the latent pneumonias of old age with the so-called rudimentary and masked pneumonias,[38] under which are grouped various ephemeral pyrexias which present the characteristic initial symptoms of pneumonia and are accompanied with great prostration, but in which the

most careful examination fails to reveal any pulmonary disease, and in which no further symptoms of pneumonia appear. (These masked forms are also to be distinguished from the so-called abortive pneumonia.) Senile pneumonia often presents almost no symptoms of importance; the patients seem to be perfectly well, perform their ordinary duties, go about, eat and drink with a relish, and complain of nothing, until finally they begin to feel exhausted, and die most unexpectedly. Notwithstanding the insignificant symptoms shown during life, one not infrequently finds at the autopsy a large area of suppuration in the lung tissue. Many a case of sudden death in the aged is due to a latent pneumonia. When certain organic predisposing causes, such as heart disease, are present, the commencement of a pneumonia is almost always latent.

The period of incubation of pneumonia is therefore without any symptoms. The sensibility of the senile organism is so obtunded that it cannot react to the irritation of the ptomaïnes, produced by the pathogenic bacteria, upon the central nervous system, and hence the prodromes which are the outward expression of this reaction are absent. The pain in the side, which is usually a prominent symptom of the prodromal stage in younger persons, and which is caused not by an accompanying pleurisy but by a toxic intercostal neuralgia (from the action of the ptomaïnes), is usually absent in the aged or is referred incorrectly to some other region.

In these latent forms there is usually no chill, the vasomotor centre, which is supposed, during a rigor, to interfere with the giving out of heat, being unaffected. The intensity of the fever depends then only upon the more rapid molecular changes, induced by the presence of the specific bacteria, the soluble products of which (ptomaïnes) are taken up into the circulation and stimulate the heat-centre. When there is an overwhelming invasion of the micro-organisms, these centres are paralyzed and the production of heat is lessened, a very low temperature (temperature of collapse) being then observed. This is a symptom of ominous import, for it is probable that nature has need of a high temperature in order to effect the destruction of the disease germs. The critical stage of sweating is also commonly absent in the aged, and thereby the elimination of the waste products and the reduction of the excessive temperature are retarded.

Most frequently at the height of the disease signs of cerebral irritation appear, and these furnish often the principal symptom of the pneumonia. But in place of the furious delirium of adults, and of the convulsions of children, we see in the aged only a mild delirium, coming on especially toward evening, or somnolence and coma. The cough is so slight that the patients themselves do not notice it, and they seldom complain of dyspnœa.

The pulse, which is often but little accelerated, offers no certain measure of the degree of fever, a better indication of this being the dryness of the throat and the unquenchable thirst.

We see, therefore, that almost all the general and local symptoms of pneumonia are absent in the aged. It is all the more necessary then, whenever a patient of advanced years complains of severe headache, shortness of breath, and oppression in the chest, to count the number of respirations and repeatedly to percuss and auscultate the chest. The respirations are frequent and uneasy, the thoracic movements are more extensive than normal, and the patient usually lies on his back, being unable to turn on his side, speaks with difficulty, and gasps between each word that he utters. Sometimes a pneumonia begins with symptoms of asthma. Occasionally expectoration is absent, and this is a bad sign. The sputa are more or less tinged with blood, but at a later stage become purulent. The breath is sometimes very foul, especially in those addicted to the use of alcohol, but this is not necessarily an indication of gangrene of the lungs.

On auscultation, crepitant râles are heard and point frequently to pulmonary œdema. The râles are bubbling, moist, or dry. At the root of the affected lung is heard constantly a blowing sound, which is caused by bronchial dilatation that may be mistaken for a cavity. The vocal resonance in the neighborhood of the hepatized area approaches more the quality of ægophony than of bronchophony. In the lungs of the aged, however, there is always a strong vocal fremitus, and bronchial breathing is more diffused, especially over the root of the lung in spare individuals.

Percussion does not always give absolute flatness, especially over the anterior and upper portions of the chest, but posteriorly the dulness is much more pronounced. The percussion note in the aged, even when the lungs are relatively

sound, is clearer than in younger persons and often as strong as in a case of well-marked emphysema in an adult. Over the apex the percussion note is duller, owing to the almost constant presence of induration in this neighborhood.

Senile pneumonia usually runs a rapidly fatal course, and when recovery does take place it is very tedious, and is seldom complete, for the so-called areas of engorgement remain behind and keep up a constant irritation in the parenchyma of the lung, which breaks out in a fresh inflammation on the slightest provocation. This offers an explanation of the frequent recurrence of pneumonia in those who have once suffered from an attack in advanced life. Double pneumonia is very frequent in the aged, and serous or purulent exudations in the pleura offer a not uncommon complication. Pulmonary abscess is a more common sequel at this period than it is earlier. Death occurs from paralysis of the lungs or heart, and also not infrequently from suffocation by the filling of the bronchi with mucus or pus. Apoplexy sometimes closes the scene.

Most of the other complications and minor ailments from which the aged suffer are chronic in their nature and stand usually in a causal relation with senile involution, which frequently passes directly into these morbid states, as we have shown above while considering the senile alterations in the individual organs.

The Heart.

Cardiac affections play an important part in the mortality of the aged. As life gradually leaves the organism it flickers longest in those organs in which it first appeared, thus retracing the path which it followed in the development of the organism (Durand-Fardel). The lungs, the heart, and the brain are the parts in which life concentrates itself as it prepares the old man for its departure.

The heart diseases of old age depend for the most part upon organic senile changes, and are often found in combination with gouty affections or with kidney troubles.

Old people suffer frequently from palpitation even before there is any insufficiency or stenosis of the valves. We have already studied the disturbances of function caused by senile alterations in the heart, and would refer the reader to what we have said on this point in the previous chapter.

Acute endocarditis, pericarditis, and myocarditis are among the greatest rarities at this period of life, for the reason that the various affections which stand in a causal relation to them do not occur or occur but seldom. On the other hand, we very commonly meet with the chronic forms of inflammation which owe their origin to atheromatous changes. The occurrence of the so-called chronic aneurism of the heart is sometimes noted, which equally with acute aneurism may terminate in rupture and cause sudden death from hæmopericardium. The most frequent result of the heart affections of the aged is hydropericardium and hydrothorax. The appearance of œdema is often the first thing that calls attention to the heart.

The Abdomen.

As regards the diseases of the abdominal organs we may refer briefly to the extreme rarity of peritonitis and to the frequency of gastritis, which latter very commonly leads to the formation of ulcers and terminates in perforation of the stomach and a fatal discharge of its contents into the peritoneal cavity. Senile gastritis does not long preserve the symptoms of a true inflammation, but soon becomes sluggish in its course. The tongue is dry and black and an adynamic condition is speedily developed.

Dilatation of the stomach occurs more frequently in old people who have always been large eaters and consumers of great quantities of beer and wine. The precursor of this affection is the so-called water brash (pyrosis senilis), which exists usually for a number of years before the actual occurrence of the gastric dilatation.

A disease peculiar to old age is an atrophy of the stomach and intestine to which Schölein has given the name "marasmus senilis." This atrophy is also an accompaniment of premature old age induced by excesses of various kinds and in women by rapid child-bearing, and has been called by Hufeland "marasmus senilis factitius." The disease progresses very slowly and is incurable.

A very frequent complication of the period of involution is ascites, which Schönlein has divided into a venous or periodic and an organic form.

Senile icterus is also a very common affection, and is due

to stagnation in the portal system and to organic changes in the abdominal viscera. The prognosis is bad, and the patients quickly succumb with toxic symptoms.

Of the other abdominal disorders peculiar to old age we may mention cancerous new-growths and concretions in the gall-bladder, urinary bladder, and kidneys. The new-growth is by no means a purely secondary affection in the dilated renal pelves and diverticula of the bladder, but is already in statu nascendi[39] in consequence of the altered circulatory conditions in the parenchyma of the kidneys and the altered constitution of the urine. We need only refer to the frequency of catarrh of the bladder. It is commonly the result of the decomposition of the urine following upon retention. The prognosis is usually grave, for the disease seldom ends in perfect recovery.

Skin Diseases.

Among the cutaneous affections peculiar to old age we have to notice chronic erysipelas, gangrene, and pruritus. Pseudo-erysipelas is one of the most frequent troubles of old age. It affects chiefly the lower extremities, and not uncommonly the skin covering the tibiæ dies and the disease passes over into gangrene. Recurrences of the trouble are extremely common.

Senile gangrene attacks the dependent portions of the body, and is usually accompanied by severe pain. It results from obliteration of the nutrient artery of a limb whenever the collateral circulation is insufficient to supply the part, but it may also follow destruction of the capillary network in consequence of senile alterations in the vessels.

The troublesome itching from which the aged so often suffer, which robs them of their rest and makes their life a burden, is also dependent upon the senile alterations in the tissues. It affects chiefly the covered portions of the body, the back and the extensor surfaces of the extremities, principally the lower. Not infrequently it is localized at the anus, or in women at the vulva.

Brain.

The cerebral affections of the aged assume for the most part the form of congestions, due to passive or venous hyperæmia, which may be either primary or secondary to diseases

of the heart, lungs, or abdominal organs. As a result of these congestions the aged frequently suffer from vertigo, fainting spells, somnolence, or even insomnia.[40] By reason of the existence of atheroma of the cerebral vessels these congestions lead to the production of miliary aneurisms, and the latter to apoplexy. Atheromatous encephalitis or softening of the brain, an affection almost exclusively belonging to old age, arises from thrombosis of the cerebral arteries, and in its turn leads frequently to dementia senilis complicata.[41] While unconsciousness appears suddenly in apoplexy it is gradual in its onset in the case of cerebral softening; the apoplectic coma is also deeper and does not lighten temporarily as does sometimes that of softening. The paralysis of cerebral softening is also distinguished from that of apoplexy by its gradual onset and greater extent. The hemiplegia of cerebral softening is generally accompanied by a paralysis of the intelligence and of the tongue. The paralysis of apoplexy usually remains stationary or even decreases, while that of softening is commonly progressive. Death from cerebral softening may occur through complete paralysis, through serous apoplexy of the brain, or through the production of decubitus. The course of the disease is often rapid, but frequently also prolonged, and the prodromes are commonly of months' or even of years' duration.

Spinal Cord.

Hyperæmia, hemorrhage, and softening of the spinal cord owe their origin to the same influences as the similar affections in the brain. They also cause paralyses, which are to be distinguished from cerebral paralyses by the preservation of the intellectual functions and by the fact that they affect chiefly the lower extremities, the bladder, and the rectum. An increase in the sclerotic process in the spinal cord gives rise to the "contracture tabétique des atheromateux" of Demange. Rosenbach and Berger[42] have also pointed out the relation exbisting between arterio-sclerosis and tabes.

There are two neuroses which are of very frequent occurrence in the aged: asthma and angina pectoris. Neither of these, however, is apt to occur as a primary affection, but they are usually dependent upon organic changes in the thoracic viscera.

CHAPTER III.

As we have seen that the diseases of old age are divisible into the affections directly dependent upon the process of involution and the occasional complications of this process, so we find that the therapeutics of old age may be divided into two parts: the retardation, as far as it is possible, of the steadily progressing tissue changes, and the prevention of the accidental complications which threaten to cut life short. It is from a knowledge of the etiology of the diseases of old age that we learn best the art of prolonging life. The hygiene of the aged is the most efficient macrobiotic.

The foundations of a long life may, however, be laid in childhood and during man's prime. When the powers of the organism are artificially weakened a precocious senility results. We have seen in a previous section that temperament, constitution, diet, occupation, disease, excessive bodily or mental strain, depressing external circumstances, and the like, may sow, in early life, the seeds of a premature old age. General and individual hygiene in early years will therefore be the best prophylaxis of precocious senile marasmus.

It is true that senile involution is in itself an incurable disease, but this character it possesses in common with many other affections, all of which, as well as senility itself, have a right to demand of medical skill postponement and delay.

If "to know how to live" may be accepted as the definition of general hygiene, then all of the hygiene of old age may well be summed up in the term "to know how to grow old" (Durand-Fardel). The serene and cheerful delineations of Cicero present to us the wise old man who has preserved his body and mind by a temperate use of them ("Potest exercitatio et temperantia etiam in senectute conservare aliquid pristini roboris.") The new conditions with which the hygiene of old age has to deal are the insufficient blood-supply, the lessened amount of nourishment, the progressive blocking up of

the capillary system, the increasing obtuseness of the nervous system and of the intellect, the weakened vitality of the skin, and the diminished secretions and excretions. The principal points to be considered in our endeavor to compensate for the deficiencies above described are air, exercise, nourishment, care of the skin, regulation of the secretions, cheerful surroundings, and an equable disposition. The decarbonization of the blood depends much more in the aged than it does in the young upon the constitution of the inspired air, for, as we have seen above, the old man breathes in a confined atmosphere, as it were, in consequence of the narrowing of his respiratory field. A residence in the country, at least during a great part of the year, in a dry and moderately elevated locality where there is a southern exposure with protection against north winds, would be very desirable. The sea-shore, by reason of the strong winds there prevalent, the mountains because of the rarefied atmosphere, and the banks of a river, on account of the strong currents of air, offer conditions which are inimical to the weakened organs of the aged. The changes in residence and in the mode of living should be effected with no undue precipitancy. Old age is the age of moderation. We cannot without danger at this time of life send those who live in the lowlands to elevated regions, or those who dwell in cities to high hills or to the unshaded sea-shore. Although cold is the greatest enemy of the aged, yet we should not always send the inhabitants of cold countries to a warm climate, for such a radical change in residence and in the mode of living is not well borne by all. The old man should, however, avoid breathing the vitiated atmosphere of crowded assemblies, of theatres, and of churches when the services are protracted.

The stiffness of the joints, the atrophy of the muscles, the retardation of the circulation, the blunted senses, the sluggishness of the intellect, and the weakness of the nervous system all invite the old man to repose and to the maintenance of a certain passive condition. Against this tendency we must fight to the end by means of active and passive movements. Driving, riding, swinging, gymnastics, and massage are of advantage. The old man needs the occupation to which he has always been accustomed as long as his bodily and mental powers render him capable of it. The tendency to sleep after a meal should not be indulged when the awakening is accom-

panied by a dull headache and dizziness. Active and passive movements short of fatigue are to be recommended after eating in order to promote the activity of assimilation, to accelerate the sluggish digestion, and thereby to remove the feeling of heaviness and oppression and to facilitate respiration and circulation. Apoplexy not infrequently attacks the aged during the heavy sleep following a full meal.

The diet should be looked after as regards both its quantity and its quality. The enfeebled contractile power of the walls of the stomach and intestines is unable to cope with too great masses of food, and becomes easily paralyzed. The food, therefore, should be small in quantity, but readily digestible and nourishing. The blunted sensibility of the digestive apparatus in the aged demands that the food should be moderately stimulating in its character. When the teeth are defective or absent, the food should be such as does not require much mastication. Beer, wine, tea, and coffee are supposed to be moderately stimulating to the vascular and nervous systems, to promote digestion and to exert a favorable action on the torpor peculiar to old age.

The necessity of a careful attention to the skin is rendered apparent by a consideration of the changes which this membrane undergoes in old age. Baths of tepid water or soap and water, salt baths, dry friction, and rubbing with alcohol are very beneficial.

Just as we must provide for the removal of the toxic products of tissue change through the skin, so must we also stimulate to greater activity the excretory organs whose functions are depressed in old age. Special attention should be paid to the proper performance of their functions by the kidneys and intestinal canal.

Complete inactivity consumes the aged. We must therefore provide for diversions suited to the failing strength. The cheerful companionship of young people is of great service in preserving the health of the aged. But especially favorable to the prolongation of life is a tranquil mind which results from the assurance of security against want, as is shown by the low mortality in institutions for the care of the aged.

The organic and functional changes which the advance of age has impressed upon the organism add a new element in the treatment of disease, and the therapeutic indications de-

pend then less upon the nature of the disease than upon the manner in which the organism reacts to it. The diseases are less typical in their symptoms and course than they are at an earlier period of life.

In the treatment of senile pneumonia we meet with a number of peculiarities which demand the closest attention on the part of the physician. The etiological moments meet, as we have seen, in their action upon the senile organism a condition which favors greatly the production of pneumonia, namely a lowered resisting power. We see also that old people who have once suffered from pneumonia are liable to repeated attacks of the disease, and prophylactic measures [43] must accordingly receive the most careful consideration.

The following are the modifications in treatment demanded at this time of life: Cold baths, by reason of the impossibility of foreseeing their action on the heart and walls of the vessels, are of very doubtful expediency. The medicinal antipyretics must also be employed with the greatest caution and only in combination with stimulants. Venesection is inadmissible, since senile pneumonia is essentially adynamic in its nature. But local blood-letting, on the other hand, is one of the best means which we have to combat passive hyperæmia. Other revulsives must be employed with caution, although we cannot dispense with them entirely, since they are often of the greatest service. The ease with which the skin becomes gangrenous in old age and the difficulty with which cicatrization takes place in wounds at this period of life show the necessity of avoiding actual vesication. When blisters are used, they should never be retained long in one spot, but should be changed about from place to place, and unfavorable situations where the circulation is naturally defective should be avoided. Cantharides ought never to be used, because of its action upon the urinary tract, and mustard should be substituted for it. The probably directly derivative action of warm baths [44] and of wet packs applied to the whole body offer a contra-indication to their use in the presence of a weakened heart and of defective nutrition. On the other hand, sponging of the entire body, followed by rubbing and wrapping the body in woollen cloths, exerts a derivative and at the same time stimulating action. A free action of the bowels is exceedingly important, and emetics are also indicated as soon as the bronchi become

filled with mucus. The use of calomel in large doses is inadvisable, as it exerts too depressing an effect upon the already weakened organism. Tartar emetic and apomorphine are also contra-indicated because of the dangerous collapse which often follows their administration. For the same reason the injection of pilocarpine, recently recommended in the treatment of pneumonia, is not advisable. Digitalis, combined with alcohol, caffeine, and other stimulants, promotes diuresis and regulates and strengthens the heart's action. Narcotics should be avoided as far as possible and given only in small doses, for they increase the depression, interfere with expectoration, and favor pulmonary congestion.

The physician should, on the one hand, not treat pneumonia in the aged too energetically, nor, on the other, should he give up the treatment too soon, for there is always a tendency for areas of engorgement to be left behind in the lung. In the first stage of the disease our efforts must be directed to a storing up of the strength in anticipation of the demands of the second stage, and later we must not be sparing in our use of stimulants.

For the heart troubles of old age, in addition to digitalis, we may advantageously make use of the new remedies, tincture of strophanthus (15 drops three times a day), sulphate of sparteine (gr. iss. from one to four times a day), and sodio-salicylate of caffeine (gr. v.–viij. from four to six times a day). Bamberger[45] has lately called attention to the analogy between the favorable action of ether injections and that of calomel in heart failure, and thinks that it is more probably exerted through a direct influence upon the renal epithelium than upon the heart itself. De Renzi[46] believes that pyridine, in doses of from six to ten drops in water, exerts a stimulant action upon the heart. Very important in all forms of cardiac trouble is moderation in the amount of fluids ingested. Oertel's method should be employed with the greatest caution.

The management of senile gastritis, atrophy of the gastric mucous membrane, and carcinoma, affections of the stomach in which the chemical reactions are abolished, demands a cautiously directed meat diet and the avoidance of vegetables rich in cellulose and of fats. The leguminosæ and peptonized preparations are the best food for these patients. In the so-called pyrosis senilis lavage with a one to two per cent saline solu-

tion, or, when fermentation is present, with a two or three per cent boracic-acid solution, is useful.

Organic ascites is to be treated by diuretics and by repeated paracentesis for the evacuation of the fluid.

In senile icterus the mercurial preparations should be employed with great caution, and podophyllin ($\frac{1}{10}$ to 1 grain in pill or alcoholic solution) is to be preferred. In addition a course of the waters at Kissingen or Karlsbad is useful.

In the treatment of chronic vesical catarrh, which is usually a sequel of prostatic hypertrophy and atony of the bladder, we may, in order to facilitate catheterization, seek to reduce the size of the prostate by means of electrolysis applied through the rectum.[47] Kasper remarked a notable improvement in the degree of retention, and a corresponding decrease in the frequency of micturition, after a reduction in the size of the prostate had been obtained. When the prostate is enlarged, the fundus of the bladder is depressed below the orifice of the urethra, and a sort of cavity is formed (W. Busch) which increases markedly the difficulty of emptying the bladder. In consequence of the altered position of the vesical end of the urethra there is sometimes complete retention, which, when catheterization is impossible, may necessitate an external urethrotomy (a far preferable operation to cystotomy). When, however, the muscular tone of the bladder is depressed, a mere reduction in the size of the prostate may afford no relief to the retention. Senile cystitis is in most cases so persistent that washing out of the bladder must often be continued for a considerable length of time in order to prevent pyæmia and the formation of perivesical abscesses.

In chronic senile erysipelas and gangrene the affected limb should be moderately elevated, all pressure by which the circulation may be impeded should be avoided, and moist or dry heat should be applied. The modern antiseptic treatment is not always efficacious in preventing gangrene, since it does not relieve the obliteration of the nutrient vessels. Extreme pain and the occurrence of septic fever call for the removal of the affected part by surgical means.

The following applications may prove of service in senile pruritus: Ol. fagi, ol. rusci, and ol. olivæ, equal parts; chrysarobin-lanolin (10 to 20 per cent); carbolized magnesia (2 per cent); corrosive sublimate, 1 to 2 per cent, carbolic acid, 2 to 4

per cent, in benzoated oxide of zinc ointment; ichthyol-lanolin (10 per cent); naphthol ½ to 1 part, lard 1 part, and lanolin 8 parts; cocaine oleate ½ to 1 part, olive oil 2 parts, and lanolin 18 parts. In pruritus ani suppositories containing ¾ grain of cocaine oleate are useful.

In the treatment of cerebral congestion and hemorrhage in the aged, blood-letting (at other times a useful measure) is not admissible, because of the tendency to stasis of the intracranial circulation which it produces. The best means of dealing with congestion of the brain is by direct or indirect derivation, in the application of which the precautions referred to under the section on pneumonia should not be forgotten. The application every evening of a mustard plaster, which, according to Schüller,[48] causes a contraction of the vessels of the pia, is very beneficial.

The insomnia calls for regularity in the mode of living, and, in cases of necessity, the administration of narcotic sedatives and hypnotics. It should not be forgotten, however, that chloral hydrate is contra-indicated in cases of atheromatous degeneration of the arteries and in fatty heart. Opium and morphine are also to be used with caution, for they only increase the hyperæmia of the brain, and are not without danger in weakness and organic degeneration of the heart. On the other hand, urethane (in doses of 15 to 30 grains dissolved in water every half-hour until sleep is obtained) seems to be as certain as it is a safe and pleasant sedative and hypnotic. Amylene hydrate (one drachm in gelatin capsules), paraldehyde (45 to 90 grains dissolved in water), and sulphonal (10 to 20 grains) enjoy a good reputation in the treatment of senile insomnia.[49]

The paralyses following apoplexy and cerebral softening are to be treated on general principles, the etiological factors being borne in mind.

The senile affections of the spinal cord are to be treated on the same lines as the corresponding diseases of the brain. For the relief of the paresis of the sphincter vesicæ we may try subcutaneous injections of ergotin and strychnine, which oppose directly the vasoparesis and also are of great service in promoting the nutrition of the brain and spinal cord.

The therapy of senile asthma and of angina pectoris must be based upon the nature of the organic changes upon which

these neuroses depend. Nitrite of amyl (Schüller, loc. cit.) is therefore contra-indicated because of its paralyzing action upon the cerebral vessels and of its exciting effects upon the heart. For the same reasons nitro-glycerin and nitrite of sodium should be employed with caution. On the other hand, inhalations of pyridine (a drachm may be poured out on a plate) and injections or inhalations of sulphuric ether are indicated. The newly discovered sedatives above referred to may also find an application in these conditions. Iodide of potassium accelerates the atrophic process and is therefore to be avoided. The direct and indirect derivatives and revulsives (sinapisms) are of service here. When the pulse is weak and rapid, Schott's[50] hot rubber bags, at a temperature of 140 to 170° F., and even higher, are strongly to be recommended.

BIBLIOGRAPHY.

1. Stricker: Vorlesungen über allg. u. exper. Pathologie. Wien, 1887.—2. Canstatt: Die Krankheiten des höh. Alters u. ihre Heilung. Erlangen, 1839.—3. Mettenheimer: Beiträge zu der Lehre der Greisenkrankheiten. Leipzig, 1863. Müller: Leben und Tod. Betrachtungen über das Greisenalter. München, 1876.—4. Gendrin: Influence des âges sur les maladies. Paris, 1840.—5. Bichat: Recherches sur la vie et la mort.—6. Traité pratique des maladies des vieillards. Paris, 1873.—7. Étude clinique et anatomo-pathologique sur la vieillesse. Paris, 1886.—8. H. Martin: Pathogénie des lésions athéromateuses des artères. Recherches sur la nature et la pathogénie des lésions viscérales consécutives à l'endartérite oblitérante progressive. Rev. de méd., 1881.—9. Paget: Fatty Degeneration. London Medical Gazette, 1850.—10. Leçons sur les maladies des vieillards et les maladies chroniques. Paris, 1868.—11. Anatomiæ corporis humani senilis specimen. Erlangen, 1800. (Reil's Arch., vii., pp. 1–119).—12. Virchow: Spezielle Pathologie.—13. Duplaix: Contribution à l'étude de la sclérose, Paris, 1883. Juhel Ronoy: Étude sur la sclérose du myocarde et son importance dans la pathogénie des accidents asystoliques. Paris, 1882–83. Letulle: Recherches sur les hypertrophies cardiaques secondaires. Paris, 1879.—14. Peter: Traité clinique et pratique des maladies du cœur. Paris, 1883.—15. Bertin: Mort (Article du Dict. de Dechambre).—16. Haushalter: Recherches sur le cœur sénile (Th. doct., Nancy, 1886).—17. Villemin: Arch. de méd., 1886. Cornil et Ranvier: Anatomie pathologique.—18. Hourmann et Dechambre: Recherches cliniques pouvant servir à l'histoire des maladies des vieillards (Arch. gén. de méd., 1835, t. viii.).—19. Natalis, Guillot: Recherches anatomiques sur la membrane muqueuse du canal digestif (Journal de l'Expérience, 1837). Hutin: Recherche d'anatomie physiologique et pathologique de la membrane muqueuse gastro-intestinale, 1876.—20. Wieger: Ueber hyaline Entartungen in den

Lymphdrüsen. Archiv für path. Anat. u. Phys., t. lxxiii., p. 25.—21. Sadler: Contribution à l'étude du rein sénile (Th. doct., Nancy, 1879). Demange: Le rein sénile, Paris, 1880. Ballot: Contribution à l'étude du rein sénile, Rev. de médecine, 1881.—22. Launois: De l'appareil urinaire des vieillards (Th. doct., Paris, 1885).—23. Arthaud: Étude sur la testicule sénile (Th. doct., Paris, 1885).—24. Meyer: Von den Veränderungen, welche die weiblichen Genitalien in hohem Alter erleiden. Bonn, 1825.—25. Patenostre (Th. doct. de Paris, 1877).—26. Barthélemy: Des verrues séborrhéiques ou verrues plates de la vieillesse (Arch. de Dermatologie, 1881). —27. Canton: Arcus Senilis, or fatty degeneration of the cornea, London. —28. Ribbert: Ueber senile Osteomalacie und Knochenresorption im Allgemeinen. Arch. f. path. Anat. u. Phys., 1, lxxx., p. 436.—29. Schoemann: Malum coxæ senile. Jena, 1851.—30. Schüller: Die Pathologie und Therapie der Gelenkentzündungen. Wien und Leipzig, 1887. Weichselbaum: Die senilen Veränderungen der Gelenke und deren Zusammenhang mit Arthritis deformans, 1877.—31. Geist: Klinik der Greisenkrankheiten. Erlangen, 1860.—32. Durand-Fardel: Traité du ramollissement du cerveau, ouvrage couronné par l'Académie royal de Médecine. Paris, 1848. Virchow: Spezielle Pathologie und gesammelte Abhandlungen.—33. Möbius: Ueber das Schwinden des Kniephänomens bei Greisen. (Centralblatt für Nervenheilkunde, 1883.)—34. Möbius: Ueber das Verhalten der Pupille bei Greisen (Centralblatt für Nervenheilkunde, 1883).—35. Ball et Chambard: Démence sénile (Dict. encycl. des sc. méd.). —36. Garré: Immunität durch lösliche Substanzen. Centralblatt für Schweizer Aerzte, Juli, 1888. Gottbrecht: Zur antiseptischen Eigenschaft des Ammoniaks. Deutsche medicinische Wochenschrift, No. 29. —37. Brieger: Beitrag zur Lehre von der Mischinfektion. Zeitschrift für klinische Medicin, Bd. xi., 1886.—38. Kühn: Rudimentäre und larvierte Pneumonien nebst ätiologische Bemerkungen über Pneumonieinfektionen. Deutsch. Arch. für klinische Medicin, Dec., 1887.—39. Güterbock: Ueber die Störungen der Harnentleerung bei Prostatahypertrophie (Berliner Klinik, Heft 4, 1888).—40. Poppelauer: Der Schlaf bei Greisen. Berliner klinische Wochenschrift, July 10th, 1875.—41. Wille: Ueber die Psychosen des Greisenalters. Allgem. Zeitschr. f. Psych., Bd. xxx. Voisin: Paralysie général sénile. Marcé: Recherches cliniques et anatomo-pathologiques de la démence sénile, 1883.—42. Berliner klinische Wochenschrift, 1879. Leyden: Ueber Herzkrankheiten bei Tabes. Deutsche med. Wochenschrift, No. 45, 1888.—43. A. Seidel: Aetiologie und Therapie der akuten Lungenentzündung. Deutsche Medicinal-Zeitung, 1888.—44. Schüller: Arch. f. klin. Med., Band xiv.—45. Wiener klinische Wochenschrift, No. 12.—46. Rivista clinica e terapeutica, 1887.—47. Kasper: Zur Therapie der Prostatahypertrophie und Prostatatumoren. Vortr., geh. i. d. Berl. med. Ges. am 18. April. 1888.—48. Berliner klinische Wochenschrift, Nos. 25 and 26, 1874.—49. Schmey. Neuere Schlafmittel und Sedativa. Der ärztliche Praktiker, 1888 und 1889.—50. Die Behandlung der chronischen Herzkrankheiten. Vort. geh. in der balneol. Sektion d. Ges. f. Heilkunde in Berlin, 1887. Deutsche Medicinal-Zeitung, April 7th, 1887.

Urinary Neuroses of Childhood.

BY

Dr. LOUIS-J. GUINON, Paris.

URINARY NEUROSES OF CHILDHOOD.

CHAPTER I.

THE origin of many diseases appears surrounded with obscurity if no account is taken of the hereditary antecedents of the patient in the study of their pathogeny. In infantile pathology the place which these antecedents merit in etiology has not always been given to them. Clinicians, deprived of the guiding line, the notion of heredity, search around the patient, that is to say, in the place where the real cause of the disease is most often not to be found. Now it is a cold, a fall, dampness, a fright; or it is the growth, approach of puberty, dentition. In this way many authors explain infantile paralysis and spasmodic hemiplegia, endocarditis, chorea, convulsions, bronchitis, while in fact the real origin of the disease is explained by nervous heredity for some, arthritic heredity for others (chorea, recurrent bronchitis); a very modest rôle, which may often be neglected, being left for the other causes.

General pathology has modified its method. In studying the relations of diseases, their transformation in the descendants, the metamorphosis of the hereditary taint according to age and sex, it has fixed the value of what I shall call the "diathetic morbid equivalent" of each age, and explains how "each individual has a very different mode of reaction to the same morbid cause" (Dejerine). It is thus that the law of heredity, derived from the general laws of Darwin, is extended gradually to the different chapters of nosology. In the field of mental diseases the alienists, following Morel's lead, have formed a natural and clear class of diseases which appear dis-

tinct and separate—the class of degeneracy. In the neuropathies, Charcot has shown, and shows daily, the close relationship between all nervous diseases, from the organic and systemic lesion like tabes dorsalis to the changing and diffuse neuroses. In the class of diatheses Bazin and Bouchard have studied the multiple transformations, the superficial or profound manifestations of arthritisme.

This is, in fine, an indispensable basis to the clinician for the formation of a valuable diagnosis. If it is true that the disease is sometimes created entirely by outside influences, and if, on the other hand, it may be doubted whether the diathesis favors the contagion of acute infectious diseases, it is equally certain that the disease is modified in its evolution and sequelæ by the hereditary condition, and that the mode of reaction of each individual varies with it. The tuberculous do not react like the arthritic, the neuropathic like the scrofulous. The same exposure will produce, in each of them, a pulmonary congestion, an attack of acute articular rheumatism, a facial paralysis, or a glandular swelling.

In the adult we may sometimes neglect, without too much indiscretion, the search for heredity, because the individual bears externally, to some extent, the marks of his diathesis. In infantile pathology, however, this is indispensable. Apart from tuberculosis, we are hardly acquainted with the external diathetic characteristics in very young children, for example in the child of an arthritic. The humoral diathesis does not appear to exist in the child, and seems to belong only to the more advanced periods of the development of the tissues. Arthritism and herpetism are peculiar to adults. Bouchard[1] insists upon the fact that the constitution and temperament change, that "furnished at birth, they are modified by age, transformed by the disease and by hygiene."

But although the external conditions may create or modify the diathesis, they cannot extinguish it when it is hereditary. This is especially true of an heredity whose weight bears upon all ages from the moment of birth, even from the moment of conception; we refer to nervous heredity. The member of the neuropathic family (Féré[2]) must choose at different ages between the most varied nervous manifestations. It may be said that every neuropathy of adults has its principle, its diminutive, its morbid equivalent in childhood. Each age has

its own stamp. Slight or grave manifestations, the exaggeration or suppression of a psychical function are signs that may be neglected by one who does not investigate, but are of prime importance to one who attempts to find the primary cause, and, enlightened by the notion of heredity, to reconstruct the chain which unites the ancestor with the progeny.

Thus Moreau recognized that "insanity is related to convulsions, hysteria, idiocy, epilepsy, strabismus, paralysis, neuralgia, cerebral fevers, apoplexy, eccentricity, tic, stuttering, asthma, deafness."[3]

Charcot, Landouzy, and Ballet regard nervous heredity as the primary cause of ataxia, the other causes (syphilis, venereal excesses, traumatism) being only exciting. Neumann[4] has made the same statement with regard to facial hemiplegia.

Heredity also explains a number of nervous manifestations in infantile pathology. Convulsions do not appear from a slight cause (gastric disturbance, dentition, helminthiasis) in children who are free from every nervous taint. At least this is what we have learned from observation, agreeing in this respect with Ballet, who showed that certain epileptoid convulsions occurring in a man as the result of worms in the intestines are merely the first manifestation of epilepsy.[5]

It is with the same justification that some of the urinary disorders of childhood, which will occupy our attention, belong to the family of neuropathies. We have studied them on the basis of the preceding facts and will present them in a corresponding manner.

Classification.

The disorders of the urinary function in childhood may affect secretion and excretion. Their complete enumeration would be a long one, because there is no chemical change in the urine in adults, no modification in its excretion, whose analogue is not found in childhood. We will discuss only those which appear independent of every local lesion, of every change in general nutrition, and which, for the most part, originate in a dynamic disturbance of the nervous system. They are generally changeable, temporary, disappear as they have come, suddenly, under the influence of a slight cause. They sometimes persist for years, without injury to the gen-

eral health, but constitute an infirmity whose importance increases with age. They may also undergo transformation and give place to a more serious modification of the nervous system.

We will discuss in succession:

Spasms of the bladder and the neck of the bladder.

Pollakiuria.

Incontinence of urine.

Nervous polyuria.[6]

Some Points in the Urinary Physiology of Infancy.—The difficulties of examination in the infant, and especially in the nursling, explain the fact that this question is not yet completely settled and that the few exact data at our command are still very recent. They refer to the quantity of urine, the frequency of micturition, the physical and chemical characters of the urine. [The importance attaching to the complete collection of the urine for twenty-four hours has led investigators to employ various devices. Martin and Ruge, Cruse applied to the genitalia of the new-born a sac of gold-beater's skin or rubber and emptied the fluid from time to time. Camerer employed two methods. In the first the child was kept upon a rubber cloth under constant observation, and immediate measurement taken of the urine collected on the cloth and body with filtering paper. The other consists in enveloping the infant in layers of flannel surrounded by rubber cloth below the navel, and weighing before and after micturition. The source of error arising from evaporation of the fluid may be neglected.]

The majority of investigations on this subject have been made in Germany and Russia. Those of Picard,[7] Bouchard,[8] Parrot and Robin[9] in France have been made chiefly in the line of its normal composition.

After the researches of Virchow, Hodam and Dohrn, it is to Martin and Ruge, Cruse,[10] Camerer,[11] and to Mme. Schabanowa[12] that we owe the most valuable results. We must exclude the insufficient estimates of some authors who are disagreed on the subject of the amount of milk absorbed by the new-born. All these investigations are analyzed in Vierordt's excellent article on the physiology of childhood (Gerhardt's "Handbuch," vol. i., pt. 1).

Quantity of Urine Excreted by the New-born and Nurs-

ling.—At birth the bladder always contains, on the average, ℥ ij. of urine (Dohrn). With regard to the following days the statements of authors vary greatly, depending on the different sources of nourishment of the child. When nursed by the mother, the urine is scanty and light; when nursed by a wet-nurse whose secretion is older and richer, the urine becomes more copious and heavier.

The following are Cruse's figures, the averages of several observations upon healthy children of medium weight (more than 6½ pounds).

Age.	Amount 24 hours.	To 2 Pounds of the Weight of the Body.
2 days	℥ 4¼	℥ 1¼ nurse's milk.
3 "	7	2 " "
4 "	7⅛	2 " "
5 "	7½	2⅛ " "
5–10 "	10	3 " "
10–30 "	12	3⅛ " "
30–60 "	13½	3⅛ " "

Camerer's figures, which differ for the first five days, are very similar for a later period.

Age.	Amount 24 hours.	To 2 Pounds of the Weight of the Body.
1st day	℥ 1½	℥ ½ mother's milk.
2d "	1¾	½ " "
3d "	5½	1¾ " "
4th- 6th "	6½	
9th–12th "	11½	
18th–21st "	12½	
31st–33d "	13	
67th–69th "	14½	3¼ mother's milk
12th–15th week	16½	3 " "
20th–25th "	15	2½ cow's milk.
35th–40th "	26½	4 " "
50th–52d "	31	3¾ " "

Relative to 2 pounds of the weight of the child's body, the amounts increase from ℥ ½ (1st day) to ℥ 3½ (2d week), to ℥ 3 (13th week), to ℥ 3¾ (1 year).

It follows from these two tables that the 24 hours' quantity

increases rapidly from the 2d to the 5th and 10th days, then more slowly.

The authors furnish the relation between the weight of the ingested milk and that of the urine. According to Bouchaud, ℥ 20½ urine correspond to ℥ 32 milk in the nursling; ℥ 19½ in the adult. According to Camerer, ℥ 24½ urine correspond to ℥ 32 water during the nursing period.

Amount of Urine in Older Children.—This has been estimated by Mme. Schabanowa upon 16 children from the age of 2 to 13 years in a series of examinations made with remarkable care and precision.

The following figures are furnished by children convalescing from mild diseases or suffering from slight affections of the skin.

Age.	Urine for 24 hours.	To 2 Pounds of the Weight of the Body.	To 2 Pounds of Water Ingested.
2 years	℥ 21¾	℥ 2⅕	℥ 22¼
2½ "	17	1½	16⅘
3 "	20	1 8/10	19
4 "	40	3 3/10	28½
5 "	30	2	26½
6 "	41½	2⅔	30
7 "	30	1⅚	24⅔
8 "	26½	1¼	24
8½ "	37	2	29
9 "	38¾	1⅓	31
10 "	60	2	31¼
11 "	38¾	1½	34¼
12 "	38½	1½	36
13 "	32½	1⅙	29½

In the adult the proportion of urine to the weight of the body is ℥ ⅘ to 2 pounds; in childhood the predominance of fluid in the food explains the difference, and it is seen clearly that the proportion diminishes progressively with age.

Age.	Quantity.	Proportion.
2– 4 years	℥ 22	69
5– 9 "	34	60
10–31 "	46	52

Frequency of Micturition.

New-born.—According to Martin and Ruge, one-eighth of the infants pass urine immediately after birth; two-thirds during the first day; the first micturition may be delayed to the second day or beginning of the third day. From the second to tenth days the bladder is emptied three or four times at the most. This statement cannot be accepted; the child urinates several times every day. According to Cruse there are six to ten micturitions from the second to fifth days, fifteen from the thirtieth to sixtieth days.

The Nursling.—Micturition is much more frequent than in older children. The mean cannot be fixed and the variations are considerable. The child of five months watched by Camerer for two successive days for eleven hours a day, had sixteen micturitions the first day and thirteen the second day.

Childhood.—Micturition becomes less frequent, but up to the age of ten years it is more frequent than in the adult. After this period it seems to present the same frequency.

	Average Number of Micturitions During the Day (Camerer).
Girl of 2 years,	6.9
" 3¼ "	4.0
Boy of 5¼ "	5.0
" 9 "	4.4
" 11 "	4.0

The number of micturitions at night is greatly diminished.

Voluntary Micturition.—After the second year or even before this time the child usually begins to announce its desire to urinate during the day. At night it continues to micturate during sleep, then micturition becomes less frequent, and sleep may be prolonged eight hours without interruption and without emission of urine. After this (2 to 2½ years) the child adopts permanent habits.

Quantity of Urine Passed at Each Micturition.—In earliest infancy the amount passed at each micturition varies considerably.

2d - 5th day,	3 1½
5th–10th "	1½
10th–30th "	1¾
30th–60th "	2¾

(Cruse.)

According to Camerer:

Children of 5 months, . .	3 $8\frac{1}{2}$–9
Girl of 2 years, . . .	℥ 2
" $3\frac{1}{4}$ " . . .	$2\frac{1}{10}$
Boy of $5\frac{1}{4}$ " . . .	$3\frac{2}{10}$
Girl of 9 " . . .	$5\frac{8}{10}$
" 11 " . . .	5

Physical and Chemical Properties of the Urine.

I will dwell very little on the characters of the normal urine; the majority of the data are taken from Vierordt's work.

Without dwelling on the changes of color during the first few days, which become less marked when the absorption of milk is more abundant, we must refer to the frequent deposits which form upon the linen, and appear as reddish-brown spots during the first two or three days. The fluid is clouded slightly by salts mixed with uric acid, epithelium, and mucus. Parrot and Robin regard precipitates of uric acid, urates, and oxalate of lime as very rare.

Specific Gravity.—At birth, before any food is taken, it varies from 1,001–1,006 (Dohrn). The specific gravity at the first micturition is 1,012 (Martin and Ruge), then we find:

1st– 3d days, ...	1009.7
4th– 7th "	1004.7
8th–10th "	1003.3

(Martin and Ruge.)

After nursing, the specific gravity necessarily varies considerably with the nourishment. In children who presented normal development, Mme. Schabanowa obtained the following figures:

Age.	Specific Gravity.	Corresponding to
2 years	1,012	℥ $21\frac{1}{2}$
$2\frac{1}{2}$ "	1,013	17
3 "	1,011	20
4 "	1,010	$39\frac{1}{2}$
5 "	1,012	$30\frac{1}{2}$
6 "	1,012	$41\frac{1}{2}$
7 "	1,014	$30\frac{1}{2}$
8 "	1,016	$26\frac{1}{2}$
$8\frac{1}{2}$ "	1,013	37

Age.	Specific Gravity.	Corresponding to
9 years	1,013	39
10 "	1,010	60
11 "	1,013	39
12 "	1,014	39
13 "	1,014	32½

The specific gravity thus increases very promptly with age, but remains less than that of the urine of adults. Camerer has compared the specific gravity of the day urine with that of the night.

Age.	Day Urine.	Night Urine.
Girl of 2 years	1,017	1,016
" 3½ "	1,013	1,017
Boy of 5¼ "	1,018	1,020
Girl of 9 "	1,015	1,018
" 11 "	1,017	1,019

Urea (absolute amount). — The urine of the new-born before nourishment is given, contains of urea 0.485 milligm. per 100, *i.e.*, 0.036 in absolute amount; in the first few days, 0.80 centigm. in the 24 hours; a little more than 1.0 gr. after the 3d week; then data are lacking until the second year. Schabanowa's article contains the most accurate estimates.

Observations in children undergoing a normal increase of weight.

Age.	Water Drank or Ingested with Food.	Mean Weight of Body.	Average Urea.	Urea Compared to 2 lbs. of Weight.
2 years	℥ 30	lbs. 21½	gr. 148	gr. 15
2½ "	32½	" 24½	" 155	" 14
3 "	33½	" 23⅘	" 200	" 18½
4 "	41½	" 24½	" 224	" 20½
5 "	37	" 33	" 217	" 14
6 "	45	" 34	" 221	" 14
7 "	39	" 41½	" 230	" 12
8 "	34	" 45	" 267	" 13
8½ "	41	" 41	" 274	" 15
9 "	49	" 49½	" 292	" 13
10 "	61½	" 61½	" 306	" 10½
11 "	36½	" 57	" 287	" 10½
11 "	41	" 58½	" 204	" 11
12 "	34½	" 60½	" 333	" 12
13 "	38	" 60¾	" 300	" 10½

The author remarks that the quantity of water ingested does not influence the quantity of urea—a fact that is contrary to the theory according to which polyuria secondarily causes increased excretion of urea. The same figures show that the absolute amount of urea increases with age, but that the relative proportion to the weight of the body, after having increased to the age of four years, then diminishes constantly. This is also expressed by Vierordt when he says that there are two minima corresponding to the two extremes of childhood and that their relation to the maximum (four years) is as 1:5:2.

It is important to note the differences between the nocturnal and diurnal excretion of urine. It has been said that certain cases of incontinence were caused by the greater concentration of the urine at night, which induced irritation of the bladder. It is seen from the following table that this phenomenon is normal (Camerer).

Age.	Day.		Night.	
	Total Urea.	Percentage of Urea.	Percentage of Urea.	Total Urea.
Girl of 3¼ years	gr. 75½	1.76	2.13	gr. 79½
Boy of 5¼ "	" 111	1.56	1.93	" 87
Girl of 9 "	" 136½	1.15	1.85	" 85½
" 11 "	" 148½	1.61	1.98	" 105

Uric Acid.—During the first few days after birth, as is well known, there is an abundant elimination of uric acid and urate of ammonia, coming from the infarctions which fill the excreting tubes. The proportion of uric acid is much greater in the nursling and child than in the adult (Vierordt). During the first period of life the proportion of uric acid to urea is much greater than at a later period. This is explained by the slowness of oxidation in the new-born, whose nutritive activity is only slightly developed.

According to Ranke, the amount of uric acid in 24 hours in a girl of 3 years is 6½ gr., or gr. $\frac{4}{10}$ to 2 pounds of the weight of the body.

Only traces of extractive matter are found in the urine of the nursling. The albumin which is present in one-third of the cases during the first week (Martin and Ruge) is absent after the 10th day (Cruse).

Chloride of Sodium does not present constant proportions in the nursling; it increases until the second week, and then diminishes. Then the relative proportions approximate those in the adult.

3–5 years,	.	gr. 118 daily	(Rumnel-Ulhe).
6 "	. .	99 "	(Mosler).
11 "	. .	159 "	(Mosler).

Spasm of the Bladder (Bokai.)[13]

1. *In the New-born.*—We know that, in the adult, the emotions and cold act upon the vesical functions; on the other hand, we also know the tolerance of the bladder for foreign bodies when the mucous membrane is healthy. The composition of the urine appears to have little influence on the frequency of micturition, the latter resulting particularly from the sensibility to distention. In the child, and especially in the new-born, the enormous surface of the body relatively to the weight increases very greatly the effects of external temperature upon the vesical irritability. As soon as it arrives in the bladder, the urine newly secreted by the new-born, being charged with urates and uric acid, produces sometimes a peculiar irritation, because it bears chiefly on the neck, "independently of any vesical lesion, of any modification of the capacity of the bladder; in fact the secreted urine may be insufficient to keep the salts in solution; the excessive precipitate produces reflex contraction of the neck, until the solution is complete" (Bokai).

Bokai's *cystospasm or spasm of the bladder* is a spasmodic contraction, attributed to the bladder, but really affecting both the body and the neck of the organ. It is a colic with tenesmus in which the latter predominates, inasmuch as it prevents micturition.

In these cases first micturition is delayed a long time (two or three days), the infant manifests the pain which it experiences by repeated cries, constant agitation, sudden flexions of the thighs on the belly; then follows a distressing, painful, abundant micturition, during which all the symptoms increase and then cease. "There is not retention of urine, but merely difficulty of evacuation."

The urine voided is highly colored, and leaves upon the

linen and prepuce a yellow powder of uric acid, the corpus delictum. These attacks return with diminishing intensity for two or three days.

The diagnosis of this vesical colic results from the negative findings on examination of the orifices of the urethra and rectum, which should always be made after birth. The ingestion of warm sugar water, an enema, and warm baths rapidly relieve the affection (Bokai).

2. *In Infants under Two Years.*—The pathogeny is analogous; the irritation results from the abundance of soluble products, the urates, due to the concentration of the urine by acute respiratory affections and fevers (Bokai). This author admits that the affection is produced at the beginning of rickets, when abundant, clear urine is eliminated.

The symptoms are more distinct than in the new-born, and we notice particularly the pain at the moment of micturition.

The same treatment is applicable as in the new-born.

3. *In Older Children. Pollakiuria.*—Vesical spasm has also been described in childhood. It is due to chilling after a cold bath, the ingestion of certain fruits, drastic purgatives, and all forms of genital and perigenital inflammations.

We have only observed non-painful pollakiuria in nervous children, generally associated with slight weakness, in two cases with a catarrhal condition of the digestive tract and dilatation of the stomach. Sevestre has observed it in younger children at the period of dentition. There may even be three micturitions within an hour after eating; the symptoms may last months.

The diagnosis of these disorders is aided by treatment (sedatives, baths, poultices), to which they yield readily. If the affection does not improve at once, methodical examination of the urethra and bladder should be made.

Phimosis sometimes produces pollakiuria with strangury. Stone in the bladder and foreign bodies give rise to similar but more persistent symptoms; rectal prolapse then occurs (Henoch), and the presence of mucus and pus in the urine shows the existence of a lesion.

In addition to the plan of treatment already indicated, we must relieve the digestive disorders (constipation or diarrhœa) and alkalinize the urine when the urates are abundant. Bokai advises catheterism when the pain becomes too severe.

Retention of urine presents nothing of interest in the child, but we must note the retention by exaggerated distention in girls who have not obeyed the reiterated desire to urinate (school discipline).

CHAPTER II.

NOCTURNAL INCONTINENCE OF URINE.

NOTHING is more common than this disturbance of micturition, in which a child, apparently normal during the day, for years passes urine involuntarily at night. It has nothing in common with the incontinences observed at all ages in the course of acute adynamic diseases; with those which result from lesions of the cord (tubercular pachymeningitis, transverse myelitis), producing paralysis of the bladder and incontinence by overflow; nor with the incontinence of the idiot who passes urine and fæces involuntarily by day and night. We have to deal, as we shall see, with an anomaly independent of every spinal or urinary lesion, and the word incontinence is as inexact as the notion which is generally connected with it. There is not a constant flow, but an intermittent emission, a real but involuntary micturition. The term nocturnal enuresis, employed in England, is preferable, still better is the term involuntary nocturnal micturition.

SYMPTOMS.

The symptomatology is very simple. The incontinence is sometimes a mere sequel of the normal condition of early infancy. At first it is supposed that the child is simply slow in development, but it is soon found that the function of micturition is permanently disturbed. On the other hand, the first years often present nothing abnormal, but toward the age of four or five years or even later—after an acute disease, an injury or fright, but usually without any apparent cause—the incontinence develops with as much persistency and tenacity as in the preceding variety. Thenceforward the child wets the bed, sometimes in a regular manner, sometimes with more or less prolonged remissions or exacerbations, due to emotions or changed habits.

Micturition occurs at different times. Some children urin-

ate during the first sleep, which is the most profound (Vogel),[14] others toward morning (Trousseau),[15] others at any time during sleep. The number of micturitions is variable, but rarely exceeds three. Each child has its own habit in this respect, but the striking feature is the slight relation between the quantity of fluid ingested and the number of evacuations. Waking the child exerts a variable influence. Some children will thus avoid involuntary micturition, but often the incontinence is not modified. The patient is taken out of bed, empties the bladder completely, but hardly has he fallen asleep again when he micturates involuntarily although the urinary secretion is much less copious and the vesical pressure is certainly less than it was two hours before (Wollheim de Fonseca).[16]

We repeat, this is not true incontinence. On uncovering the patient during sleep, there is no visible flow from the meatus. The bladder is emptied intermittently, in a full stream as some authors say.

With regard to the amount of urine evacuated, no average can be furnished. One child who urinates only once, wets the clothing very slightly; another voids such a large quantity that it passes through the mattress to the floor; the quantity varies from one night to the other. It is interesting to note the amount secreted during each half of the day, when the child is awakened with sufficient frequency to make nocturnal incontinence impossible. We have made this experiment several times. The child is allowed to remain in bed for twelve hours, the night being assumed as equal to the day. The total nocturnal excretion generally has been less than the diurnal excretion, but we have sometimes seen a predominance of urine at night. In two cases (girls of 17 years and 14 years) the amount eliminated at night was much larger than that passed during the day; one of these cases does not inspire us with much confidence, because she was watched poorly by day and neglected several times to collect the evacuations.

At first we were tempted to attribute the incontinence to this relative nocturnal polyuria, but since we have measured the urine of normal children we have found that this occurs frequently. It is explained by the proximity of the hour for retiring to the dinner time, so that the children eliminate, during sleep, all the fluid ingested during dinner.

Patients suffering from incontinence sleep in two very dif-

ferent ways. Some sleep so profoundly that they can hardly be awakened, and they rise half asleep in order to urinate. But is this not the normal condition of healthy children? Others, on the contrary, are in constant agitation; they move, uncover themselves, and speak so as to disturb their neighbors.

During the day the urinary functions of the patient may be perfectly normal, and this is true of the large majority; the patient retains his urine as long as other children of the same age. On the other hand, he may have a frequent desire to urinate, which must be satisfied very quickly. Restrained by the discipline of the schools, he often fails to evacuate a portion of the vesical contents. There are thus two distinct varieties.

The character of these children has been but little studied. Bokai[17] remarks that they are sad, behind-hand, which he attributes to the bad treatment they receive and to the lack of *amour-propre* resulting from chastisement. Their external appearance has also been neglected or an incomplete and erroneous description has been given. Thus, Vogel[18] and Bohn[18] describe rickets and scrofula. Onimus attributes to them a mean appearance and a poorly developed muscular system. Others describe lymphatic children with flabby muscles and a pale complexion. Trousseau and Mondière have combated these statements; we will see later on that they are inexact.

The affection runs its course without regularity, sometimes disappearing completely for months. It may be suppressed entirely during the course of certain acute diseases (eruptive fevers), but returns during convalescence.

Complete cure is seen at times under such circumstances; this may happen after a lively emotion, at the period of puberty, or a first pregnancy. The recovery is often progressive, and the patient either adopts the habit of leaving the bed or has learned to control the vesical functions.

Pathogeny.

The theories of incontinence may be arranged under two heads, the psychical and the physico-physiological.

A.—J. L. Petit,[19] analyzing the cerebral condition of these children, divides incontinence into three groups.

1. The incontinence of children who are too lazy to rise in order to micturate at the first warning.

2. The incontinence of those who sleep so profoundly that the sensation which precedes the desire to urinate is not strong enough to awaken them (this is explained by saying that the voluntary contractility, that of the sphincter, is abolished and the organic contractility, that of the bladder, persists alone, or that the susceptibility of the bladder to distention by urine is diminished).

3. The incontinence of those who dream that they are micturating. Henoch, who adopts this etiology, compares it to nocturnal pollutions by erotic dreams.

This classification is very ingenious, but we do not think that it corresponds to the actual facts:

a. The laziness is not an explanation, because we must really interpret the more frequent desire to urinate. Pollakiuric children, who have no nervous taint, do not wet the bed. The laziness may explain the persistence of the trouble but not its first appearance. *b.* The deep sleep is merely a symptom of the cerebral condition of these subjects. It aids involuntary micturition, but this is a disturbance of a very complex nature. *c.* Dreams are rare. The child sometimes dreams that he is urinating, but this takes place at the very moment of micturition, and is then the mental perception of an actual occurrence. The dream is not the cause but the effect of micturition.

B. 1. *Disturbances of the Muscles of Urinary Excretion.* —*a. Vesical Irritability* (Trousseau, Vogel). From some unknown cause the muscular fibres of the bladder (detrusor urinæ) are too sensitive to distention and react violently. This theory is based chiefly on the fact that, during the day, the patients void a stream of urine of remarkable velocity and that the penis is almost always erect during sleep (?).

In only one case have we noticed this phenomenon of erection during involuntary micturition, and the stream of urine has not presented any peculiarity. A more important factor is the frequent desire to urinate during the day, which some children present. In them we must admit a slight degree of vesical irritability, but they never suffer from diurnal incontinence; they are merely compelled to obey an imperative desire. One of our patients passed urine involuntarily during the day, but only after meals, when she went to sleep during digestion. This vesical irritability also exists in pollakiurics,

but they do not wet the bed. Thus there is something which sharply distinguishes them from those suffering from incontinence. Vesical irritability does not suffice to explain the affection under consideration.

b. Atony of the Urethral Sphincter (Trousseau, Guyon). —In these cases the exploring bougie passes freely into the membranous portion without experiencing the ordinary constriction. We have never observed this and have always found that in boys the olive-shaped tip was grasped at the level of the terminal portion of the urethra. We may also remark that if atony of the sphincter system were the real cause, girls, whose muscular apparatus is less developed, would be much more subject to incontinence, but this is not true (Vogel, Hyrtl, Henoch). According to this hypothesis, incontinence should develop at a very early age (this is not constant) and should diminish with age in boys, according as the prostate approaches its full development, *i.e.*, toward the tenth or eleventh year (Dittel). Instead of improving, however, incontinence sometimes appears toward the age of five or six years, and often increases during the following years. In girls it is sometimes found to be aggravated at the approach of puberty. We may add that atony is contradicted by the fact that febrile diseases, which weaken the body, suppress incontinence.

Bokai unites these two theories in admitting a lack of equilibrium between the two vesical forces (destrusor urinæ and sphincter).

2. *Change in the urine* whose salts are so excessive as to irritate the bladder. Vogel has found nothing abnormal in the urine which he has examined, and examination of the specific gravity has also furnished normal figures.

3. We will merely mention the lack of sensibility of the sphincter, which, according to several authors, explains everything. The children feel well, even too well during the day, since they have frequent desire to urinate; why should they not have sensibility during the night?

4. Major, of Canda (Ziem),[20] has noted incontinence in children in whom an enlarged tonsillar system interfered with respiration through the nose; the insufficiency of hæmatosis producing carbonic-acid poisoning, is the cause of the incontinence. My friend Balme (Thesis, Paris, 1888) appears to accept this view, but himself furnishes the real solution of the

question by demonstrating, in his cases, that this adenoid hypertrophy is frequent in degenerated individuals.

Etiology.

Trousseau insisted upon the relations of hysteria and epilepsy to incontinence of urine. He thought that this disorder was sometimes a transformation of epilepsy, something like an aborted form. This comparison is a forced one, inasmuch as incontinence has no other relations to epilepsy than it has to all the neuroses or psychoses. In fact, all the various causes which writers have imagined, may be attributed to a single one, to nervous heredity in all its forms, to nervosisme and degeneration in all its degrees.

Examine the antecedents of all these children, analyze their somatic characteristics, moral tendencies and intellectual aptitudes, and the conviction will be gained quickly that they all possess a common taint, nervous heredity, and that all possess physical and psychical stigmata.

Heredity.—The following are the results of the study of 35 cases of incontinence. Nervous heredity in all its forms is the chief element in the statistics. Alcoholism is frequent in the father; one of the parents was violent, hot-headed, subject to outbursts of speech and action; or they suffered from severe neuralgia, violent headache at the vertex, angina pectoris, all forms of disturbance peculiar to neurasthenic and hypochondriacal individuals. Hysteria is found in the mother. A special manifestation which we have often found, since my colleague Gibotteau called attention to it, is dreaming in different degrees, from the vivid impression, which leaves a memory on waking, to the most perfect somnambulism in which a complex act is accomplished but not remembered. Convulsions in the relatives, epilepsy, mental failure, insanity and the various deliriums, should be noted; also strabismus, which has such intimate relations with nervous diseases. But in order to secure these data, a complete family history must be obtained and the inquiries pushed beyond the immediate family. In fact, we often find the most distinct stigmata among the collateral branches, the uncles and aunts. The father rarely visits the hospital and the mother does not like to mention the nervous phenomena from which she has suffered. Similar

heredity of incontinence, either from a parent to the child or from an uncle or aunt to the nephew, is very frequent. It is associated often, but not constantly, with the other above-mentioned manifestations. The latter may diminish and even disappear, and the incontinence is transmitted alone as the last trace of a nervous heredity whose other features are extinct.

It is no less interesting to study the other children in the family. We are informed, as a general thing, that the brothers and sisters of the incontinent patient have had some interesting functional anomalies. One of Gibotteau's observations is instructive from this point of view. In a family, two children (girl and boy) wet the bed, two sisters had had convulsions; one brother is sly, bizarre, with the peculiar look of the nervous, all the others are nervous and mean. In another family one brother has strabismus, or the other children are emotional and excitable to a high degree, or they are backward, unintelligent, began to walk at a late period; they are weak-minded or even idiotic. All the nervous stigmata thus appear isolated or united, convulsions, chorea, night terrors, mental weakness, or imbecility.

Personal Antecedents; Somatic and Mental Characteristics.—Finally, the study of the patient himself furnishes more than one lesson. In early infancy he may have had convulsions or night terrors, then he began to walk at a late period.

His physiognomy is often very characteristic, sometimes emotional and timid, with slight protrusion of the eyes, sometimes dull and inert, or suspicious and shy. The face and skull may be asymmetrical; the hairs, set improperly, form a double vertex (Féré considers this a trace of an error of development of the cephalic extremity of the vertebral canal) and descend very far on a low forehead. The palatine arch is very pointed and deeply excavated. The superior dental arch is retracted and consequently produces a more or less marked prognathous effect. The teeth are uneven, pointing in all directions and imbricated. The conchæ of the ears are poorly formed, inserted into the skull like a handle, with a sessile lobe. The genital organs sometimes present anomalies (phimosis, delayed descent or small size of the testicles, congenital hernia). These are the somatic stigmata assigned to degenerated indi-

viduals (Morel, Magnan, Legrain), but they are usually slight and few in number, and must be sought for.

The intellectual and moral characteristics of these children are also worthy of being noted, and from this point of view they may be divided into two distinct classes. One class includes weak, backward children and idiots (epileptic or not); these are rare in our hospitals and constitute the incontinent patients of asylums. The other class includes intelligent but emotional children, who dream at night, are restless during sleep, ashamed of their incontinence, desire to be cured, and submit voluntarily to treatment; some are undisciplined, of annoying vivacity, and are feared by the family; others are mischief-makers, liars, braggarts.

These characteristics appear to us to be important, and we cannot entertain Bokai's opinion, who considers the condition of timidity, "of moral and intellectual depression" of these patients as the result of the incontinence; "maltreated by their parents, lying on a hard bed, frequently punished, it is not astonishnig that they remain backward."

Nature of Incontinence.—We regard involuntary nocturnal micturition as a stigma of nervous heredity. It is, in a measure, a benign stigma, because, although it may coincide with profound intellectual degeneration, resulting from grave psychical heredity, it is also the manifestation of mild nervous heredity, of benign nervosisme. It may even be completely isolated as the last trace of this heredity. [The search for incontinence of urine in the antecedents of adult cases of nervous disease often gives positive results. We have not made a very extensive investigation, but may note, among others, the following instances: 1. A man suffering from transverse myelitis, one brother wet the bed until the age of 15 years; 2. A paraplegic of 15 years, his brother wet the bed until the age of 8 years; 3. An hysterical alcoholic of 17 years, had incontinence until 12 years old. Observations among degenerated individuals very often show the same thing. Dr. Lament, interne in the Central Infirmary of the Prisons, has found incontinence of urine several times during the childhood of degenerated criminals. Dr. Legrain, Physician-in-Chief at Vaucluse, has observed it more frequently and better developed among intelligent degenerated individuals.]

To heredity, also, we may attribute generally the incon-

tinence of boys suffering from phimosis, from hypertrophy of the tonsillar apparatus (at least in the cases under our observation), from congenital lesions of the heart (Day).[21] We admit, however, that the disorder may be excited by an acute disease (scarlatina), by bad hygienic conditions (coffee drinking before retiring), by general weakness, by adhesion of the prepuce.

From our previous remarks we will understand how improbable is the notion which attributes nocturnal incontinence to a local disorder of the bladder.

The affection is central, it results from improper function of the nervous centres, it is there that we must search for the cause. The emotions, the various perturbations of the nervous system, excite a very lively and immediate action upon the vesical function, "the bladder is the mirror of the soul" (Born). We will not be astonished, therefore, to find it disordered in an individual characterized by exaltation or lack of equilibrium of the nervous system.

The desire to urinate results from the sensibility of the bladder to distention (Guyon, Duchastalet). Micturition is effected by the simultaneous action of the vesical fibres which contract and of the urethral sphincter which relaxes. At the present time we accept the notion that this second part of the phenomenon is an act of inhibition. In the normal condition it is commanded and regulated by the will. Hitherto mention has been made only of the spinal centres of micturition, but it is evident that the brain also contains such centres, since the will can produce micturition apart from every desire, and, on the other hand, can prevent or arrest it despite this sensation.

At night, during sleep, when the bladder has acquired a certain tension, the desire makes itself felt (for if one happens to awake, micturition is usually necessary), but by a reflex unconscious act the sphincter system contracts and prevents emission. In the incontinent child this reflex, of superior origin, is wanting; the brain does not rule the cord. The patient sleeps too profoundly to accomplish this act; the spinal reflex of micturition alone is produced and evacuation takes place. It will recur as soon as the vesical tension has acquired a degree capable of exciting the same reflex. Nocturnal involuntary micturition is therefore an anomaly, a vice of function

of the nerve centres during sleep. Some children evacuate the bladder at the slightest pressure, and wet the bed three or four times during the night; in others the cord reacts only to a greater tension.

If the conditions of sleep are modified, if the child awakes before the tension has reached its maximum, the incontinence will not take place. The acute disease which disturbs sleep, stops drinking at night, and diminishes the urinary secretion, acts in this way. If phimosis exaggerates the spinal reflexes, the liberation of the more or less inflamed glans re-establishes the equilibrium of the nerve centres, and the nocturnal micturition ceases. If an exaggeration of sensibility in the posterior urethra is produced by any means (catheterization, electrization), the arrival of the urine there produces an impression sufficiently active to awaken the child. From this moment he assumes the habit of rising and urinates while awake. Little by little the urethral sensation may produce the cerebral reflex and contraction of the sphincter without awaking. Recovery is then assured, and, as if by the same reflex activity, the sphincter contracts so much better the stronger it is.

Emotions, the dread of public chastisement, produce the same effect by the nervous perturbation to which they give rise and by a simple effort of the will.

Diagnosis. Symptomatic Incontinence.

1. *General Weakness.*—Acute diseases are followed sometimes by incontinence which, concealed more or less during the day by frequent voluntary micturition, appears at night in all its intensity. This form is due to the weakness of convalescence. It yields readily to tonic treatment, and differs by its mildness and by the good effect of tonics from the form which has hitherto interested us. The children who are attacked are often free from all heredity.

2. *Albuminuria and Pollakiuria.*—The change in the urine and the resulting vesical irritation may produce incontinence in a feeble child who does not awaken spontaneously. Day reports such cases. A child of nine years is attacked by anasarca after measles, which had been followed by whooping-cough and scarlatina. Diurnal and noturnal incontinence then appeared. The urine contained a large amount of albumin

and had a specific gravity 1,020; it was very pale, frothy, acid; contained a few cells; no fever. Under a milk diet the incontinence disappeared, at first during the day, later at night. The albumin disappeared entirely. The child remained well for several months, then, during the winter, the albuminuria and frequency of micturition reappeared.

Henoch mentions incontinence in the course of diabetes or nephritis, but this has not come under my observation.

3. *Urinary calculi* rarely produce nocturnal incontinence alone. The local pains and the changes in the urine suffice to direct the examination and prevent error. Yet cases are known in which treatment was directed for a long time against the incontinence alone.[22] Urethral calculus causes permanent incontinence.

Examination of the bladder with a metallic explorer and rectal touch are necessary in order to diagnose vesical calculus.

4. *Diseases of the Cord.*—Remak[23] reports three cases of locomotor ataxia in children, a girl of 12 years and two boys of 14 years and 16 years, who suffered for a time from nocturnal incontinence. In two cases this was converted into diurnal and nocturnal incontinence at the end of several months; in the third the incontinence recovered spontaneously.

The diagnosis of these three cases was not doubtful because they were constantly under observation; but it is probable that, for a time, the presence of incontinence alone might have caused hesitation. In such cases the careful examination of the reflexes and the study of the movements will enable us to discover the spinal lesion.

5. *Incontinence from overflow* is rare in children apart from lesions of the cord (Pott's disease, transverse myelitis). A very curious cause has been noted by Hennig.[24] According to this author, it is found in certain chronic diseases that the abundant perspiration produces such constipation that enormous quantities of hard fæces accumulate in the rectum, and cannot be removed by enemata or purgatives. As a result the bladder is poorly emptied, becomes distended, and evacuation occurs constantly in drops, especially in boys. The diagnosis is made by rectal touch. In order to effect a cure it is sometimes necessary to scoop out the fæces with a spoon, and later to perform massage of the large intestine in order to prevent the return of the constipation.

6. Incontinence sometimes occurs from malformations of the bladder, urethra, or ureter, but it is constant, drop by drop, and will not give rise to error.

7. In rare cases children simulate nocturnal incontinence. We must be on our guard, and if this is suspected, not hesitate to use threats and painful measures of treatment.

Treatment.

The adoption of treatment should be preceded by a thorough examination of the genital organs and adjacent parts. It is particularly useful to examine for inflammations in the vicinity (vulvitis, balanoposthitis, rectal fistula, ascarides) and genital anomalies (phimosis, preputial adhesions). The cure of these affections by medical or surgical means suffices to relieve the incontinence. Catheterization with an exploring bougie with an olive tip, of a diameter conformable to the child's meatus, will determine the permeability of the urethra and neck of the bladder. Exploration of the bladder eliminates the presence of calculi. Finally, we must satisfy ourselves of the absence of a spinal or cerebral lesion.

When convinced of the absence of any external cause and of the spontaneity of the nocturnal disorder, we find ourselves confronted by a series of measures as numerous as they are discouraging. Although their value is very unequal, any one of them occasionally may produce recovery. The following are the principal ones.

1. *Medicinal.*—The list is infinite. Antispasmodics (bromides, belladonna) against the irritation; muscular excitants (strychnia, ergot); and tonics (iron, arsenic) against the weakened general condition; alkalies (bicarbonate of soda) against the acidity of the urine.

Trousseau recommended belladonna or nux vomica according as the bladder was too contractile or the sphincter insufficient. The rules for administration were formulated by him and stated precisely by Picard ("Prog. méd." 1886): "Trousseau began by giving one pill of extr. belladonnæ, gr. $\frac{1}{4}$ at night on retiring, for several days. Then, without regarding the cessation or persistence of the affection, he increased gradually the dose of the drug until he reached 1, 2, or even 3 grs. This was continued for a month or two, when recovery was effected, even if intolerance had not been produced.

"Nux vomica is administered, in children, under the form of syrup of strychnia (7 grs. to 3 ounces syrup).

"In children of five to ten years we begin, on the first day, with two teaspoonfuls, one in the morning, the other in the evening, for two days. If this dose is tolerated well, an additional teaspoonful is given after an interval of two days, *i.e.*, three teaspoonfuls are given for two days. Then, after another interval of two days, four teaspoonfuls are administered, and thus increased up to six, care being taken that the interval between the doses is measured accurately.

"When this dose is reached, a dessertspoon is substituted for the teaspoon, and at length, following the same rules, we reach six dessertspoonfuls (℥ ij. syrup, gr. ss. sulphate of strychnia). Finally, the dessertspoon is replaced by a tablespoon, and the dose increased, giving ℥ 2, 3, 4 syrup, *i.e.*,gr. $\frac{1}{2}$, $\frac{6}{10}$, and finally gr. i. strychnia. After the age of ten years we begin with a dessertspoon and finally administer in the same way, ℥ 6½ syrup, *i.e.*, gr. iss. of the active principle."

Bradbury has obtained some success with chloral. Tinct. rhois aromat. has succeeded in our hands in several cases.

2. *Hygienic* (frequent waking, tonics, salt-water baths, douches, restriction of fluids).—These means should not be neglected, but usually they alone do not suffice; they are merely adjuvants.

3. *Mechanical* (compressors, obturators).—Brutal and useless measures.

4. *Surgical.*—Repeated catheterization. This measure will not give permanent results unless repeated often.

5. *Electrical.*—Only the induced current has been used. Hertzker and Fieber apply it externally, one pole to the sacrum, another upon the symphysis. Ultzmann places one pole in the vagina or rectum, the other upon the perineum or hypogastrium. Seeligmueller stimulates more directly by introducing into the urethra a brass wire, 1 cm. long, which is covered, and forms the negative electrode; the positive pole is applied upon the symphysis. Erb applies the positive pole over the lumbar spine, the negative to the perineum (one to two minutes), then closes the sitting by introducing a metallic rod 2 cm. long into the urethra of boys or applying a sponge (one to two minutes) between the labia majora of girls. This second part of the operation should excite some pain. Guyon introduces, in

boys, a metallic olive as far as the membranous portion, and connects it with the negative pole; the other pole is placed on the symphysis; in girls the tip is applied to the neck of the bladder. The current should be weak, the interruptions few, and the sittings one to two minutes in duration. This method has given us some complete successes, improvements, and two absolute failures.

6. *Mental.*—Vogel and Henoch insist on the efficacy of certain measures which attempt to produce a violent emotion and to strike the child's imagination. The admission to hospital, various modes of punishment, and painful subcutaneous injections act in a psychical manner.

Liébault,[25] starting from these results and from the idea that the disorder is psycho-physiological (diminution of the perceptive faculty of the brain), has applied the method of hypnotic suggestion in favor at the Nancy school to 77 incontinent children between the ages of 3 and 18 years. This has effected a large number of cures (23 after one or two sittings, 23 after several sittings, 10 after prolonged treatment). "The older the patients were, the greater were their chances of recovery."

This method is a logical one, and we accept fully Liébault's conclusions. We should adopt the plan advocated by Bernheim in his book on suggestion, care being taken to free the child from every fear. More or less complete sleep usually is produced very quickly. It is well to multiply attempts at the same sitting, to waken the child and put him to sleep several times in succession, then to suggest a simple order which is to be performed on awaking. When the training is sufficient, the child is ordered to rise and urinate at a certain hour, then little by little the hour is delayed, the child being forbidden to wet the bed. The injunction is carried out very regularly. We will not enter into an analysis of these complex phenomena, because they present nothing peculiar to childhood. We have experimented with this method only upon three children under very unfavorable conditions. In one case we were unable to obtain sleep, in two others the sleep was so profound that none of our orders was carried out; all three patients were unintelligent. My colleague, Gibotteau, was more successful in two of our cases.

Case I. (personal). *Nocturnal Incontinence in a Degenerated, Nervous Individual; Nocturnal Polyuria; Direct Similar Heredity; Dreams and Nightmare.*—Camille D., æt. 17 years. Hereditary antecedents: Father incontinent until the age of 10 years; mother tubercular, extremely emotional, hypochondriacal, multiple neuralgias; brother tubercular, taciturn and egoistic, dreams of animals. Personal antecedents: Incontinence from infancy, intermittent prior to menstruation, constant since that period (14 years). Treated without success by urethral electrization, belladonna, douches, strychnine; during electrical treatment, did not wet the bed for three nights in succession. Present condition: A large, well-developed girl, extremely emotional, weeping every moment; slight facial asymmetry; ears sessile, no hysterical signs. During the day she urinates four or five times, the quantity normal (℥ 16–25 in 8 to 10 hours). She wets the bed at least three times a night, as she wakes at 12 o'clock, 2 to 3 o'clock, and 8 o'clock, and finds herself wet each time. The night urine, collected upon waxed cloth, measures ℥ 25–27 in 10 hours. If she remains awake at night, she rises often in order to urinate, but as soon as she falls asleep, she begins to secrete freely, and is always very wet on awaking (she never has an urgent desire). Dreams or menstruation have no influence on the incontinence. Sleep is heavy, disturbed only by the discomfort due to the wet; frequent nightmares, during which she sees dogs who bite her.

In May, 1887, Dr. Baudron employed urethral faradization for a few minutes every other day. After the eighth sitting, she wet herself only in the second half of the night, and several times not until after 4 o'clock. She awoke spontaneously more often than before, but after a month's treatment she became accustomed to the faradization (which had produced slight urethritis) and the incontinence was as severe as before.

In January, 1889, she is in the same condition, more nervous and depressed than ever.

Case II. *Nocturnal Incontinence from Infancy; Nervous Heredity; Somatic Stigmata; Weak Constitution.*—Louis P., æt. 3½ years. Hereditary antecedents: Father subject to violent migraine, threatened to kill his wife, alcoholic excesses without drunkenness. Mother tuberculous. No nervous disorders in collateral branches. Personal antecedents: Born

prematurely; poorly nourished by the nurse, ate solid food at an early period, severe diarrhœa when 1½ years old. Has always wet the bed, incontinence of fæces until the age of 2 years. Present condition: Child lean, pale, poorly developed; chicken breast without rachitis of the limbs; cranial asymmetry, lobe of ear sessile, palatine arch vaulted and deep. Left testicle at the ring; no urethral anomaly. The bed often wet half an hour after falling asleep. During the day the desire to urinate is sudden and imperative. Intelligence normal; very emotional. Treatment: A single faradization was followed by very violent spasm of the neck of bladder, preventing micturition for four or five hours and yielding spontaneously. Measles contracted in the ward prevented his return to the Policlinic.

Case III. *Nocturnal Incontinence; Similar Collateral Heredity; Somnambulism in the Father.*—Bertha S., æt. 3 years. Hereditary antecedents: Father nervous, somnambulist until age of 25 years; still talks occasionally during sleep. Mother's cousin suffers from nocturnal incontinence. Personal antecedents: Bottle-fed child until age of 2 years, passed a little urine involuntarily during the day; incontinence every night. Present condition: Child well formed, intelligent, very timid; cannot retain urine during period necessary for examination. Wets the bed after 11 o'clock and after midnight; when awakened she urinates very little, but appears to secrete much more freely as soon as she falls asleep. Is awakened with difficulty, but is very restless and in danger of falling out of bed. Drinks a good deal in the day, and, on retiring, a quarter glass of cider. Treatment: Cold douches to the perineum, night and morning; syrup of iodide of iron. After a few days she ceases to urinate at night, and retains urine much longer during the day. During a bronchitis, treatment was suspended and the incontinence returned; an attack of measles prevented her return.

Case IV. *Nocturnal Incontinence in a Brother and Sister; Nervous Heredity, Strabismus, Chorea; Cure of Incontinence by Measles.*—Hereditary Antecedents: Father very nervous, subject to tremor and attacks of anger; did not walk until the age of 5 years. Mother very emotional, violent fits of anger; 12 children, of whom 5 are alive; several are nervous and excitable; one boy wet the bed until the age of

4 years and soiled himself during the day; was cured by measles. One girl of 8 years had chorea last year; wet the bed until 3 years old, when she was cured by measles; this child is thin, and has right internal strabismus.

Case V. *Nocturnal Incontinence in a Girl of* 19 *since the Age of* 5 *Years; Convulsions during Childhood; Cured by Suggestion.*—H. G., æt. 19 years. Hereditary antecedents: Father subject to attacks of anger, disorderly. Mother and sisters healthy, not hysterical. Personal antecedents: At age of 5 years febrile disease lasting three months, with frequent convulsions; rarely wet the bed before this time. Present condition: Since then, incontinence several times a night. Sometimes wets herself during the day; the menses arrest the incontinence. Very profound sleep. No signs of degeneration; girl vigorous, emotional, weeps easily, slight exophthalmus, left ovaralgia, frequent temporal headache. First improved by few sittings of faradization; then treated by suggestion and cured by Dr. Gibotteau.

Case VI. *Incontinence from General Weakness; Recovery by Tonics.*—A. C., æt. 8 years. Hereditary antecedents: Father phthisical, mother vigorous, one brother rachitic. Personal antecedents: Born at full term, nursed at the breast, has always been delicate. Last winter, as the result of privation, began to wet the bed and the disorder increased gradually. Present condition: She urinates generally before midnight and toward 4 o'clock; waking her has been useless, although sleep is light. Never wets herself during the day, although the desire to urinate is always pressing. The child is pale, nervous, intelligent, evidently weak and poorly nourished. Treatment: Cold ablutions to the perineum, dry rubbings, syrups of iodide of iron, gentian; recovery.

Case VII. *Nocturnal Incontinence; Similar Collateral Heredity; Recovery from Entrance into the Hospital.*—F. C. Hereditary antecedents: Father alcoholic, mother nervous, emotional, paternal aunt had incontinence until marriage (18 years). Personal antecedents: Bottle fed, no ailments, incontinence from infancy. Nothing found in the genital organs. No incontinence during the stay in the hospital.

Case VIII. *Incontinence, Nervous Heredity.*—R. D., æt. 4½ years. Mother had chorea at 10 years, somnambulism since childhood, hysterical since marriage. Paternal uncle

had convulsions and atrophic paralysis of one limb. Sister healthy.

The patient very small, feeble, not very intelligent; preputial adhesions and slight phimosis. Rupture of the adhesions produced a temporary effect. Neither general tonic treatment nor suggestion were beneficial, and the child ceased his visits to the Policlinic.

Case IX. *Incontinence in the Father and Sister; Recovery by Suggestion* (reported by Gibotteau).—Clement and Ernestine V., æt. 12 years and 15 years. Mother hysterical and intelligent; aunt had convulsions which left her idiotic; the great-grandfather died insane; grandmother nervous; father drunk, choleric, cranky; grandfather had epileptic attacks at age of 53; uncle had attacks (?). Among 14 children, one girl of 14 and another of 8 years have had convulsions, a boy of 14 is peculiar.

Clement V. has always wet the bed, dreams often that he is urinating, and talks during sleep. Ernestine had convulsions at 3 years, has incontinence since an abscess of the vulva at the age of 7 years. Gibotteau treated both children by suggestion; the girl was cured in three sittings, the boy wets the bed much less frequently.

Case X. *Nervous Heredity, Mental Weakness; Cured by Rhus Aromaticus*—D. D., æt. 6 years; mother and grandmother hysterical; father died of angina pectoris (?), æt. 50 years; a sister had convulsions, æt. 2 years. The patient is well formed, intelligent and extremely careless, so that she soils herself with fæces during the day. Tincture of rhus given 10 to 15 days produced recovery; incontinence had not returned at the end of two months; the fear which we inspired appeared to act as much as the rhus.

Case XI. C. L., æt. 10 years. Parents healthy, with the exception of slight nervousness in the mother; a brother died of meningitis. The patient is a vigorous but excitable child, undisciplined, extremely turbulent; he is so restless during sleep that his neighbors complain. The incontinence began after a fall on the crotch. Faradization produced considerable improvement.

CHAPTER III.

NERVOUS POLYURIA OF CHILDHOOD.

KIEN distinguishes two forms of polyuria, that in which there is merely an increase of water (hydruria of Willis),[26] and that in which there is an increase of one or more normal elements, which he calls diabetes insipidus. He defines it as "the excretion for a longer or shorter period of urine which is notably more abundant than in the normal condition, or contains, more or less permanently, a sensible excess of normal solid principles. Despite the restriction of fluids, this quantity cannot be confined within physiological limits." Thus, according to him, polyuria, without an adjective, implies the absence of abnormal elements from the urine. Lancereaux[27] gives almost the same definition, "a morbid condition characterized by the exaggerated and more or less permanent discharge of urine of a low specific gravity, without sugar or albumin." He restricts it somewhat more closely because the specific gravity must be low, but he admits in the category of polyuria an abundance of urine with increased amount of a normal element.

True or simple polyuria does not involve a notable increase of the normal elements, it is hydruria. This will occupy our attention chiefly because it forms the largest number of nervous polyurias. We will have occasion to note examples of other varieties in order to distinguish them from the former.

NATURE OF SIMPLE POLYURIA.

The two most striking symptoms of the disease, increased amount of urine and exaggerated thirst, are closely connected, and it was natural to ask what was the primary disorder in the morbid evolution. The answer varies according to the bias of the observers and the cases which they have seen. One opinion has been held by the majority of authors, viz., that the thirst is the most important symptom; it is this desire and

its satisfaction which constitute the primary disorder from which all the others flow. In a word, polydipsia is the entire disease, and it is under this term that Trousseau published his clinical lecture on the subject. It seems that clinicians have avoided physiological discussions, finding it more simple to see nothing beyond a perversion of taste. But since Bernard's[28] discovery concerning puncture of the fourth ventricle, the question has been supposed to be settled, and every writer blindly accepts the theory of polyuria from lesion of the medulla oblongata.

Among the former writers we may mention Lacombe,[29] H. Roger,[30] Bouchut,[31] Guéneau de Mussy,[32] Trousseau, Romberg,[33] who attributes the disorder to a visceral hyperalgia; Kuelz admits the existence of polydipsia and polyuria.

Lacombe appeared to be justified in his opinion by the observation of a woman who, after various perversions of taste for solids, became polyuric, and he regarded the symptom as the result of a real desire, so that it is astonishing that he should reject the opinion of Sauvages,[34] who, likewise admitting the priority of the polydipsia, classifies it among mental diseases (vesania).

Bouchut wrote: "Polyuria is the result of polydipsia. The neuroses of taste are numerous and admitted by all physicians. What is known as pica, boulimia, dipsomania, are perversions of taste, in which appear peculiar appetites or antipathies to solids or fluids that are not eaten at all or only in moderate amounts. We cannot prevent chlorotics from eating charcoal, chalk, earth, paper, because they obey an imperative desire.

"In certain cases, boulimia and anorexia are simple neuroses of taste, independent of lesions of the stomach, and the passion of drunkards is due evidently to the pleasures of taste, whose imperative memory leads to desires that terminate in drunkenness. It is not unreasonable to believe that the thirst may be a neurosis of taste. Its consequences upon health depend on the nature of the ingested fluids. Immoderate taste for water or aqueous fluids corresponds to the abundant secretion of urine, *i.e.*, to polyuria and hypertrophy of the kidneys."[35]

Yet Bouchut seems to recognize a primary polyuria, because he mentions polyuria among the symptoms of nervosisme.

"The urine is watery, pale, odorless, abundant, especially

during the paroxysms, and it is discharged very frequently. This frequently repeated desire is extremely disagreeable, and ruled completely by the mental condition. The least annoyance, shame, or preoccupation, give rise to it at any moment. This is diabetes insipidus or polyuria." [36]

Guéneau de Mussy also raises the question of priority. He reports two cases in which the polydipsia preceded the polyuria, and he thinks that the malady consists of a perversion of thirst with vesania, "of that instinct which is called ingestion of fluids."

"There may be vesania of all the organic instincts, as there is of the intellectual faculties and moral sentiments. Boulimia and pica are aberrations of instincts." [37] Polydipsia is also a variety. He appears to be hampered in this conception, however, by clinical observation. One of his patients, who recovered, had diminished the amount of fluids ingested before the urine began to diminish. Hence he concludes that clinical cases cannot settle the question, that the dosage of the respective amounts of urine and drinks is difficult on account of the water of composition contained by solid food, and he believes finally that either one of the two symptomatic elements may appear first indifferently.

With similar notions Baudin [38] attempted to separate polyuria and polydipsia. According to him, there is polyuria whenever the solid matters are increased. This is equivalent to saying that there is no simple polyuria. According to him, every polyuria that is not produced by general causes is a polydipsia. These authors restrict very materially the domain of polyuria. Valleix also finds a distinctive characteristic. "In polyuria the urine is much more abundant compared with the amount of fluids ingested. In polydipsia, on the other hand, the quantity of urine is proportionate to the amount of fluids ingested."

In the lecture of Guéneau de Mussy, who admits the varieties mentioned above, we look in vain for cases which warrant the division into primary polyuria and secondary polyuria (vesania of thirst). But are these comparisons between dipsomania and other aberrations on the one hand, and the thirst of polyuria on the other hand, legitimate, or are they not, rather, more plausible than true? In polyuria we do not find the suddenness, the impulsive, imperative, illogical desire of dipso-

mania. "Dipsomania progresses by paroxysms of variable duration, separated by intervals of rest, in which the patient exhibits the most profound disgust for alcoholic drinks" (Legrain).[39]

The polyuric drinks because he is thirsty, with him it is not a question of taste or impulse. Deprive a dipsomaniac suddenly of his drink, sequester him, he is seized with terrible moral anguish, with a turmoil of the mind which may drive him to suicide, in a word with purely psychical disturbances. Nothing resembles the profound physical disorders which are produced in a polyuric by the abrupt withholding of water.

In fact, the drink of a polyuric cannot be withheld with impunity. The polyuria continues and we find it passing rapidly through the stages of a veritable torture of desiccation. The mouth becomes dry, the pharynx burns, the contractions of the almost empty stomach produce severe pain; the skin wrinkles, the lips become corrugated and cracked. This condition must be checked rapidly by a return to a sufficient amount of drink. We have not to deal here with the gratification of an ill-regulated passion or with the desire or habit of the organ of taste. The thirst is not a local, but a diffuse, general sensation. It is not the pharynx which is thirsty, but the cells and tissues of an organism whose normal functions are disordered. Hence, there is a primary polyuria. Is not the polyuria observed during protracted emotion, at the approach of danger or any sudden trial, of this nature? And yet, it is unattended with a sensation of thirst or, at least, this develops later.

On the other hand, the more careful study of the concomitant phenomena of polyuria leads to the same conclusion. We refer to the diminution of the cutaneous and pulmonary exhalations, the excretion of urine, which is often more abundant than the ingested fluids, as is shown by the figures of Strauss[40] and Pibram[41] and the investigations of Parkes and Neuffer.[42] If we accept these results it is difficult to conceive of primary polydipsia under such conditions, and it is not a solution to assume, with Kaurin, that the two symptoms are separate effects of the same cause. [Kraus, relying upon very well-conducted observations, affirms that the cutaneous and pulmonary exhalations have only a secondary importance in polyuria, and that they vary as in the healthy individual who

drinks a good deal. Kuelz also found that these functions were inconstant in diabetics. According to Kraus, the difference between the polyuric and the healthy man who drinks the same amount of water as the former and in the same time, resides not in the quantity of water eliminated (if we check suddenly and simultaneously the ingestion of water in the two subjects, the amounts eliminated will soon become nearly equal), but in the rapidity with which the polyuric eliminates. He is then tachyuric and is secondarily polyuric if he can repair by drinking the losses of water which he suffers.

Geigel thinks that the polydipsia may be primary, but that, on account of the permanent changes in the vascular apparatus of the kidney, which result from the excess of water, the polyuria follows and develops permanently.]

It is better to admit, with Senator and Lecorché, "either that a part of the excess of water is formed by the food, or that water is formed directly in the economy," which is equivalent to admitting an abnormal function which produces an increase of urine. In vain can the objection be raised that the excessive production of water should cause an elevation of temperature in the polyuric. This would be counterbalanced by the temperature of the ingested water.

We may admit, therefore, a displacement in the normal activity of the transformation of the ingesta which, diminishing the cutaneous and pulmonary exhalations (Neuffer), increases by compensation the urinary excretion.

Frequency.—Etiology.

Simple polyuria is frequent during childhood. In all the works which treat of this disease, we find it noted in childhood, and statistics show that it occurs with greatest frequency in childhood and old age.

It is unnecessary to say that the disease is not the same at these two periods of life; that which begins at an advanced age is always symptomatic. Among 20 cases Lacombe noted 9 which dated from childhood, among 31 cases Neuffer found 9 below the age of 10 years, Lancereaux collected 9 cases beginning between 5 and 30 years, Strauss 9 cases below the age of 16 years. We have been able to collect 29 cases of simple polyuria dating from childhood; this figure is less than that of other writers who have collated the most dissimilar

observations. The period of onset varies, but the majority of cases begin after infancy.

But even the period of nursing is not exempt. Legroux's patient furnishes an excellent illustration. The mother or nurse recognizes that the breast or bottle does not suffice, that the unsatisfied infant cries constantly, and from this period the polyuria has developed. It is not congenital except when hereditary in origin.

Sex exerts little influence on the development of the disease.

Among our 29 cases there were 15 boys and 14 girls; among Kuelz's 35 cases there were 19 boys and 14 girls. The latter table, however, contains very different elements (tubercular meningitis, bone abscesses, hydrocephalus).

Is there any relation between the diathesis and polyuria? Lacombe and Pain infer, from a certain number of cases, that the polyuric is often scrofulous or tuberculous. It seems clear that this is a simple coincidence or, at least, if tuberculosis is observed during the course of, and as a consequence of chronic polyuria, it is probable that we have to deal with a symptomatic diabetes insipidus with increase in the proportion of some normal element (azoturia or phosphaturia), as Teissier has observed in phthisical individuals. These observations are too incomplete to permit serious discussion of each particular case, but the most recent observations do not warrant the notion of such a relation. The young patients belong to all the various temperaments, lymphatico-nervous, sanguineo-nervous, and especially nervous. Those which have come under our notice did not correspond, in any way, to the scrofulous type. Nothing in their antecedents warranted us in suspecting tuberculosis, even for the future, and their health was generally excellent. This is also true of hereditary polyuria.

Exciting Causes.—Simple polyuria appears under the most varied conditions. In many cases we find some shock, some physical or mental perturbation, a vivid emotion caused by an animal, the sting of an insect (bee, beetle). In such cases we are tempted to attribute the symptoms to poisoning, but this cannot give rise to symptoms which last several years. It is much more plausible to attribute them to the nervous perturbation to which the fright and pain have given rise. Similar effects are produced by sudden chilling, by a cold bath while the body is perspiring, by the ingestion of iced drinks,

by all forms of traumatism, from the most violent—a kick upon the head by a horse's hoof (Charcot), a fall into a hole (Delpierre)—to the slightest injury, such as the mere effort to disengage a wheel stuck in the mire (J. Frank).

In the same category we mention the infections, typhoid fever (Bidard), intermittent fever (Leteinturier, Niemeyer), scarlatina (Kuelz).[43] In fact, infection may act like traumatism; in this way, hysteria, latent for a long time, appears during convalescence from an infectious malady (diphtheria, typhoid fever).[44] Poisoning by alcohol, mercury, and lead act in the same way in producing toxic hysteria. We may admit by analogy that polyuria may arise under the same conditions.

There are a very large number of cases in which we can find no indication with regard to etiology. The writers mention the number of litres of urine voided, the quantity of fluid ingested, and the condition of the skin, but they rarely furnish sufficient data concerning the antecedents.

Fortunately the more recent observations enter fully into the hereditary antecedents, and, by studying the concomitant symptoms, we are able to classify the disease in a more exact manner.

Influence of Nervous Heredity, of Hereditary Degeneration.—Before we began the study of polyuria, it seemed evident to us that a disease so protracted and obstinate, and yet so variable in its intensity according to circumstances, must be independent of organic changes. An analysis of its phenomena has led us, in fact, to regard it as a purely nervous disorder. We find here the neurotic characteristics of sudden appearance, permanence, and benignancy. In order to establish this theory, we will be forced, at times, to forsake the department of infantile pathology.

For a long time, says Ebstein,[45] the cause of polyuria has been sought in the nervous system. Guided by the researches of Bernard, by the notion of the transitory polyuria of emotional excitement, and by the polyuria due to injury to the brain, authors have interpreted simple polyuria as the result of a lesion or disorder of the bulbar centre. Pathological anatomy furnishes support for this hypothesis, inasmuch as polyuria has been observed during the course of diseases of the medulla oblongata. Ebstein collected eight cases of polyuria in which a lesion of the medulla oblongata was certain or probable.

Little by little, however, polyuria has been separated from an anatomical substratum, and the question has been raised whether it did not depend on mere functional disorder of the bulbar centres.

Such investigations had already been made with regard to diabetes mellitus. Seegen claims that the large proportion of cases of mellituria is associated with diseases of the nervous centres, and that a third of the cases result from depressing mental affections, particularly profound worry.

Griesinger[46] has shown that diabetes mellitus alternates with nervous diseases in psychopathic and neuropathic families. Ebstein, starting with the fact that diabetes insipidus is closely related to diabetes mellitus, since it may replace the latter, states that it may have the same origin, and mentions several cases of descendants of diabetics who became epileptic and insane. Marchal de Calvi reports similar cases. [Lasègue says, "we regard diabetes insipidus as entirely distinct from diabetes mellitus. When we see diabetes involve a considerable polyuria with a small amount of sugar, this happens usually after the glycosuria has lasted a long time. Under these conditions it is not rare to find that the diuresis, without sugar, remains profuse. On the other hand, not a single case has been observed in which polyuria, at first simple, was converted into glycosuria" ("Arch. méd.," 1866). Yet Walls ("Lancet," 1848) has seen diabetes mellitus and insipidus alternating with one another, and Traube and Bence Jones have observed analogous cases.] In fact, polyuria occurs during the course of mental diseases. Although this subject has been studied very little, we know that the urine is generally increased in paralytic dementia (Simon), reaches 65–100 ounces, that the urates are very abundant, and the specific gravity varies from 1,015–1,025.

Ebstein observed polyuria in a family of exalted, violent individuals, among whom one child was a microcephalic idiot, another was very violent and polyuric; in an epileptic woman of 25 years; in a man of 28 years, the son of an epileptic subject to attacks of grand and petit mal with aphasia. Kraus reported a very profuse chronic polyuria in a young epileptic of 17 years. In all these cases there is a close relation of the urinary disorder and cerebral disease. We will see that many cases of polyuria occur in neuropathic families, and that this

relationship, concealed if we confine our attention to the polyuria, becomes evident when we analyze the heredity.

Let us take an illustration (Axel Johannessen's case). A child is bitten during sleep by a beetle, which penetrates deep into the skin of the neck. On the following days the child passes a remarkably large amount of urine. It may be supposed that the injury, the fright, the poisoning, either singly or combined, have sufficed to produce this symptom, but if we investigate the family history it will be found that this abnormal mode of reaction is explained by the hereditary antecedents. "A family subject to mental disorders, a sister idiotic, a brother cranky, another brother suffering from incontinence of urine and sometimes of fæces; the mother subject to violent headaches, the father nervous and subject to migraine and palpitation of the heart." This suffices to show that the family has a neuropathic taint and that its members will react, by nervous symptoms, to traumatism. A child free from heredity would have suffered from dermatitis, swelling, and severe pains; in Johannessen's patient the polyuria is the result of the abnormal and isolated reaction of the secretory centres of the urine. Legroux's patient furnishes another illustration. This child is polyuric since infancy and appears to be intelligent. But look at its physical structure; palpatine arch deep, the teeth extremely abnormal, projecting in all directions from the apparently normal alveolar border, the eyes with that peculiar expression which is described with difficulty but which belongs only to nervous subjects. Nevertheless we might still remain in doubt, although this condition appears to us to be sufficient. The study of the collateral and ascending branches, however, will enlighten us. The paternal aunt is nervous, extremely excitable, subject to syncopal attacks; one cousin wets the bed, is neurasthenic, hypochondriacal and suffers from vertigo; the mother is hysterical. Thus the paternal and maternal sides combine to develop a purely nervous reaction.

Other cases will show us this heredity under different forms. More than once we will find, in addition to nervous antecedents, convulsions in the brothers and sisters, hysteria, epilepsy, incontinence of urine during the childhood of the patient or his relatives. The combination of these circumstances enable us to place these patients in the degenerated class.

We will here refer to one of the principal characteristics of this class, upon which Magnan has insisted so strongly, viz., the condition of "constant lack of equilibrium." "The degenerated individual seems to dissect himself," says Magnan, "and presents for observation the isolated functional activity of all the nerve centres, from the beginning to the end of the cerebro-spinal axis."

In the normal condition the centres, which are united functionally, supplement and inhibit one another; the spinal reflexes are moderated by cerebral influence, the psychomotor centres act in a synergic and coördinated manner.

In those suffering from lack of equilibrium, there is complete functional independence, and the same patient will present in succession the exaggerated activity of these isolated centres "united by twos or threes, without any order and without any volition or reason in directing them." A number of examples of such dissociation are found in Legrain's thesis.

There is nothing illogical in assuming an isolated stimulation of one or more of the centres of urinary secretion, manifesting itself in polyuria. This centre may act separately with the exaggerated activity peculiar to this class of patients, in a constant manner, but subject to important variations. Like the majority of nervous manifestations, polyuria requires for its development an external cause, an injury, emotion, acute disease, etc.

From this point of view a peculiar hypersecretion, viz., the sialorrhœa of nervous and degenerated individuals, must be compared with polyuria. Rayer and Mabille[47] have observed sialorrhœa in nervous, hysterical women, without any disturbance of the general condition. For several weeks we have watched an intermittent, but intractable and extremely profuse sialorrhœa in a non-hysterical, nervous woman of 20 years. Idiots suffer from ptyalism of such an amount that it cannot be explained by the absence of deglutition or by the presence of adenoid vegetations, as Coutéoux assumes.[48] This secretory disorder appears to be connected intimately with dynamic disorders of the nervous system.

Criminals form a class which have been studied by anthropologists for some years and have been found to possess very constant morbid characteristics. The analysis of their character, physical structure, and physiological aptitudes, explained

by the light of heredity, have led to the conviction that there is a class of criminals, "of moral idiots," who, born with the aptitude for evil-doing, fall inevitably into crime on account of the psychological taint which they present, and of which they are the victims. In a word, they belong to the class of "degenerated," and we find in them all the characteristics, the somatic and psychical stigmata, of grave nervous or mental heredity.

We have asked ourselves whether nervous polyuria, like incontinence, is not found in this class of patients. Our investigations have not been in vain; we have collated the observations in this work, although the majority of the patients were adults. It is a striking fact that they belong to professional criminals, subject to violent impulses, of inferior intelligence, abnormal character, illogical in their acts, true moral idiots.

A remarkable passage is found in Landouzy's Clinical Reports from the Hospital at Reims: "One comparison struck me. This boy, suffering from diabetes insipidus, has a very feeble intelligence and was about to be condemned for robbery. Eight years ago, in this very ward, was another boy of 16 years, also suffering from diabetes insipidus, also of very feeble intelligence and also condemned for robbery. This young man is now 24 years old, drinks about 20 litres of water daily, urinates in proportion, and has only a very moderate appetite. His urine contains no trace of sugar and registers only 98 upon Gay-Lussac's densimeter. His general condition is good, but his intelligence remains almost nil, and he can only be employed for little chores about the house.

"If, on the other hand, I compare with these boys, three girls, 15, 22, and 11 years old respectively, all three suffering from diabetes insipidus, with poor and capricious appetite, all three very intelligent, but affected with inexplicable manias, I adhere still more strongly to the opinion that this disease takes its origin in some cerebral disorder."

Hence there is a polyuria whose origin is purely nervous, which may or may not be associated with other disorders of the nervous system, and is generally secondary to a mental perturbation of varying intensity. It merits a special description in the pathology of childhood and adolescence, because it is most frequent at this period, and is most apt to be independent of an organic lesion.

Symptoms.

Onset.—Simple polyuria of childhood may be divided into two forms, polyuria of nervous origin and hereditary polyuria.

Polyuria of nervous origin begins very suddenly. The parents are sometimes struck by the avidity with which the child drinks, by the dryness of the mouth of which he complains, sometimes by the frequency of micturition. When it is due to injury, cold, or emotional excitement, the polyuria sometimes appears on the following night, more often a few days afterward. Maucotel[49] has noted it almost immediately after an injury. At the onset it may be overlooked because other cerebral phenomena engross attention; these are the signs of cerebral concussion, with or without fracture of the skull, more or less prolonged loss of consciousness, coma, contracture, incontinence of urine and fæces, paralysis, then disorders of vision, ringing in the ears, deafness, more or less permanent vertigo. Then all these symptoms disappear by degrees and the polyuria is left over. These symptoms need not attain such severity in order to account for the appearance of polyuria. They may be confined to loss of consciousness for a few moments; or even this may be entirely wanting and slight mental confusion may be the sole phenomenon which follows the injury. Grancher's little patient went home after his fall, and in Johannessen's case, the polyuria did not begin until four or five days after the bite.

When polyuria follows an acute disease, it begins probably during convalescence, but our observations are not very explicit on this point. Leteinturier's patient manifested the first evidences after an intermittent fever. The polyurias, whose causes have not been discovered, attained their maximum in a few days.

When the disease is established, it is manifested by a group of symptoms of which the most striking is the increased thirst. We do not insist upon the intensity of this symptom because it is directly proportionate to the urinary secretion. While in some children it is merely an eager desire, in others it becomes a veritable torture. The infant, hardly withdrawn from the breast or bottle, cries and protests, and, under the influence of the thirst, which is not understood by the mother, passes nights without sleep. The older child employs every means to

procure fluids, steals them if not supplied freely, and drinks everything that falls into his hands. The thirst varies; it is usually as pronounced in the night as it is during the day, and regularly wakens the patient. The quantities ingested agree very closely with those of the urine passed in the corresponding period.

If the child is deprived of drink, the dryness extends to the lips, which become fissured; the stomach experiences sensations of emptiness, prickling, even actual pain; there is sometimes vomiting, with palpitation and oppression. A child of eight years may then drink half a litre of fluid or even more.

Character of the Urine.—The amount of urine, like that of the drinks, is very variable. In a table furnished by Kuelz, we find 8½ quarts passed by a girl of 7 years weighing 37 pounds, 10½ quarts by a child of 4 years (Gerhardt). These are the maximum figures. The most enormous figures are undoubtedly those furnished by Vierordt, who reports 32 quarts passed by a boy of 6½ years weighing 27½ to 28½ pounds, so that this child eliminated an amount of urine greater in weight than his own body, in the proportion of 1.157:1. It is true that in this case the child died from some unknown cause, and we are warranted in separating the case from the others. As a general thing the less enormous amounts attain $\frac{1}{7}$, $\frac{1}{5}$, $\frac{1}{4}$ the weight of the body. Vierordt, comparing the amounts in children and adults, concludes that the child can furnish relatively larger amounts. In the case of polyuria in the adult in which the urine was most abundant (reported by Trousseau) the proportion was 0.666:1. Vierordt explains this difference by the fact that the weight of the kidney compared to that of the body is relatively greater in the child than in the adult. In a child of 6 years, the weight of the kidney is ℥ v., that of the body 37½ pounds (1:113), in an adult the proportion is ℥ ixss. to 143 pounds (1:232).

Micturition takes place at varying intervals, less often at night than during the day. The quantity passed in the day is generally less than that passed at night. In one case which we watched for several weeks, the nocturnal urine was ℥ vi.–xx. less than the diurnal urine. Pribram obtained different results, but the predominance of the nocturnal urine was slight. Schafino also found the nocturnal urine twice as large in amount as the diurnal urine.

The emission of urine generally follows rapidly upon the ingestion of fluids; according to Kuelz, it begins later in the polyuric than in the healthy man. On the contrary the investigations of Kraus show that when the organism is saturated with water, the polyuric secretes more rapidly than the healthy man who drinks the same amount of water.

Relation Between the Amount of Urine and Ingested Fluids.—Strauss and Pribram studied two cases from this point of view and concluded, from the following figures, that the amount of urine exceeds that of the ingested fluids.

Drink.	Urine.
13 qts.	9½ qts.
13 "	10½ "
12 "	11 "
59 "	59½ "
56 "	58 "
55 "	56 "
43 "	46½ "

One patient gave very variable results. In 18 days of observation, the amount of urine was less than that of the drinks 11 times, greater than that of the drinks 7 times. These variations must be attributed to losses in defecation and cutaneous perspiration, and to the water which enters into the composition of food and which cannot be estimated exactly. But if we admit, with Kuelz, that the polyuric secretes, in comparison with the ingested fluids, more urine than the healthy subject, and that this water is derived from the tissues, the disease cannot last long without affecting the general condition. The ingested water actually compensates for the losses.

Character of the Urine.—The physical properties of the urine possess but slight interest. They are these of all copious urine; a specific gravity falling to 1,001–1,000.5, feebly acid or neutral reaction, very pale color.

The examinations of the urine have generally been imperfect. Urea is the only element which has been measured with any frequency. Kuelz[50] says: "in Strauss' young girl (æt. 10 years, weight 24 pounds) the urea varied between ʒ iiss. and ℥ ss.; in Niemeyer's boy (æt. 10 years) between ʒ vij.–ʒ ixss.; Dickinson's patient (æt. 8 years, weight 22 pounds), who was tuberculous, passed ʒ iiiss.–vss. with a mixed diet; with vegetable diet the urea fell to ʒ iss. and increased to ℥ i. on animal

diet. In another girl of 6 years, weighing 28 pounds, the urea, under a mixed diet, reached ʒ iij.–vi., under vegetable diet ʒ iss.–ij. under animal diet ʒ vi." We have been unable to obtain a report of the cases referred to in this passage, and do not recognize the form of polyuria in which the urea reaches such high proportions (ʒ xiss.). The patients of Mosler and Chavains (traumatic polyuria) also eliminated increased amounts of urea, but the three analyses which we have made in our own cases give less than the normal amount.

Dickinson thinks the amount of phosphoric acid is diminished.

Inosite is found in polyuric children as in adults; the amount has not been estimated. Strauss, Kuelz, and Richardt do not attribute to it any pathogenic importance. This substance, which in the normal condition does not appear externally because it is converted into carbonic acid and water, may be extracted from the tissues and eliminated without change, by the lavage of the organism. It is often wanting.

In fine, in the polyuria of childhood, which is associated with a disorder of the nervous system, the constituents of the urine are generally present in smaller amounts than normal. This cannot be regarded as the rule, however, and traumatism may give rise to azoturia as well as to simple hydruria; hysteria and degeneration may also cause increase of chlorides. Teissier has also observed phosphaturia in adults of an irritable character, or of great impressionability, or in those who have experienced great worry; this phosphaturia is temporary and unattended with pronounced disturbances of nutrition. To what may this rare increase of solid constituents be attributed ? Becquerel [51] and Kiener [52] state that the persistence of polyuria gives rise at a certain period to the excessive excretion of normal products. But there is one condition which absolutely disproves this view. We refer to hereditary polyuria, in which the composition of the urine remains unchanged for a lifetime. In view of this contradiction we must remember how great are the variations of the different elements of the urine in normal subjects. May we not admit that, with a polyuria of similar origin, each individual, reacting after his own fashion, will differ from others by the excessive excretion of this or that element.

The traces of albumin found in two cases may be neglected.

The digestive functions are usually normal. The appetite may be retained or it is diminished in some nervous individuals, and this explains the slight excretion of urea. An increased appetite is observed in cases in which the urea is abundant. We are unable to say whether the hunger results from the azoturia or vice versa. Digestion is slow in some; it is accompanied by cephalalgia and cerebral distress. These are the signs of gastric dilatation. This dilatation might be expected to be frequent in these cases, but it has been noted in only two instances.

The ingestion of large amounts of water does not prevent good digestion of food. Indeed, Legroux's patient could eat well only after he had drunk, as is explained easily by the dryness of the mouth.

There may also be dilatation of the bladder, which often rises to the umbilicus or above it; this may be associated with nocturnal incontinence.

One phenomenon which is associated with the activity of the urinary secretion, is the immunity against the action of alcoholic drinks and drugs. Extract of valerian in large doses, ergotin in grammes, continued for several days, antipyrine in doses of ʒ iss. administered regularly, produce no bad effect on these children, because the active element of the drug is eliminated by the lavage of the tissues. The temperature of the body is lowered (Kuelz) on account of the loss of heat resulting from the increased elimination of water and the frequent ingestion of cold fluids. Pribram found the temperature greater or less than normal, according to circumstances.

The cutaneous perspiration has been studied by Strauss and Buerger.[53] This subject was very important formerly when it was supposed that in diabetes the amount of urine may greatly exceed that of the ingested fluids, and this was explained on the supposition that the skin absorbed water from the surrounding air. But it is certain that the difference between the urine and ingested fluids is never very great, and moreover the experiments of Nasse[54] have shown that the skin of diabetics does not absorb water, even in a bath.

In experiments made simultaneously upon a polyuric and a healthy girl, Buerger obtained the following results concerning cutaneous perspiration.

	Healthy Girl.	Polyuric Girl.
In 24 hours (child up)	℥ 30	℥ 22½
During the day	℥ 22½	℥ 18
During the night	℥ 8	℥ 3¾
In 24 hours (child lying down)	℥ 31½	℥ 17

According to Vierordt the perspiration constitutes 32–33% of the loss of weight of the body; in a patient Buerger found a loss of only 8%. The perspiration accordingly is diminished in the polyuric, and the water which should be eliminated in this way escapes partly in the urine. This is not always true, because, as we have seen, the patients sometimes eliminate less water in the urine than they have ingested in the shape of drinks. In fact, Kuelz, Engelmann, and Krauss have shown that the perspiration is of minor importance and that it varies considerably.

It is a mistake to admit, with some writers, an exaggeration of vascular tension. Roger's patient who suffered from epistaxis and palpitations was albuminuric, and thus differs from our polyurics. In Grancher's patient the arterial tension measured 10 upon the sphygmomanometer—much less than the average in adults.

Some children dread the cold (Dickinson), but despite the fact that the nutritive conditions appear so different from the normal, the polyuric children under our observation looked healthy. They were plump, vigorous, well formed, ruddy, active. After several years of the disease, a girl of 18 years remained vigorous; another is remarkable for her great obesity, but this is exceptional, if we credit Kuelz's table in which are shown the weights of 14 polyurics (unfortunately of very different origins), and which are less than the average weight of healthy children. We have met with no cases which warrant the following description by Bouchut:[55] "the skin is dry and dirty, the pulse small and feeble, palpitation of the heart is frequent; anæmia develops, which is so much more pronounced the longer the increase in the urinary secretion is continued; the children become sensitive to cold, they are low-spirited and in a condition of more or less pronounced languor."

We have already spoken of the nervous manifestations presented by these children: neuralgia, irritability, emotional character, convulsions in infancy. We do not dwell upon the varied disorders presented by the polyuric when he belongs

to a family of degenerated individuals. In such cases, if the intelligence is intact, we often notice moral anomalies, perversions, mental instability.

The beginning of menstruation is sometimes delayed, and yet the functions of generation are performed normally; a polyuric woman was the mother of 11 children, a polyuric man the father of several children.

We may add that polyurics are not refractory to contagion; they suffer from measles and smallpox. They tolerate injuries well and wounds heal kindly in them.

Course.

1. *Polyuria of nervous origin,* in childhood, is essentially chronic, and, as a rule, does not recover. Bouchut mentions a case of cure by injections of morphine; Lunin reports another, which was cured at the end of nine years, after various modes of treatment. Desgrange's patient recovered after a suppurating blister which had been applied during the course of a pleurisy. But as these cases were not followed up, we may doubt whether the recovery was permanent. In the large majority of cases polyuria lasts 20, 30, 40 years, leaving the patient generally in a good condition. Bidard's patient, a polyuric since the age of 19 years, "is in good health, although lymphatic;" another, 32 years old, sick since the age of 4 years, is "not very vigorous, with normal appetite, the gums pale." A man of 53 years, polyuric since childhood, is lean, with the thorax poorly developed, but he is never sick.

Death, which occurs rarely, is the result of accidental causes; one child died suddenly after taking a solution of nitrate of potash, a non-toxic substance (J. Frank).

Polyuria exhibits notable variations in its course. Acute febrile diseases diminish or suppress it, but Dickenson, von der Heijden, and Kuelz have observed the inefficiency of scarlatina, pleurisy, rubeola, and erysipelas.

Volition exercises very little effect on the symptom. Kuelz reports that, despite the promise of a bountiful reward, an intelligent child could not suppress the excretion for a single night. But Pribram and Geigel, starting with the idea that habit increases the desire for drink, have shown that the amount of fluid can be gradually diminished a little.

2. *Traumatic Polyuria.*—This does not differ from the adult form. It is more frequent in children, at least when unconnected with glycosuria. It is remarkable for its benignancy and may recover very quickly.

3. *Hereditary Polyuria.*—The reported cases do not mention at what age or under what influence the first case develops in polyuric families. But we know that, when once established, the heredity extends to a large number of descendants, both boys and girls; it may skip a generation. It begins at an early age, often during the nursing period.

This disease cannot be attributed to any diathesis or constitutional change. In Pain's cases, it was found in the tuberculous and scrofulous, the most polyuric being also the most scrofulous; but the great amount of phosphates (ʒ 1¼ in a child) in the only urine analyzed makes us believe that the case was one of symptomatic polyuria or of the coincidence of tuberculosis and hereditary polyuria. Weil gives a very different table: a family of vigorous and healthy people in which all the men did military service; the polyurics who died succumbed to some accidental disease. The intelligence of the members of the family was normal; very little mental affection, no neuroses. Nevertheless, there was a very large mortality among the younger members (14 among 91 members), and some anomalies, such as a supernumerary finger, ectopia of the testicle, right inguinal hernia in a boy of 5 years.

This form of polyuria is therefore compatible with the best of health.

Prognosis.

The prognosis of nervous polyuria, as we understand it, when unconnected with any renal lesion, is relatively good.

On the other hand, Bouchut [56] says: "Polyuria in children is a grave disease, which often proves fatal by consumption and after a comatose condition without paralysis. I have seen several children die in this way at the end of several years, but I have also seen others recover." This comatose condition resembles uræmia a good deal, and it may be suspected that such polyurias accompany albuminuria or glycosuria. Unfortunately we find, in Bouchut's works, no observation in support of this conclusion. Crapart [57] and H. Roger [58] give an extremely unfavorable prognosis, but, on examination of the

cases which follow this statement, we find that they refer to three cases of albuminuria, two with retinal lesions, one with purulent cystitis.

We must therefore conclude, like Senator, that the recovery of simple cases, without complications, is rare, but that death is also very rare.

Pathological Anatomy.

We do not know the exact anatomical conditions of the kidney in polyuria. It is certain that there are no primary lesions, but the excessive function should produce some changes either in the vascular system or the epithelium of the gland. Tuffier has shown that, after resection of a portion of the kidney in a dog, the part which continues to secrete undergoes true hypertrophy, affecting all the elements of the organ; the convoluted tubes and their epithelium are larger, the glomeruli are hypertrophied; the excretory tubes are dilated, the vessels enlarged. The entire organ gives evidence of intense functional hyperactivity without a trace of acute or chronic inflammation. This was easy to foresee, inasmuch as the exaggeration of function implies absence of inflammation, although it may finally favor it.

But if hypertrophy exists in simple polyuria it should be confined to the vascular elements, because there is no hyperactivity of the kidney except in regard to the excretion of water.

There have hardly been two or three instructive autopsies from this point of view, those which have been published referring, for the most part, to symptomatic polyuria. The best demonstration is furnished by the autopsies of Kiener (kidneys intact in a phthisical patient of 26 years, who died after polyuria of 10 or 11 years' duration), and that of Pribram (a boy of 14 years, polyuric since the age of 1 year; no change found in the canaliculi despite very careful examination of fresh and hardened sections).

Pathological Physiology.

This question has not advanced since Claude Bernard's experiment, which was supposed to explain everything. Since the experiments of Duret, who produced by cranial shock,

hemorrhages, ecchymoses, and bloody phlyctenulæ in different parts of the cranial cavity, it has been assumed that the traumatisms which produce polyuria, give rise to extravasations in the medulla oblongata or the floor of the fourth ventricle, and that their cicatrices continue to irritate the secretory centre. Everything was thus ascribed to a lesion of the medulla. Recently Johannessen, explaining the polyuria secondary to the bite of a beetle, assumed a spinal neuritis, which, extending to its nucleus of origin, reached by contiguity the nuclei of the vagus, acoustic, and inferior branch of the facial nerves (lingual and labial paresis) and finally extended to the centre of polyuria (?).

But we cannot admit the existence of an anatomical lesion in all these cases, for this will produce very severe cerebral phenomena from the affection of the neighboring parts (we know that they may be entirely absent after the injury). Furthermore, if such a lesion exists it cannot undergo a retrogression which is confined so constantly to an area only 1 millim. square.

A gross anatomical lesion, therefore, cannot be accepted.

The very situation of the centre is doubtful, for, on the one hand, Bernard's puncture produced only transitory polyuria, and, on the other hand, Schiff, Eckhard, and Thiernesse have obtained polyuria by injuring very different parts of the brain (cerebellum, inferior vermis), by irritating the sympathetic in the neck, and the dorsal ganglia placed along the course of the great splanchnic nerves. Kahler has observed polyuria in lesions which affect the parts situated in the occipital fossæ and the entire pons Varolii. Hence we are unable to localize the centre of polyuria. It seems to be a mode of reaction peculiar to certain individuals in the face of injuries or certain irritations.

The diagnosis need not detain us. The first condition to be fulfilled is to collect the urine and analyze it thoroughly. Careful examination of the nervous system is necessary, particularly in the tract of the cranial nerves. We there find the phenomena of paralysis or irritation which are the first evidences of cerebral tumor. The polyuria may really be connected with grave lesions of the brain or meninges, and in such cases it will soon be complicated by the symptoms of cerebral tumor, tuberculosis, etc.

Treatment.

1. *Hygienic.*—We can give no better advice than that of Kuelz: "avoid losses of heat by giving warm drinks, cover warmly, keep up the functions of the skin by frictions and warm baths; permit exercise proportionate to the strength; improve nutrition by cod-liver oil, iron, bitters, quinine."

2. *Medicinal.*—As none of the means hitherto employed has produced positive results, Kuelz recommends the greatest caution. There is hardly a drug which has not been employed in this disease. Almost all have had only partial success, or complete but temporary success, and the polyuria returns as soon as the drugs are discontinued or the system becomes accustomed to them. We may mention opium, valerian, nitrate of potash, belladonna, strychnia, bromides, acetate of lead, with opium, antipyrin, salicylate of soda.

All these drugs may be tried, particularly as the polyuria usually protects the patient against poisoning.

Galvanization, recommended by Seidel, may be tried; the rules for its administration depend upon each particular case.

CHAPTER IV.

CLINICAL CASES.

I. Traumatic Polyuria.

Case XII. *Acute Polyuria in a Child of Eleven Years, Secondary to the Bite of a Beetle* (ixodes ricinus). (Axel, Johanessen, "Arch. f. Kinderheilk," 1885.)—G. J., æt. 11 years, examined August 25th, 1882.

Hereditary antecedents: Paternal grandfather very strong and active, of an artistic family. Maternal grandmother had frequent severe headaches with aphasia. Several cases of psychoses in the family, one sister idiotic, a brother cranky. Patient's father suffers from migraine and palpitation. One brother has frequent headaches, one sister in good health, the youngest has had nocturnal incontinence for a year, and involuntary passages of fæces. Mother died four years ago of Bright's disease; pregnancy and nursing normal.

Personal antecedents: Dentition normal, child began to speak and walk at age of 1 year. At age of 6 years, after eating large amount of anticus sylvestres, had gastric pains, vomiting, and loss of consciousness and dilatation of pupils; rubeola in May, 1882.

August 16th, while asleep, was awakened by severe pain in occiput. An ixodes ricinus had bitten him severely behind and to the left of external occipital protuberance; the head remained imbedded after insect was pulled off; a painful swelling formed and extended to left ear. Pains in head and left ear on following day; patient pale and somnolent. August 22d. Walk is slow and heavy; in addition to headache, painful stiffness and spasmodic contraction of left muscles of neck, preventing motion of head. Memory poor; great thirst and frequent micturition. August 25th. Child answers vaguely during examination; face pale with an idiotic expression; pupils react slowly. Head stiff and drawn to the left; left trapezius contracted and painful. Pulse 108. Urine clear,

acid, sp. gr. 1,008, no albumin or sugar. August 31st. Condition aggravated after ride in a carriage, pains intolerable, movement impossible, increased stupor, hardly answers, delirium, skin dry and hot. Drinks 9 to 10 quarts daily, and passes 8 to 10 quarts urine. Very little sleep, pulse 64, temp. $102\frac{3}{10}$° F. Pupils dilated and react slowly; abdomen tender, slightly retracted. Two leeches behind ear. September 3d. Less pain, better sleep, about 10 quarts urine and drinks in 24 hours. Timid expression, irrational answers, photophobia. Urine, sp. gr. 1,003, no albumin or sugar, chlorides and phosphates diminished. Leeches, solution of nitrate of potash. September 7th. Calm, no pain, quantity of urine less; September 5th; 7 quarts; September 7th, 5 quarts. Answers clearly, more attentive expression, pupils react better. Head more movable, hearing diminished on left side; right angle of mouth deflected slightly to right side. Pulse 70, regular. Sept. 13th, cheerful, head moved easily, excellent appetite. Ideas still confused, no tension in neck or mastoid tenderness; pupils slightly dilated, still react slowly. Mouth and tongue deviate slightly to right. Abdomen softer, pulse 80, temp. 100° F. Urine, 2 quarts, sp. gr. 1,009, no albumin or sugar. Urine and drink from September 8th–13th:

	Urine.	Drink.
September 8th	4 qts.	4½ qts.
" 9th	3½ "	4 "
" 10th	2 "	2 "
" 11th	2½ "	2 "
" 12th	1½ "	2 "
" 13th	2 "	2 "

September 21st. Is doing well. Since September 16th amount of urine 1½ pints, drinks 1 quart. Memory has returned, pupils react well, hearing same on two sides. Deviation of tongue and mouth gone; walks easily, left the hospital September 30th. Condition same as before illness, urine, 1½ pint.

Case XIII. *Traumatic Polyuria in a Boy of Seven Years* (Reported by Queybrat).—George L., æt. 7 years, entered hospital March 5th, 1888. Hereditary antecederts: Father healthy but nervous; mother healthy, has frequent profuse expectorations which she thinks come from stomach (probably

intermittent sialorrhœa). Two sisters died young of pneumonia; a brother of 29 years, healthy; no convulsions in family. Personal antecedents: Born at full term, bottle fed, rubeola at two years, typhoid fever at five years, jaundice at six years. June, 1877, was struck on left forehead by a horse's hoof; no sign of cerebral concussion. A week or two later began to drink and urinate excessively (6 quarts). Present condition: Child large, vigorous, very little emaciated; no cranial or facial anomalies. Erythematous patches on right cheek which have lasted some time; they disappear on pressure. Tongue red and moist; becomes dry when child is deprived of drink for few hours, and speech becomes very difficult. Palatine arch pointed, teeth normal. Appetite fair, habitual constipation, thirst constant (5–6 quarts in 24 hours).

Liver, spleen, heart, and lungs normal. Patellar reflexes slightly exaggerated.

March 14th. Urine, 8 quarts, no sugar or albumin. The urine appears to be more abundant than the drinks. Treatment. Antipyrine in increasing doses of grs. 22, 37, 50, 65. Urine falls from 8 quarts to 7, then suddenly to 3½ quarts. How long this diminution lasted could not be ascertained, because the child confessed to have thrown away a part of the urine. When seen in January, child drank as much as before, but had not emaciated and appeared well. Potain's sphygmomanometer showed arterial tension 10, a figure much less than the average in the adult.

Case XV. *Polyuria in a Boy, after Ingestion of Cold Water while the Body was Sweating* (Reported by Lacombe).—F. G., æt. 14 years. Rather feeble constitution but always in good health. Father, mother, and sister healthy. At age of 10 years, while sweating, drank freely of cold water. Within next few days suffered intense thirst and began to urinate a great deal. Soon emaciated and appetite and strength diminished. At age of 12 years, passed 18 quarts urine. General health good, bowels natural, skin dry and cool; sometimes blue color of lips, nose, and cheeks. Drinks and urinates very often; generally wakes four or five times a night to drink and urinate. During the first sleep often wets the bed. Urine watery, 14–15 quarts, slightly acid, sp. gr. 1,000, no albumin or sugar. Treatment: Opium useless, powdered valerian, gr. x.–xv. Urine falls in two months to 5–6 quarts.

II. Polyuria of Nervous and Degenerated Individuals.

Case XVI. (Reported by Guinon).—M. B., æt. 13½ years. Hereditary antecedents: Father is 52 years old, healthy not nervous; paternal aunt extremely nervous, subject to fainting spells; one of her children wet the bed after the age of 3 years and again from 7–10 years. Mother, frequent migraine, began to walk at age of 4 years. One brother, æt. 13 years, died of meningitis, her three sisters are well, not nervous. Personal antecedents: First nursed at breast, then bottle fed; digestive troubles during first year; had measles and whooping cough before end of first year. While still at the bottle, was found to drink more than ordinary child, and as it was necessary to fill bottle constantly, child received starch water, wine and water, etc. Has lived in Paris for 1½ years; not very vigorous, easily tired. Drinks about 7 quarts water daily; could never reduce the amount below 4 quarts. If child was deprived of drink absolutely for 2 or 3 hours tongue became dry, lips fissured, and nausea and even vomiting resulted. Patient obliged to drink a glass of water before each meal, otherwise she was unable to eat. If she drank a good deal during the day, took less at night. Legroux began treatment in July; valerian, belladonna, sulphur baths every other day; drinks ingested fell to 5 quarts daily. Since admission to hospital urinates still less and becoming somewhat stouter. Present condition: October 1st. Child is very stout and vigorous, with a nervous look; not very intelligent, no cranial deformity; teeth inserted poorly, palatine arch very deep, lobes of ears sessile. Breasts large and fat, heart and lungs normal. Urinates less than on admission (3–4 quarts instead of 5–6 quarts). During the day urinates every hour, at night awakes about every three hours, chiefly on account of thirst. Eats moderately, often less than other children of her age; after meals, has flashes of heat in head and a feeling of constriction which makes her open her clothing; often has gastric distress, but does not vomit; constipation. Frequent headaches; has not menstruated. The amount of urine excreted is generally less than that of the ingested fluids, the difference being explained by the cutaneous and pulmonary exhalations. The urine is almost watery in appearance, sp. gr. 1,002, acid, contains small amount of inosite. December 7th. Child is again seen; still

urinates from 4–5½ quarts; eats better, is not constipated; has grown remarkably fat, the adipose layer of the abdomen making the part almost shapeless.

CASE XVI. *Polyuria in a Degenerated Individual* (Ebstein: "Ueber d. Beziehung d. Diabetes insipidus z. Erkrankung. d. Nervensystems").—A young man came under observation July 20th, 1871; the father and paternal uncles were known as violent, peculiar people; one brother is healthy, the other is almost idiotic, microcephalic. The patient is 26 years old, also a violent individual. At the age of 14 years, began to drink excessively of brandy, beer, and wine. The disease began in 1868; in 1869 he lost appetite, grew very weak, incapable of work; drank enormous amounts of fluids and hardly slept at all; at same time complained of distressing, painful cough. In 1874, after eight weeks' cure at Carlsbad, is so feeble mentally and physically that he is obliged to secure a travelling companion. At this time cried easily; had bronchitis at right apex; amount of urine enormous, clear as water, sp. gr. 1,001. When seen at the end of a year, patient appeared better, but still drank 12 quarts daily, and urinated equal amount; sp. gr. 1,005; cough had stopped; the least cause threw patient into violent passion.

CASE XVII. *Polyuria and Incontinence in a Degenerated Individual* (Communicated by Legrain, Physician in Chief at Colony of Vaucluse).—M., æt. 10 years, moral and instinctive perversions; tendency to thieving and vagabondage; from time to time melancholic depression with hypochondrical ideas. Physical stigmata; cranio-facial asymmetry, teeth implanted irregularly, development slow. Passed urine and fæces in bed until age of five years; then had incontinence of urine until ninth year; during the day emptied his bladder every half hour. Since his entrance, January 14th, 1881, has had nocturnal incontinence very often and pollakiuria during the day. For two months, the amount of urine collected in 24 hours (except that lost in bed) varied from 1½–3 quarts. Urine sometimes very clear, sometimes surcharged with urates or mucus; enormous excess of chlorides, otherwise normal. Heredity incomplete; father weak-minded, with tendency to melancholia.

III. Cases Without Precise Etiology.

Case XVIII. *Simple Polyuria in a Girl of Nine Years. Recovery After Nine Years* (Lunin, "Jahrb. f. Kinderh.," 1884, p. 420).—The child belonged to a family in which neither this nor any nervous disease was known. When two years old, considerable and progressive increase noted in amount of urine and thirst; no previous disease or injury. After this patient had scarlatina, rubeola, and whooping cough, without influence on the disease.

On entering the hospital, child was well developed, but a little pale, weight 47 pounds. Skin dry, axillary temp. $98\frac{2}{10}$° F; all the organs healthy. Ova of tænia mediocanellata found in the fæces. Patient was constantly thirsty and drank 9–10 quarts water; urine varied from 7–8 quarts, very clear, slightly acid, sp. gr. 1,001, no sugar or albumin. July 15th, about a month after admission, child daily took three, then four doses salicylate of soda, gr. vij. Amount of urine fell from 7–5 quarts in five days, but remained at this amount despite an increase of the dose of the drug.

July 21st—August 18th. Infusion of valerian, ʒ i. : ℥ iiss. Amount of urine fell progressively to $5\frac{1}{4}$ quarts. Then prescribed ergot in infusion, ʒ ss : ℥ iij. daily. The urine was thus reduced to ℥ xxxvij., and, after discontinuance of ergot, varied between ℥ xxxvij. and ℥ xl. The child remained in hospital, without treatment, for three weeks, passing same amount of urine; did not drink more than 1 pint water.

Case XIX. *Polyuria in a Woman, Beginning at Age of Three Years.*—Cath. B., æt. 35 years, lean, polyuric since childhood; after she was weaned, drank good deal of wine; soon afterward suffered from vomiting; toward age of three years drank a great deal; until her marriage (æt. 22 years) drank three pailfuls of water daily; afterward drank less (two pailfuls) but between the first and fourth pregnancies drank more.

When she is sick, drinks less; when she has been delivered of a child, is much more thirsty than usual. Thirst is manifested by a gastric sensation similar to the sensation of hunger. After she has drunk, an intense chill around the stomach compels her to stay near the fire. She has had eleven children in ten confinements; only two remain alive; almost all those whom she nursed suffered from various diseases; the

eldest living child has a skin disease similar to itch but it is not contagious. The youngest child, whom she nursed only a month, is in good health.

She is the only one in the family who has had great thirst. She perspires freely and the urine is proportionate to the amount of drinks; states that she eats a good deal. Rises at night every hour and a half in order to drink.

Case XX. *Polyuria in a Man Belonging to a Family of Diabetics* (Fisk, "Bost. Med. and Surg. Journ.," 1884).—The patient, æt. 28 years, had three diabetic uncles, who had succumbed to this disease. His symptoms began after an attack of rheumatism. He suffers from simple polyuria, which had been unaffected by different plans of treatment. There was a slight increase of urea, phosphoric and sulphuric acids.

IV. Hereditary Polyuria.

Case XXI. (Lacomb, loc. cit.)—Constant, æt. 59 years, in habitual good health, lean. His parents died at an advanced age; his mother was subject to intense thirst. Had two brothers who drank like himself, and who died during the wars of the empire. He says that a maternal uncle was polydipsic and that the latter's children suffered from the same disease. This uncle died at an advanced age, having had intense thirst from childhood; he left two sons and two daughters; one son and one daughter had intense thirst from early childhood; the former died of cholera, the latter of phthisis. The patient drank much since childhood, has chronic diarrhœa (4–5 passages daily).

Mother polyuric.			Uncle polyuric.			
son polyuric, diarrhœa.	son polyuric, died in the wars.	son polyuric, died in the wars.	son healthy.	son polyuric, died of cholera.	daughter healthy.	daughter polyuric, died of phthisis.

Case XXII. (Samuel Gee, St. Bartholomew Hosp. Rep. Vol. XIV., 1877.)

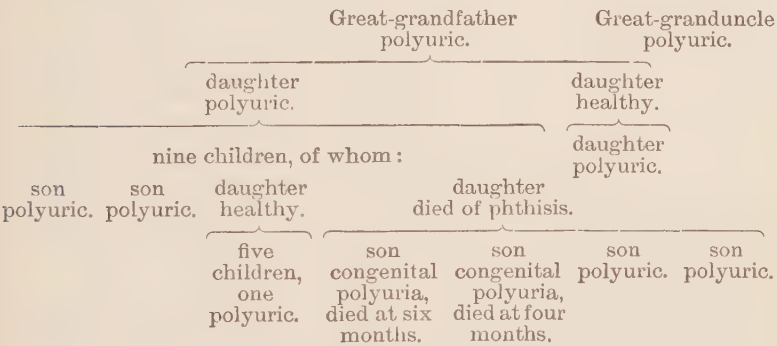
A. 1st Generation. Great grandfather polyuric since childhood, paralyzed at 28 years, died at 44 years. A brother also polyuric.

B. 2d Generation. Patient No. 1 has two daughters, one

polyuric, the other not; the latter has a daughter suffering from some disease.

C. 3d. Generation. Woman No. 3., has 9 children; two suffered from polyuria, two other daughters, who were not sick, transmitted the morbid predisposition to their children; the first had one polyuric son among five children; the second had four sons, all affected with diabetes insipidus; two died young, the other two were treated by the author. Their mother died of pulmonary phthsis.

The polyuria was congenital in a child of the fourth generation. Since birth, it cried incessantly and was tormented by insatiable thirst; it was quiet only when water was given to it constantly; died at the age of 6 months. His brother had same symptoms and died at 4 months. The two surviving children were 8 years and 9 years old; their thirst was constant and resisted all medication.



V. Symptomatic Polyuria.

Case XXIII. *Alkaline Polyuria After Diphtheria* (Whittle, "Dublin Quart. Med. Journ.," 1867, Jan.).—Girl of 11 years, attacked by diphtheria; at end of a week, appetite returned, but fever persisted. Urine alkaline, 1,008, no albumin. Was then allowed to go about. On 12th day, unable to rise, headache, skin hot and dry, urine still more alkaline, 1,002. Polyuria, child drinks only water. Treatment: Dover's powder. Better on following day, less fever. At the end of a month, little improvement; passes 5–6 pints of urine, 1,002–1,003, always alkaline. Violent paroxysms of headache which disappear spontaneously after abundant passage of urine. Quinine

replaced by chloride of iron with good results; headache less, urine slightly diminished. March 12th. Urine diminished, 1,008. March 18th. Urine 1,010, neutral; appetite restored, headache almost gone. March 26th. Urine 1,013, acid for first time, quantity nearly normal. The advice to continue treatment for a week disregarded. April 2d. Headache returned, urine again alkaline, increased amount, traces of albumin. Treatment renewed. Improvement. April 12th. Urine acid, 1,013, soon entirely normal.

Case XXIV. *Polyuria in a Child During the Course of a Tubercular Meningitis* (Hagenbach, "Jhrb. f. Kinderh.," 1882, XIX.).—R. G., æt. 4½ years. Diarrhœa in infancy; in winter of 1879–1880 became depressed and drank good deal of water; appetite greatly diminished and then could drink only milk and water; urinates excessively. Dec. 23d, 1880. Cries readily; for some days complains of weakness, sleeps much and is hot at night. No injury; parents living and healthy; a maternal uncle died of lung trouble. Present condition: child emaciated and pale, slight fever; nothing abnormal in the chest; no other nervous phenomena except great irritability and restlessness; urine abundant, sp. gr. 1,004, no sugar or albumin. Dec. 24th. Vomiting. December 25th. Headache. December 27th. Sudden loss of consciousness, contracture of limbs, very rapid pulse; then convergent strabismus, stronger convulsions on right side of body; marked somnolence. January 2d, 1881. Pupils dilated, somnolence, clonic convulsions of limbs; patient died January 6th. During all this time there had been fever, the average quantity of urine was ten quarts, sp. gr. 1,000–1,004; quantity was much less during last few days. Autopsy: Cheesy tubercle of infundibulum, tubercular meningitis, spot of softening in right corpus striatum; tubercular peribronchitis at the apices, recent miliary tubercles of left lung.

Conclusions.

I. There are, in children, a certain number of functional disorders of urinary secretion and excretion without lesions, either local or remote.

II. Incontinence of urine or nocturnal involuntary micturition and simple polyuria are the chief disturbances.

III. These disorders are observed at all periods of child-

hood. They result from the perturbation or irritation of a cerebral or medullary centre. They are associated with neuropathic heredity, and coincide with other mental or somatic anomalies in the patient or his relatives.

IV. Nocturnal incontinence appears in two distinct classes of children, those who are feebly constituted or are weakened by an acute disease, and, on the other hand, those who have inherited a nervous taint or are degenerated in various degrees. Neuropathies (hysteria, insanity, spinal lesions, etc.) are found in the ancestors. The disease is a benign stigma of nervous and psychical heredity.

V. Simple polyuria is also observed more frequently in children with neuropathic heredity. It co-exists with incontinence of urine in the same families. It may result from the most varied nervous disturbances, but when the latter appear insufficient we always find an hereditary predisposition to abnormal manifestations of nervous activity (degeneration or neuropathic heredity). It does not affect the general condition, and its duration is indefinite.

Polyuria which is traumatic or secondary to an acute disease alone appears capable of recovery.

Hereditary polyuria is a simple polyuria; it cannot be attributed to any diathesis.

BIBLIOGRAPHY.

1. Bouchard: Maladies par ralent. de la nutrition, pp. 25, 26.—2. Féré: Arch. de neurologie, 1884.—3. Féré: Loc. cit.—4. Neumann: Arch. neurol., July, 1888: Union méd., Nov., 1888.—5. Ballet: Soc. clin., 1888, France médicale.—6. Graucher: Gaz. méd. de Paris, June 9th, 1888.—7. Picard: Thèse, Strasbourg, 1856.—8. Bouchard: Mort par inanition, Thèse, Paris, 1864.—9. Parrot and Robin: Arch. gén. de méd., 1876, vol. i.—10. Cruse: Ueber d. Verhalten d. Harns b. Saeuglingen, Jahrb. f. Kinderheilk., 1877, vol. xi.—11. Camerer: Ueber d. Stoffwechsel d. Kinder., Zschr. f. Biologie, 1878, and Jahrb. f. Kinderheilk., 1880, vol. xv.—12. Schabanowa: Beitr. z. Kenntniss d. Harnstoffmengen, etc., Jahr. f. Kinderh., vol. xiv., p. 281.—13. Bokai: Gerhardt's Handbuch, vol. iv., 3d. edit., p. 538.—14. Vogel: Traité des maladies des Enfants.—15. Trousseau: Clin. méd., vol. ii.—16. Wollheim de Fonseca: Inaug. Dissert., Kiel, 1878.—17. Bokai: Gerhardt's Handbuch, vol. iv., pp. 547–555.—18. Bohn: Jahrb. f. Kinderh., 1870, vol. iii.—19. J. L. Petit: Œuvres chirurgicales.—20. Ziem: Allg. med. Cent.-Zeitung, 1885.—21. Day: Brit. Med. Journ., 1886.—22. John Lunin: Lancet, 1884, vol. i.—23. Remak: Berl. klin. Wschr., 1885, p. 105.—24. Hennig: Jahrb. f.

Kinderh., xxiv., p. 367.—25. Congrès de Nancy, 1886; Revue hypnotisme, Sept., 1886.—26. Kien: Thèse, Strasbourg, 1865.—27. Lancereaux: Thèse agr., 1869.—28. Bernard: Leçons physiol. expériment., 1854-55, vol. i.—29. Lacombe: Th., Paris, 1841.—30. H. Roger: Journ. de méd. et chir. prat., 1866.—31. Bouchut: Leçons cliniques, Gaz. des Hôp., Nov., 1877.—32. Guéneau de Mussy: Clin. méd., vol. ii.—33. Romberg: Lehrb. d. Nervenkrankh.—34. Sauvages: Nosologie méthodique.—35. Bouchut: Gaz. hôpitaux, Nov., 1877.—36. Bouchut: Traité du nervosisme, p. 244.—37. Guéneau de Mussy: Clin. méd., vol. ii., p. 239.—38. Baudin: Th., Paris, 1855.—39. Legrain: Th., Paris, 1886.—40. Strauss: Die einfache zuckerlose Harnruhr, Inaug. Dissert., Tuebingen, 1870.—41. Pribram: Prager Vierteljahr., 1871, p. 28.—42. Neuffer: Diabetes insipidus, Tuebingen, 1856.—43. Kuelz: Gerhardt's Handbuch, vol. iii., 1878.—44. George Guinon: Thèse, Paris, 1889.—45. Ebstein: Ueb. d. Beziehungen d. Diabetes insipidus z. Erkrankungen d. Nervensystems, Deutsch. Arch. f. klin. Med., vol. xi., p. 344, 1873.—46. Griesinger: Studien über Diabetes, Arch. f. physiol. Heilk., 1859.—47. Rayer: Dict. Jaccoud, Art. de Letulle.—48. Coutéoux: Rev. de clin. et de thérapeutique, Dec., 1888.—49. Maucotel: Th. de Paris, 1883.—50. Kuelz: Loc. cit.—51. Becquerel: Séméiotique des urines.—52. Kiener: Th., Strasbourg, 1866.—53. Buerger: Deutsch. Arch. f. kl. Med., 1873, vol. ii.—54. Nasse: Arch. f. phys. Heilk., x.—55. Bouchut: Loc. cit.—56. Bouchut: Loc. cit.—57. Crapart: Th., Paris, 1865.—58. H. Roger Journ. de méd. et chir. prat., 1865.

VARICOSE VEINS

OF THE

LOWER EXTREMITIES.

BY

WILLIAM H. BENNETT, F.R.C.S.,

Surgeon to St. George's Hospital; Lecturer on Clinical Surgery and on Anatomy in St. George's Hospital Medical School; Member of the Board of Examiners Royal College of Surgeons of England.

VARICOSE VEINS OF THE LOWER EXTREMITIES.

CHAPTER I.

Of the advice so abundantly offered to the student during his clinical studies, there is probably none more thoroughly sound than that which urges upon him the necessity for acquiring an intimate knowledge of the most common diseases, and at the same time warns him against devoting too large a share of his short curriculum to the study of those rare and interesting cases which are naturally so fascinating to us all. Such advice cannot be often enough repeated, nor can it be too closely followed, for I presume it must fall to the lot of most of you, a little later in your career, to be called upon to treat diseases which are ordinary rather than rare; and you may take it as a fact that by the success which follows your treatment of these common affections will your ability, for the most part, be estimated by that section of the public from which your patients come.

However much the out-patient departments of our general hospitals may be from time to time adversely criticised, there is not the least doubt that—at present at all events—they afford the only accessible means by which the study of common diseases is open to the ordinary student, and those of you who have attended my practice among the out-patients here will recollect how constantly I was in the habit of calling your attention to valuable series of these common affections.

I hope, therefore, that I need offer no apology for devoting these lectures to the consideration of varicose veins of the lower extremities, one of the commonest as well as one of the most troublesome of surgical affections.

I propose to take as a basis for my observations the accompanying tables, in which are included, I believe, all the cases which came under my care in the out-patient department of St. George's Hospital, in which the varicose veins were obvious and pronounced, and in which treatment was sought either on account of the abnormal veins themselves or for some condition directly dependent upon them.

Twenty-three other cases, not tabulated, in which the symptoms were unassociated with any obvious varicosity are of interest, and will be considered separately. I shall also have to refer to various cases which have been under my care in the wards of the hospital and in private practice.

At the expense, I fear, of being rather wearisome, it is my intention first to briefly consider *seriatim* the several points suggested by the headings of these tables—not that I suppose any new or startling facts will be forthcoming, but because, so far as I know, it is a long time since any analysis of such a large number of cases of this affection has been published, and also because I believe it cannot be otherwise than profitable to occasionally test the accuracy of teachings some of which owe their acceptance to little more than tradition, and which may in course of time require modification.

Relative Number of Cases occurring in the two Sexes.

The number of cases among the females exceeds that on the male side by 21.62 per cent, which may be easily accounted for by the large proportion of cases apparently dependent on pregnancy; indeed, if the column of causes under this head be noticed, it is manifest that in the absence of pregnancy the number of cases in females would be considerably exceeded by those in the males. This is more particularly seen in cases affecting both limbs.

Taking the cases in which the affection is limited to one side, the number in males actually exceeds that in the opposite sex (particularly on the right side), in spite of the probable influence of pregnancy as a determining cause.

Extent of the Disease and the Veins involved.

General Varicosity Throughout Leg and Thigh.—On both sides. Here the difference in the number of cases occurring

Table A.—Males (259 Cases).

Limb or Limbs Affected.	Total Number of Cases.	Veins involved.						Circumscribed Varix at Saphenous Opening.			Apparently Predominating Causes.				Age of Patient when Disease was First Noticed.				Predominating Complications.									Uncomplicated.
		Generally Throughout Leg and Thigh.	Generally Throughout Leg Below Knee.	Popliteal Region and Outer Side of Leg.	Internal Saphena Only.	Veins Over Internal Saphena Only.	Veins Over Calf of Leg Only.	Both Sides.	Left.	Right.	Constipation.	Hereditary Tendency.	Injury.	Doubtful.	Before Twenty-five.	Between Twenty-five and Forty.	After Forty.	Doubtful.	Piles.	Varicocele.	Varicose Veins Over Pubes.	Ulcer.	Eczema.	Persistent Œdema.	Thrombus (Recent).	Evidence of Old Thrombus.	Valgus.	
Both..	92	58	.....	15	19	....	....	7	15	6	45	20	...	27	40	46	....	6	13	9	1	29	15	....	.....	In	5	20
Left ..	93	39	26	.....	25	2	1	....	20	.....	36	15	1	41	13	29	25	26	8	6	1	30	7	3	9	many	....	29
Right.	74	29	25	5	10	2	3	....	... .	6	8	39	2	25	41	9	11	13	...	2	....	8	13	....	17	cases.	....	34
.....	259	126	51	20	54	4	4	7	35	12	89	74	3	93	94	84	36	45	21	17	2	67	35	3	26	.. ...	5	83
.....	...	48.62	20.07	7.72	20.85	1.54	1.54	2.70	13.51	4.63	34.36	27.02	1.15	35.90	36.29	32.43	13.89	17.35	8.10	6.56	0.77	25.86	13.51	1.15	10.03	.....	1.93	32.04

Table B.—Females (315 Cases).

Limb or Limbs Affected.	Total Number of Cases.	Veins Involved.						Circumscribed Varix at Saphenous Opening.			Apparent Causes Occurring Singly or in Combination.				Age of Patient when Disease was First Noticed.				Predominating Complications.								Uncomplicated.
		Generally Throughout Leg and Thigh.	Generally Throughout Leg Below Knee.	Popliteal Region and Outer Side of Leg.	Internal Saphena Only.	Veins Over Internal Saphena Only.	Veins Over Ankle and Foot Only.	Both Sides.	Left.	Right.	Pregnancy.	Constipation.	Hereditary Tendency.	Injury.	Before Twenty five.	Between Twenty five and Forty.	After Forty.	Doubtful.	Piles.	Varicose Veins in Labia.	Ulcer.	Eczema.	Persistent Œdema.	Thrombus (Recent).	Evidence of Old Thrombus.	Anæmia and Menstrual Irregularities.	
											Combined.																
Both..	163	140	14	....	9	.....	.....	....	14	13	131	59	?	. .	20	119	.....	24	42	2	47	31	2	23	In	10	6
Left ..	91	43	8	13	27	.....	.....	....	6	....	15	63	26	....	31	7	.....	53	14	.....	33	9	4	7	many	18	6
Right.	61	.....	15	13	24	2	7	....	.....	6	6	7	34	2	34	13	14	.....	3	... .	13	.....	....	....	cases.	16	29
.....	315	183	37	26	60	2	7	...	20	19	152	139	60	2	85	139	14	77	59	2	93	40	6	30	.....	44	41
.....	....	58.10	11.74	8.25	19.04	0.63	2.22	....	6.34	6.03	....	.....	.....	....	26.98	14.12	4.44	24.44	18.73	0.63	29.52	12.69	1.90	9.52	.....	13.90	13.01

in the two sexes is most marked. In many of the cases on the female side there was a distinct history of the affection having first shown itself very slightly in one leg, and then to have subsequently affected both limbs, equally, after two or three pregnancies, the limb originally affected being, as a rule, the left. In some of the cases the disease commenced in both limbs simultaneously after the first pregnancy.

On the left side only. Here the number of cases is about equal in the two sexes, being slightly in excess on the male side.

On the right side only. It is remarkable that in the females not a single instance of general varicosity was noted, although in three cases it was stated, by patients who were affected in both limbs, that there had first been varicosity on the right side, the left having become afterward involved, in consequence of pregnancy in two of the cases, and "low fever" in the other.

Among the males, on the other hand, there was evidence of general varicosity in many cases; and it is noteworthy that, in five of these, there was fulness of the veins throughout the whole limb at the time of the first appearance of the disease.

General Varicosity not Extending Above the Knee.—Here, again, the difference in the number of cases in the two sexes is very marked, and is not easily explained. On the male side not a single instance presented itself in which both limbs were involved, whereas the females produced fourteen cases; in three of these the disease appeared in the second limb after the first pregnancy, and as the patients were young, it is probable that successive pregnancies will, if they occur, entail varicosity throughout both limbs.

Of cases limited to the right or left limb only, it will be seen that the males afford far more examples than the females, the difference in numbers being less marked in the right limb, although in that the number is much greater in the males.

Varicosity of the Veins over the Popliteal and Fibular Regions Only.—Of this condition no instance occurring in both limbs was seen among the females, but it was stated by a few of those who presented themselves with general varicosity of both limbs that the disease was first noticed in these regions, and gradually increased.

In the males there were fifteen cases, but I could obtain no evidence of the condition having become general in any man.

In the examples of this class which were confined to one limb the women were affected to the extent of thirteen cases in each limb, whereas the men provided no instance in the left limb, and only five in the right.

Varicosity Limited to the Internal Saphena and its Immediate Branches.—The aggregate number of these cases was rather larger in the females, but the instances occurring in both limbs were very much more common in the males. When confined to the left limb they were almost equal in numbers in the two sexes, but in the right limb the cases on the female side very greatly exceeded those in the males. In all the patients seen with this condition the internal saphena was affected from end to end, including its radicles on the foot; there was no fulness or increase of tension about the deep parts of the limb; nor was there, when the affection was confined to a single limb, any general increase in size on the affected side more than could be accounted for by the enlargement of individual and manifest veins. As a rule, the varicosity in the leg was in excess of that in the thigh, but in about 20 per cent of the cases the tortuosity or size of the veins in the thigh exceeded that in the leg.

In a certain number of instances the disease was most evident as a large mass of veins occupying the lowest third of the thigh, above and below which the saphena was only slightly involved, and in the majority of these there was a large globular varix at the saphenous opening.

In most of the cases of varix limited to the saphena occurring in men, the affection was first noticed at the ankle, and gradually extended upward. In fourteen instances, however, the fulness, when first observed, involved the whole vein; four of these were on the left side and ten on the right, these latter having become manifest early in life—a point of interest in relation to the possible hereditary nature of some of these varicosities.

As to the point at which the disease was first observed in the women no definite evidence was forthcoming, in consequence of difficulties due to pregnancy, excessive constipation, and menstrual troubles.

Mass of Varicose Veins at Inner Side of Thigh, Overlying but not Involving the Saphena.—Only six cases were seen in which this condition could be with certainty diagnosed; two

of these were in females. In all the cases the varicose mass could be entirely separated from the saphena, which, apparently healthy, ran beneath. The veins of the leg in the men were not visibly affected, but in the women, both of whom had borne children, there was some fulness below the knee. In two instances, both males, there was a globular varix at the saphenous opening.

It is possible that some of the cases previously mentioned, in which a large mass of veins existed in the lower part of the thigh, the saphena being almost healthy above and below, may belong to this class; indeed, I have recently dissected out a mass of this kind from a patient in the Belgrave Ward, which proved to be quite unconnected with the saphena. (See Plate, Figs. 1 and 2.)

Varicosity Limited to Veins of Ankle and Foot.—The seven instances of this condition occurred in females, and were confined to the right limb. In all, with the exception of two cases which followed upon fracture of the leg, the disease was noticed at an early age.

Isolated Mass of Varicose Veins over Calf of Leg.—This appears to be the rarest form of varicosity met with in the lower limbs. Only four cases occurred; all were in males—one in the left leg, three in the right; and there was no evidence of other varicosity. In all, the external saphena seemed to be involved.

I have recently had another case under my care in the hospital and on dissecting out the mass, which consisted of fifteen inches of greatly dilated veins, the efferent vessel was found to be the proximal part of the external saphena, while the afferent vessels were the distal part of the same vein, and two small veins which came through the fascia, one on each side of the upper end of the tendo Achillis. (See Plate, Figs. 5 and 6.)

Circumscribed Varix at the Saphenous Opening.—This condition occurred much more frequently in men than in women.

The seven instances in which the affection existed at both saphenous openings were limited to males with varicosity in the two limbs. Five of these were in cases of general varicosity of the legs, and more or less of the thighs, the remaining two being in cases of varicosity confined, apparently, to the two internal saphenas and their immediate branches.

In varicosity of both limbs with varix at the saphenous opening on one side only, the percentage occurring in males largely exceeded that in the females, on the left side; but on the right, the percentage among the women was rather greater than in the men. In the males with general varicosity of one limb only, the varix at the saphenous opening was present on the left side nearly thrice as often as on the right; whereas with the females the percentage was larger on the right. In all the patients presenting this condition the saphena itself was large or varicose, although in a few cases to a hardly perceptible degree.

I have recently seen a case in which there was a globular varix in Scarpa's triangle of the kind under discussion, without the least sign of varicosity of the saphena or any other vein. The patient had long ago been told it was a hernia, and indeed came to me with a view to submitting himself to the operation for the radical cure of that affection. The condition had existed from childhood.

Probable Causes.

Foremost among the causes commonly spoken of as producing varicose veins are constipation and pregnancy; the latter, acting either alone or, as often happens, in combination with the former, accounting almost entirely for the large excess of cases of general varicosity seen in females.

It is, however, important to realize that most of the so-called originating causes are, in reality, only causes which determine either the increase of the disease, or of certain symptoms connected with it, and are not necessarily associated in any way with its real origin.

Certain forms of hepatic and cardiac disease have from time to time been credited with a large share in the production of varicose veins in the lower limbs; and it is possible that in a very slight degree this may be correct, although the evidence upon which the conclusion rests is somewhat vague.

At the same time there is no doubt whatever that *morbus cordis* may be, and often is, a fruitful source of complications, to be referred to later, in patients whose veins are already varicose.

The only obvious originating causes of general varicosity in the lower limbs appear to be the following:

Obliteration of a large vein following upon the formation of thrombus, such as may occur in typhoid fever, certain forms of gout and other affections, or injury, of which five instances are noted in my tables, all being cases of fracture (one of the pelvis, two of the thigh, two of the leg).

Local pressure, such as that produced by tumors or fæcal accumulations, the latter being limited for all practical purposes to the left limb, and being, in my mind, of doubtful value as a cause.

Pressure of a long column of blood may, in the case of the internal saphena, placed as it is outside all accessory muscular support, no doubt account for the fulness and slight elongation, hardly amounting to real varicosity, which is commonly seen in that vein among subjects who follow occupations necessitating long standing; but in the exaggerated varix, which is not uncommon in this vessel, some other cause must exist, since it occurs indifferently in all classes of individuals, and quite independently, at times, of continuous standing.

It is, however, clear that in a large number of cases, especially those affecting the right limb, varix occurs without any such obvious cause. The question therefore arises as to whether a limb, in which the venous apparatus was originally perfect, becomes varicose till late in life, excepting as the result of some obvious cause of the kind alluded to.

Personally, the more I see of this affection the more I incline to the belief that a large proportion of these varicosities originate in congenital defects in the venous apparatus, which in some patients are distinctly hereditary. If this be the case, it would naturally be more easily demonstrated in instances of varicosity affecting the right side, which for all ordinary purposes may be assumed to be free from the influence of that doubtful originating cause, a loaded colon.

A reference to the tables will show that the percentage of cases on the right side with a distinct hereditary history is about equal in the two sexes—viz., rather more than fifty per cent. That the disease must—sometimes at all events—depend upon peculiarity in the veins themselves seems probable from the way in which severe varicosity develops in men who are robust in all other respects, and who do not suffer from constipation or other conditions in the least degree likely to produce the affection. Further, the disease affects all classes,

rich and poor, with curious impartiality; it occurs in the active and sedentary, in the weak and strong, in the short as well as the tall, and is to a great extent independent of occupation.

Again, the difference in the behavior of the veins of the two limbs when these are subjected to precisely the same conditions is sometimes remarkable, for it is occasionally found that under these circumstances varicosity will develop in one limb only, leaving the other unaffected, either entirely or until a much later period in life. The best illustration of this is seen in varicosity arising apparently from the local constriction produced by the use of garters, in which the disease may be confined to one side, perhaps the right.

I hardly see how the occurrence of such cases can be explained otherwise than by assuming that some inherent defect or abnormality exists in the venous apparatus.

I have been unable to connect the occupation of the patient in any definite degree with the actual production of this disease, excepting in certain cases, already alluded to, in which the internal saphena was involved; but some occupations which entail long standing, exposure to great heat, or repeated slight injury undoubtedly tend to increase the varicosity or cause complications, such as thrombus, ulcer, eczema, and œdema.

The influence of stature is interesting but vague. As a rule the veins seem to be affected in nearly equal proportions in the tall and short, though general varicosity, without much affection of the saphena or other purely subcutaneous veins, is more commonly seen in short than tall subjects. On the other hand, cases in which the disease is more or less limited to the saphena are not often seen in any but tall patients.

The results of local constrictions from external sources (*e.g.* the use of garters) have been referred to incidentally; and although I believe more importance than necessary has been attributed to these local constrictions in the production of the disease, they have, without doubt, a tendency to cause a rapid increase in the varicosity, and may give rise to painful complications. It is, of course, difficult to obtain precise information from females as to these probable causes, in consequence of the constant association of pregnancy and constipation, the former predominating when the disease affects both limbs, and constipation when the left side only is involved.

Time of the Appearance of the Disease.

Under this head was noted the time at which the patient's attention was first called to the affection. This has, of course, but little real bearing upon the date of the actual commencement of the disease, for in some of the cases it was noticed accidentally after trifling injury, and in many others it was obvious that the varicosity had existed long before it was seen by the patient.

The males afforded no instance in which both limbs were affected before the age of twenty-one, but between twenty-one and twenty-five no less than forty of the total number of ninety-two cases first attracted attention. In cases confined to the left limb, two of the thirteen examples noticed before the age of twenty-five had existed from "childhood," and three gave trouble before the age of twenty-one. In varicosity of the right side only, fifty per cent of the cases were noticed prior to the age of twenty-one, five of these having been observed soon after puberty. It is noteworthy that no case in which both sides were involved commenced after forty.

The females presented but few cases affecting both limbs before the age of twenty-five, but between twenty-five and forty an enormous increase is shown, which is easily accounted for by the effect of child-bearing. Of the cases limited to the left side, about thirty-five per cent were noticed before the age of twenty-five; of these three caused trouble before the age of twenty-one and after pregnancy. When the right limb only was involved, more than half the cases were noticed before twenty-five (twelve of which attracted attention prior to the age of twenty-one). The only examples of varicosity commencing after forty were the fourteen cases in which the right limb was affected.

Complications.

Under this head are noted conditions which were held to be fairly dependent for their existence or continuance upon the varicose veins, or which owed their origin to the same cause.

Hæmorrhoids.—As may be expected, these occurred more commonly in patients in whom both limbs were varicose (*i.e.*, in cases in which general varicosity was most advanced), and

under these circumstances were more frequently seen in women than in men. In varicose veins of the left limb this complication was again found more frequently in females than in males, in whom it was only present in association with excessive constipation. It is remarkable that no instance of piles was seen in males when the varicosity was confined to the right limb, only three cases being seen in females. In two of these the piles followed upon lingering parturition long after the original varicose veins had been noticed, and in the third they seemed to have been caused by uterine displacement in a virgin.

Varicocele.—In the majority of the examples of this complication its existence was unsuspected by the patients. In the cases occurring with varicosity of both limbs, two were double, and with one of these there existed also severe piles; the remaining seven varicoceles were on the left side. Those connected with varicose veins in the left limb only were not observed by the patients, excepting in one case where there was neuralgia testis. The two instances associated with varicosity in the right limb only, were manifest to the patients, but caused no trouble.

With reference to this complication, one point of interest was elicited, viz., that, with one exception, all the cases of varicose veins in which varicocele was present were noticed at a comparatively early age.

Varix over Pubes.—The two examples of this condition occurred in males; one followed fracture of the pelvis, and the other was a sequel to typhoid fever; in the former there was general varicosity of the left limb, and in the latter both limbs were involved.

Varicose Veins in Labia.—The two instances of this affection were seen in cases of the most exaggerated varicosity of the lower limbs I have ever met with. The patients were very fat multiparæ; in one attention was called to the complication by the formation of an abscess in the labium, presumably due to the breaking down of a thrombus. There were extensive piles in both cases.

Ulcer and Eczema.—Under these heads there is little that requires comment. A large number of the ulcers, which presented every variety, were due to slight injuries to parts already eczematous; a few were the result of the bursting of

varicose veins; and a small number were apparently spontaneous, but in reality resulted from the breaking down of thrombi, this cause being most commonly seen in men. A considerable proportion of the cases of eczema were associated with multiple hemorrhages (purpura).

It is curious that on the female side no instance of simple eczema was met with in cases of varicosity confined to the right limb.

Persistent ("solid") Œdema.—These cases were all of long standing, and were limited in each instance to the lower two thirds of the leg. The parts were greatly hypertrophied, of stony hardness, and insensitive. In all there were scars of old ulcers, and in five a distinct history of syphilis was present.

Recent Thrombus.—In connection with this complication occurred some of the greatest differences between the men and women; for while in the former there was no example of recent thrombus in varicosity affecting both limbs, there were twenty-three cases in the total number of 163 varicosities of both limbs in females. In the majority of these the thrombus was confined to one side, principally the left, and seemed to be associated with either the puerperal state or menstrual irregularities.

Thrombus in cases of varicose veins limited to the left limb was more frequent in men than women. Three of the instances seen in the males were due to injury, four were examples of passive coagulation (gouty ?) in old age, and two occurred spontaneously in young subjects. For the seven cases on the women's side no definite cause could be found.

In varix affecting the right limb only, no recent thrombus was noticed in the females; but in the males there were seventeen cases, all due to slight injuries, excepting three which were spontaneous in origin.

Although it has become a rather common custom to speak of thrombi which form spontaneously in many of these cases of varicose disease as being of "gouty" origin, they are, I believe, far more often due to extreme feebleness of the circulation connected with some form of heart affection, either functional or organic.

I have under my care at the present time a lady, beyond middle-age, who is afflicted with general varicosity of both lower limbs, and whose heart's action is so weak that if the

limbs are allowed to remain in the dependent position for any considerable time one or more thrombi invariably form.

So long as she remains in the recumbent posture, or is careful to keep the legs well elevated, no formation of thrombus occurs. She has been assured that the blocks are due to gout, although her personal history is free from evidence of its existence, and she does not come of a gouty stock.

There is in my mind not the least doubt that the unfortunate tendency to coagulation in this case is entirely the result of the defective cardiac action.

There is, however, a peculiar variety of thrombus which seems to occur only in those persons who are of a pronounced gouty habit; such, at least, is my experience.

The peculiarity referred to exists in the fact that the blocks form only when the patient happens to be confined to bed or laid up suddenly in any way after having been previously in apparent good health. So long as the customary exercise, etc., is uninterrupted, the tendency to coagulation appears to be in abeyance. All the cases of this kind which have come under my immediate notice have been in males. I have recently had a good example under my care in the person of a gentleman whom it was necessary to confine to bed for a few days after a small operation, he having before been in good general health, but, at the same time, greatly disposed to gout.

Five days after taking to his bed he awoke in the middle of the night with a feeling of pain on the inner side of the knee, and found in that situation a hard, tender lump, which proved to be a thrombus in the internal saphena vein.

Upon inquiry it was ascertained that a similar block had formed, in the same way, higher up in the thigh, some years previously, while he was temporarily laid up.

Cases like this, although not common, are certainly not very rare, and their occurrence is well worth bearing in mind, as it shows the necessity for caution in confining a certain class of patients to bed for a longer time than is absolutely necessary.

Old Thrombus.—Evidence of this condition existed in many cases in both sexes, and varied from the minute phlebolith to a mass of hard, tortuous, insensitive blocks several inches long. The largest seen in the cases tabulated was in a man in whom the whole of the saphena below the right knee was completely

occluded. This block had been years in forming; it began above the ankle, and gradually increased by the occasional deposit of new coagulum at the proximal end of the old thrombus, which caused pain and tenderness for periods varying from three days to a fortnight, and then gradually became insensitive, hard, and permanent. I have recently had under my care in the hospital a still more exaggerated instance of this "creeping" form of thrombus, which extended from the ankle to the middle of the thigh, and was still growing.

Valgus.—The occasional association of valgus with general varicosity of the veins of the lower limbs is of certain interest in connection with the possible hereditary origin of some of these cases.

In the five instances noted in my tables, the valgus had existed from childhood, and the varicose veins had become troublesome very early in both limbs simultaneously. The patients were all males, and there was in each case a history of valgus and varix on the part of the father.

Uncomplicated Cases.

At first it may appear singular that so small a proportion of the total number of cases should have been free from complication. In reality, however, this is easily explained by the fact that my tables include only hospital patients, who, as a rule, do not apply for treatment until the symptoms from which they suffer are sufficient to interfere with their occupations, the result, therefore, being that treatment is generally sought, not on account of the abnormal veins themselves, but for some complication immediately connected with them.

A careful consideration of the cases tabulated and many others seen in hospital and private practice, in addition to dissections, into the details of which I need not enter now, seems to me to render the following conclusions reasonable:

That, discounting the influence of pregnancy, the tendency to the development of varicose veins in the lower limbs is greater in men than in women.

That the disease for the most part arises from congenital and often hereditary defect or other abnormality in the venous apparatus, there being no evidence to show that a vein originally perfect (excepting, perhaps, the long saphena) ever

becomes varicose before middle age, in the absence of such obvious causes as those previously referred to.

That two distinct classes of the disease exist: one beginning in the deep veins, and subsequently involving the superficial; the other affecting the superficial veins only, usually the long saphena and its radicles, and being confined almost entirely to tall subjects.

That the large mass of varicose veins in the lower part of the thigh, often associated with a globular varix at the saphenous opening, may in many cases, although apparently connected with the long saphena, be independent of it, being fed for the most part by veins coming through the fascia from the deeper parts of the limb—a point worthy of consideration in the treatment of these cases, as I propose to show in a future chapter.

CHAPTER II.

I PROPOSE to devote the present chapter to the consideration of the following points of interest: the diagnosis of varicose veins in the incipient stage of the affection, and the symptoms, mode of formation, and significance of circumscribed varix at the saphenous opening.

THE DIAGNOSIS OF VARICOSE VEINS IN THE INCIPIENT STAGE.

In the majority of cases of varicosity met with in hospital practice, the symptoms are so obvious that at first sight it seems superfluous to waste time upon their consideration; but if you will follow me I think that I can demonstrate that the disease is not always so easy of recognition as is commonly imagined, for it is necessary to bear in mind that the swollen, pouched, and tortuous veins coursing over a limb, which may itself be greatly enlarged, are seen only in advanced disease, and are quite absent in the very early stages; which accounts, I presume, for the fact that the onset of the affection is sometimes entirely overlooked. Although, as I have said, our hospital practice does not afford many cases of incipient varicosity, there is no doubt that a number of patients apply for treatment on that account, but the disease of which they complain seems so trivial that they have to give place to others who are more urgently ill.

I have, however, seen among the out-patients the twenty-three cases alluded to in the first chapter, belonging, I believe, to this class, in which no obvious varicose veins were present —a fact which probably explained the reason for these patients having been previously treated for affections of a totally different nature.

In private practice these early cases are less rarely seen than in hospital work, for the reason just referred to, and in

consequence of the way in which hospital patients delay application for treatment until diseases are as a rule advanced. In uncomplicated varicosity it is essential, for the purposes of treatment, to determine as to whether the disease affects for the most part the deep or the superficial veins. In the greater number of advanced cases the deep and superficial vessels are probably involved to about the same degree; but this is by no means always so. Let us, therefore, take an instance in which varicosity is only obvious in the superficial veins (*e.g.*, the saphena with its immediate branches), and consider how the existence of the disease in the deeper vessels may be determined.

The condition most commonly pointing to implication of the deep parts is a general enlargement of the limb, limited in the early stages to the leg, which is more than can be explained by the mere existence of the large superficial veins. The increase in size may or may not be associated with slight œdema, in the absence of which the enlargement presents itself as a kind of flabby fulness, excepting after the standing position has been assumed for some time, when the flabbiness gives place to a feeling of unnatural tension. In many cases the increase is so slight as to be hardly appreciable, and I have known the peculiar hardness of the calf of the leg after standing mistaken for an especially fine tone of muscles, in an instance in which a comparison of the two limbs had manifestly not been made.

Further evidence is afforded by the appearance of a collection of small varicose veins below and behind the inner ankle, or, as is more common, over the outer side of the popliteal region or fibular side of the leg at its upper part—conditions which merely indicate that varicosity involving the deeper parts is prone to extend to the superficial veins in one or all of these situations.

Although this extension to the superficial vessels is not necessarily connected with any general fulness of the limbs, sufficient to attract attention, it is, I believe, in most instances preceded by pain of a peculiar kind, to which I particularly wish presently to call attention, as it differs from that usually described as depending upon varicose veins.

In the thigh the evidence of implication of the deeper parts is less pronounced in the very early stages of the disease, but

there are two symptoms which may be considered, excepting in very rare instances, pathognomonic, viz., a varix at the saphenous opening, and a collection of fine radiating veins appearing over the inner aspect of the limb, not far above the knee. There may also be some general increase in size, but in the thigh it is so difficult to estimate a very slight increase that it is a symptom of little value, especially on the right side.

The commonest symptoms produced by uncomplicated varicosity are pain and œdema, with a feeling of weight and general weakness in the limb. In ordinary and fairly advanced cases, the patient rises in the morning almost if not entirely, free from discomfort; as the day advances pain comes on, and gradually increases toward night, until the legs are raised or the recumbent position is assumed. Should œdema coexist, it will, like the pain, gradually increase during the day, and is often toward evening accompanied by intense itching, which is generally at its worst shortly after the patient lies down, *i.e.*, just as the œdema commences to subside. Although pain and œdema are very commonly associated in these cases, there is not necessarily any definite relation between the two symptoms. At times the occurrence of œdema seems to be a relief to the acuteness of pain, so long as it does not assume a formidable character and give rise to painful complications like eczema or ulcer. In passing, it may be worth mentioning that although ulcers—which are, I need hardly say, common in cases of varicose veins—as a rule cause great discomfort, instances are occasionally met with in which they are a distinct relief to pain. I have seen several cases myself in which little or no pain was felt while the sores were open, but in which upon the healing of the ulcers great pain followed. In each of these cases there was œdema, and the discharge from the sore, which obviously relieved tension by acting as a drain, was profuse and watery.

The pain occurring in commencing varicosity of the deep veins before any perceptible change has taken place in the superficial vessels is peculiar in its nature and behavior. It is pathognomonic, I believe, of incipient varicosity, but I can find no reference to it in this relation in the ordinary works on surgery. Although I cannot say that it is to be met with in all cases, in many it certainly occurs, its real cause being

often undetected, and the patients treated, as a rule, for gout, rheumatism, or valgus.

The pain under discussion commences in one or both legs almost directly after the patient rises in the morning, or at any other time upon assuming the erect position after the recumbent posture has been maintained for a considerable period; it is very acute, and rather "crampy" in character; the limb is neither stiff nor markedly tender. After an hour or so the acute discomfort subsides, the "crampy" pain disappearing first, leaving the patient apparently well till later in the day, when in the more advanced cases aching pain follows, with or without œdema. The following are good cases to illustrate this symptom:

Case I.—A strong, healthy-looking girl, twenty-five years old, applied for treatment among my out-patients, for pain in the right leg, which came on daily directly after rising in the morning, was severe and "crampy" for an hour or rather more, and then by degrees entirely left her. Toward evening a little aching pain followed about the lower part of the leg, and occasionally she noticed some swelling around the ankle.

She had been treated, she said, elsewhere for hysteria and flat-foot. The latter she certainly had not, and of the former the only evidence was slight hyperæsthesia of the leg, which is not at all uncommon in early cases of varicosity. The leg itself was quite natural in appearance, but was very slightly larger in circumference than the opposite limb, the increase being more than could be explained by the fact of the affected side being the right. I at once concluded the case was one of commencing varicosity in the deeper parts, and ordered an elastic stocking, which entirely relieved the morning pain.

Six months afterward she again became an out-patient, and then had distinctly marked varicose veins in the right upper fibular region. She had worn out her elastic stocking, but thought it hardly necessary to get another, as she suffered then from no morning pain, but later in the day there was some aching and more œdema than when she was at the hospital previously.

Case II.—A gentleman consulted me on account of pain in the right leg, which had been previously diagnosed as gout and rheumatism.

This pain commenced in the morning precisely as in the preceding case, and subsided in the same way, leaving him comfortable till the afternoon or evening, when aching pain of an entirely different kind supervened, being sometimes accompanied by considerable swelling "just above his boot," which caused so much distress that occasionally when out shooting he had to return home early. By the following morning the swelling was usually gone, and the same daily sequence of symptoms recurred. The leg itself was entirely free from any indication of superficial varicose veins, but was little enlarged, and there was slight œdema about the ankle. I told him the cause of his suffering was commencing varicosity, and ordered an elastic support for the limb.

A year later I saw him again, when there were well-marked varicose veins coursing over the popliteal region, and below and behind the inner malleolus was a collection of small tortuous veins, forming quite a pad in that situation.

It is, I think, evident that this particular pain is, as I have said, quite distinct from that which is usually described as produced by varicose veins, both in the manner of its onset and in its general character; indeed, I have heard it stated by a surgeon of repute that the very peculiarities of the pain negative the possibility of its being due to varicosity.

The explanation is, however, it seems to me, simple, if the difference in the condition of the veins at the commencement of the change toward varix, and in advanced cases of the disease, be considered. In the incipient stage of the affection the veins are to some extent resilient, their coats perhaps being at the same time rather hypertrophied, especially in the muscular layer. In the advanced disease, the veins, pouched and tortuous, are insensitive, flabby, or rigid tubes, without resilience at all. The results of these conditions are the following: In the incipient cases, the veins, when subjected to more than the normal pressure, immediately resist the unnatural tension, being assisted in this by the contractions excited in the fibres of the muscles around them—at all events, in the case of the intra-muscular veins. Thus is produced the acute, somewhat crampy pain which, under these circumstances, naturally occurs at the time of rapid increase in the blood-pressure in the veins, which, by reason of their resiliency, re-

cover something like their previous calibres during the time the abnormal tension is relieved by the recumbent position of the patient.

The peculiar subsidence of this pain is accounted for, in my judgment, by the active resistance in the vessel being gradually overcome by the continual, uniform, increased blood-pressure, so that no noticeable further discomfort occurs until the distention is sufficient to cause the ordinary pain, with or without œdema, met with in advanced varicosity.

The daily repetition of these conditions results in the gradual advance of the disease, the typical morning pain ultimately entirely disappearing, either in consequence of the veins becoming over-dilated and insensitive, or, as more commonly happens, from the extension of the disease, in the directions previously indicated, to the superficial veins, which, being independent of muscular or other extraneous support, dilate rapidly, and so afford a speedy relief to the tension in the deeper parts of the limb.

Varicosity of the superficial veins occurring without general enlargement of the limb or antecedent pain is probably due either to the disease commencing in these vessels themselves, or to their implication sufficiently early to prevent, by the comparative ease and rapidity with which they yield to pressure, any considerable tension in the subjacent parts.

Varix at the Saphenous Opening; its Symptoms, Mode of Formation, and Significance, particularly with Reference to the Development of Varicosity in the Limb below.

The appearance of this condition may be one of the earliest indications before middle age of commencing varicosity, or it may only become manifest late in life, after the whole limb has become varicose.

The affection shows itself in the situation of the saphenous opening to the inner side of the femoral artery, either as a somewhat flattened but distinct swelling, or as a prominent globular tumor, the surface of which is often of a bluish tint. In either case, the swelling, which is softish, fluctuating, and "compressible" (not "reducible," as it is usually described to be), presents distinct impulse on coughing, and is most tense

and prominent in the erect position, becoming softer, less marked, or even entirely disappearing when the patient lies down. The tension produced by the standing posture is further increased by abduction or outward rotation of the thigh, which may cause discomfort, or possibly acute pain, especially if the tumor is of the projecting kind. Under ordinary circumstances, no inconvenience is caused by the affection in many cases, the patient being unaware of its existence until attention has been accidentally called to it. Sometimes, however, there is a distressing feeling of weakness or pain in the groin, the latter varying in degree with the position of the limb.

Varicosity of the long saphena to a greater or less degree almost always coexists, and there may or may not be other manifest varicose veins in the thigh or leg, or in both. The only case I have ever seen in which varix at the saphenous opening was entirely unassociated with other evidence of varicosity is the rare instance mentioned in the preceding chapter.

Although the two kinds of tumor which I have described as occurring in the form of varix now under discussion are generally looked upon merely as different stages of the same condition, I have no doubt they represent two distinct varieties of the affection, differing in seat of origin and subsequent relations.

The first kind, showing itself as a flattened swelling, and retaining almost the same shape, however large it may become, is situated for the most part beneath the cribriform and deep fascia, and may therefore be called "subfascial."

The second kind, globular and projecting, is principally superficial to these fasciæ both in origin and situation, and may be termed "subcutaneous." Of the first variety I once dissected a perfect specimen; the dilatation affected the long saphena above its proximal valve, implicating at the same time the femoral vein to a large extent; the whole varix was beneath the cribriform and deep fascia, excepting just around the point of entrance of the saphena. Of the second kind I have never dissected an example of so large a size; but the curious prominence and shape of the tumor, even when small, and the ease with which the color of the blood in the varix is perceptible through the skin, leave little doubt as to its differing in some respects from the subfascial variety. Moreover, it is easy to produce the two distinct conditions artificially by

a simple experiment on veins *in situ* or recently removed from the cadaver.

I do not, of course, mean to infer that in a large subfascial varix no part of the tumor ever becomes subcutaneous, nor do I mean that no part of a well-marked sample of the subcutaneous kind ever extends beneath the fasciæ. Originally, however, they are distinct conditions, and to whatever size they may grow, the two varieties will be anatomically recognizable, the one being for the most part deep, the other principally superficial.

Given the necessary state of blood-pressure, the tendency to the formation of saphenous varix appears to depend upon the disposition of certain vein valves in the upper part of the thigh.

It is therefore necessary to consider with some care, first, the most perfect arrangements of these valves found in the human subject, and, secondly, the commonest departures from the perfect types, with a view to the proper estimation of the possible effect of these deviations in the production of varicosity.

The following details are derived from the examination of a considerable number of subjects, and will, I believe, be found to be generally correct, although they may in some respects differ from the descriptions given in certain of the anatomical text-books, in which the situations of the vein valves are but vaguely indicated.

Arrangements which may be Considered Perfect.—(1) A pair of valves a little above or below the level of Poupart's ligament, which may be conveniently called "ilio-femoral," as they may be situated indifferently in the iliac or common femoral vein; a second pair, "inferior common femoral," placed just above the termination of the profunda vein; and two pairs in the upper end of the long saphena, one of these being placed, as a rule, at the orifice of this vein, on a level with, or more commonly beneath, the cribriform fascia; the other lying superficial to this fascia, half an inch or more farther down the trunk of the vessel,—called respectively the proximal and distal "upper saphenal valves."

(2) A similar arrangement to the foregoing, but with absence of the inferior common femoral valve, which is replaced by two pairs of valves, one at the orifice of the profunda vein, the other at the point of junction of the superficial femoral

vein with the common femoral. This, although a presumably perfect arrangement, is very seldom met with, the profunda valve being apparently the rarest in the thigh.

(3) Two pairs of valves in the external iliac vein; one, the ilio-femoral, close to Poupart's ligament; the other, the "superior external iliac," situated at any point between the ilio-femoral valve and the entrance of the internal iliac vein, the saphenal valves being double, as in 1 and 2, but with entire absence of the inferior common femoral, profunda, and superficial femoral valves.

Deviations from the Perfect Types, with Suggestions as to their Possible Effects.—Absence of Ilio-femoral Valve. In this condition any increase of blood-pressure from above will result in abnormal strain on the inferior common femoral and the proximal saphenal valves. As the former is much the stronger, and very rarely inadequate, there will be a tendency to inadequacy on the part of the proximal saphenal valve, or to dilatation of the saphena and immediate part of the common femoral vein, resulting in a varix at the saphenous opening of the subfascial kind. Should the proximal saphenal valve be absent, or, as is rather common, inadequate, the distal one existing, and being at the same time adequate, the tendency will be to the development of a prominent globular dilatation of the saphena between its distal valve and the femoral vein, which will also be involved to some extent if the dilatation becomes excessive.

The varix thus formed at the saphenous opening is of the subcutaneous variety, the main part of the tumor being superficial to the cribriform fascia both in its origin and relations. If, under similar conditions, both of these upper saphenal valves be absent or insufficient, the tendency then is to more or less general varicosity of the saphena vein.

As having some bearing upon the variety of saphenous varix developed in this class of deviation, it is worth mentioning that absence of the ilio-femoral valve is very commonly associated with absence or inadequacy of the proximal saphenal valve.

Absence of the Ilio-femoral and Inferior Common Femoral Valves; one or both Saphenal Valves being present, and a strong Pair of Valves (Superior Superficial Femoral) existing at the Termination of the Superficial

Femoral Vein, the Orifice of the Profunda being without Valves.—In this case it seems reasonable to assume that the tendency would be to varicosity of the branches of the profunda vein, which may, if the proximal saphenal valve be wanting or inadequate, be associated with globular varix at the saphenous opening, possibly accounting for the occurrence of this kind of saphenal varix with a varicose mass at the inner side of the thigh above the knee, which may be unconnected with the long saphena vein itself, the efferent vessels passing into the deep parts, and being in reality connected with the lowest branches of the profunda. Should both upper saphenal valves be wanting or inadequate, some general varicosity of the saphena may replace the local varix at the saphenous opening.

The existence of this arrangement may also possibly explain certain cases of varicosity in which the disease appears to affect the lower parts of the thigh more than the leg. The only case of this kind that I have been able to dissect I found by chance in the post-mortem room. The superior superficial femoral valve was very strong, and appeared unusually large; the profunda was valveless, its branches being very large, and in some instances pouched; from the lower branches there could be traced through the fascia several vessels which passed into a collection of varicose veins above the knee, which also communicated by two very small vessels with the saphena, which was but slightly dilated. It was, however, plain that the varicosity was mainly connected with the profunda.

As a natural corollary of the arrangement just described, it would seem probable that a deviation would be found in which a strong valve existed at the termination of the profunda, the superior superficial femoral valve being absent. I have, however, never seen such a profunda valve, excepting in one case, where the superficial femoral valve was also present (perfect type, 2).

Entire Absence of all Valves in Common Femoral, Superficial Femoral, and Profunda Veins, with Absence or Inadequacy of External Iliac Valves; the Saphenal Valves being Present, and perhaps Numerous.—In one case there were no less than seven pairs of valves in the saphena above the knee. Here, in the development of varicosity the tendency would be to its early occurrence throughout the limb, either

simultaneously in the thigh and leg, or more probably first in the leg and subsequently in the thigh and saphena, the latter becoming involved throughout, as the disease in this case would be of the ascending kind.

I need hardly say that the deviations which have been now described are not intended to include all the various and complicated abnormalities sometimes met with in the arrangement, number, or adequacy of the vein valves, but are merely selected as they afford examples of departures from the perfect types which seem to be capable of influencing to some degree the development of certain varieties of varicosity.

Although I have no doubt that, under certain circumstances, the arrangement and adequacy of these valves have an important relation to the production of varicose veins in a fair proportion of cases, it is perfectly certain that a large number of instances of varicosity, so called, are seen in which the valves have nothing whatever to do with the occurrence of the disease.

This remark especially applies to those cases of general varicosity limited to one limb, in which the whole of the veins are extensively affected, the limb itself being greatly enlarged, but in which there is neither œdema nor inconvenience, unless some accidental cause gives rise to complication.

In these instances it seems to me that the affection is a purely congenital one, the patients having, in fact, been born with veins abnormally large, and perhaps tortuous, but otherwise natural, the valves being strong and not inadequate.

In some cases, again, the disease may approach a condition which is almost nævoid. In either case the direct congenital origin of the affection is rather indicated by the occurrence of evidence of other vascular abnormities—*e.g.*, the scars of old nævi—in a considerable number of patients who are the subjects of this class of varicosity. Further corroborative evidence of this view is afforded by the following case, which Dr. Penrose kindly dissected for me.

A man, forty years of age, who died in the hospital of bronchitis, was seen, after death, to have great general enlargement of the right lower extremity, the opposite limb being natural. The skin of the affected side was perfectly healthy and natural, showing none of the thinning which is common in cases of acquired varicosity.

On examination the whole limb was found full of large and tortuous veins, the common femoral being nearly double the size of that in the left limb, the veins of which were perfectly normal in every respect throughout. The saphena was as large as the femoral vein in the opposite thigh. The external and common iliac veins were much larger than normal, greatly exceeding in size those on the opposite side. The valves present on the affected side were ilio-femoral, inferior common femoral, and two upper saphenals. All of these were natural in appearance, but were very large, being of just such a size as the magnitude of the veins required. Moreover, they were so strong that distention of the veins by the injection of water almost to the point of bursting failed to make them inadequate. On the left side the ilio-femoral valve was absent, the inferior common femoral and upper saphenals being present and competent.

The case was typical of its kind, being finally rendered complete by the discovery of the scar of an old nævus on the left hip, and four congenital vascular tumors (nævi) in the liver.

I cannot imagine any condition, other than congenital asymmetry, which could produce such an increase in the size of the veins, particularly the common and external iliac, on one side of the body only, the valves being at the same time so perfect in relative size, strength, and adequacy.

CHAPTER III.

THE management of ordinary and uncomplicated cases of varicose disease of the lower limbs is, at first sight, so simple an affair, that it may perhaps seem hardly necessary that a lecture should be devoted to the details of this form of palliative treatment.

As a matter of experience, however, it is impossible to avoid feeling that, apparently trivial as these details may be, their real importance is not so fully recognized as they deserve by some of us.

This seems to be, in some part, due to an impression which not uncommonly exists in the minds of some, that in the majority of cases of the kind we are now discussing, very little more is necessary than to instruct the patient to obtain some form of support for the veins, and to see that the bowels act with regularity and freedom.

Now it is only fair to admit that these simple instructions do include, in the abstract, a great deal of what is necessary in these cases. At the same time there are various details connected with the treatment thus advised in the several classes of varicosity which will repay careful attention.

I therefore propose to devote the present lecture to their consideration.

As I have already pointed out, there is every reason to believe that all cases of general or local varix, with the exception of the instances mentioned in Chapter I. under the head of "Probable Causes," are the outcome of congenital and often hereditary imperfection in the venous apparatus.

It is also fairly certain that in many individuals the imperfection is so slight as to have no harmful result, unless some unusual strain be thrown upon the faulty mechanism.

The commonest causes of this extra strain are constipation and suppressed or deficient menstruation.

With regard to the latter of these two causes, it is remark-

able that the varicosity develops in a manner which is peculiar to this class of case, inasmuch as the affection shows itself, not by the appearance of an exaggerated, or even well-marked coarse varix of individual veins, but as a general tendency to varicosity throughout both thighs, in the small veins, as is shown by innumerable radiating and stellate vessels; the legs, at the same time, being somewhat œdematous and painful, but entirely free, excepting in the most advanced cases, from any apparent varicose disease.

In the more persistent of these cases the small veins are studded with minute cystic dilatations, not larger, as a rule, than a pin's head, which are liable to burst in or under the skin, giving rise to an appearance of multiple bruises. On the other hand, when the development of the affection is due to constipation it invariably appears first in the leg in the form of well-marked coarse varicosity of large veins, the thigh becoming involved subsequently if the disease progresses. In passing, it is well to bear in mind the fact that although the varicosity associated with constipation is as a rule more or less confined to the left limb, it may also, especially if the liver be sluggish, appear simultaneously in both lower extremities, in which case piles will be nearly always present.

This is a point of some importance in the matter of treatment, as many patients will be found to consider a slight inclination to hemorrhoids of much more consequence than the existence of varicose veins in the limbs below, however marked these may be.

There is no reason to doubt that in the earlier stages of these cases the discomfort, and indeed the manifest varicosity, may be removed by appropriate treatment of the amenorrhœa or constipation, or both of these conditions if, as often happens, they coexist.

It is also quite certain that any mechanical treatment will prove entirely futile so long as the bowels do not act with regularity and the menstrual function is in abeyance or deficient.

Cases in which relief of constipation is followed by a corresponding amount of comfort in the varicose limb are so common that it is unnecessary to narrate examples.

It may, however, be worth noting that, so far as drugs are concerned in the treatment of the inactive bowels, the relief afforded to the distended veins appears to be the greatest

when the milder purgatives are used rather than those of a very drastic kind.

In my own experience, the most effectual remedy has been some combination of sulphur, and next to that in efficacy has been cascara. The watery purges have afforded far less relief.

In cases where the liver is sluggish it is hardly necessary to say that an occasional mercurial dose is absolutely necessary.

The other class of case, viz., that in which the menstrual function is at fault, has not, so far as I know, received much attention. I therefore venture to narrate the two examples which follow in illustration:

Case I.—A girl twenty years old menstruated naturally at the age of fifteen, and continued to do so regularly till she was seventeen. Then, without any apparent reason, there was a cessation of the "period" altogether for five months. At the end of that time there was a very scanty flow. Subsequently menstruation never occurred more often than once in two months, and on such occasions to a very slight extent only.

For two years before coming under my observation she had continually suffered from pain in both lower limbs, and the veins in both thighs became full and painful; both legs becoming at the same time sensitive, tender, and toward evening swollen.

Upon examination the veins in the leg were not manifestly affected, but both thighs were full of collections of small radiating varicose veins. There was no large varix of any kind, but in many of the small dilated vessels were minute cystic dilatations, the walls of which were so thin that they seemed on the point of bursting.

A month after I first saw the patient a profuse menstrual flow occurred, presumably as the result of treatment at the hands of the obstetric physician. Subsequently, after two rather scanty "periods," she became quite regular.

The re-establishment of the regular menstrual habit was followed almost directly by cessation of pain in the limbs. In five or six months the full dilated veins in the thigh became so shrunken as to be unnoticeable, and all tendency to œdema in the legs disappeared.

Case II.—A married woman, thirty-two years old, who had

borne a child at the age of twenty-three without trouble of any kind, and without a suspicion of the development of varicose disease, became very irregular in her menstrual arrangements two years after her confinement. From being merely irregular, menstruation became by degrees very deficient, occurring only once in two or three months or thereabouts. On one occasion there was a complete cessation for four months, although there was no reason to suspect pregnancy.

For three years prior to my seeing her there had been pain in both lower limbs and slight swelling of both legs. For two years the veins in both thighs had been full and prominent in the form of collections of small dark-colored beaded vessels, over which, from time to time, appeared large bruises, which she had been told were due to the bursting of veins under the skin.

Upon examination, both legs were found to be slightly œdematous, painful, and tender, but showed no evidence of varicosity. Both thighs were covered with small dilated veins having many cystic dilatations upon them, similar to those described in the previous case.

On the inner side of the left thigh was a large blackish discoloration which appeared very probably to have been produced in the manner referred to, viz., by the bursting of one or more of the small cysts under the skin. On the opposite thigh, on its outer surface, was a mark like a fading bruise. The patient was absolutely certain that these bruises were not due to any external injury. When I first saw her she had not menstruated for more than two months, and was habitually somewhat constipated.

A simple laxative of sulphur and tartrate of potash served to relieve the constipation, and three months later a very profuse menstrual flow occurred, which the patient thought was a miscarriage, although it seemed highly improbable that such was the case.

Subsequently, without having been placed under any particular form of treatment, she became quite regular in every way.

Upon this the œdema subsided, the pain disappeared, and the dilated veins in the thighs became almost imperceptible, although their return to the normal condition was not so complete as in the first case.

It is an interesting fact, and one worthy of note, that patients who when quite young suffer from the kind of varicosity illustrated by these cases, are peculiarly prone to develop general varicose disease of a pronounced type, in both lower limbs during their first pregnancy, although the pregnancy may run a perfectly natural course and terminate in easy delivery.

As having some additional bearing upon this class of case, it may be well to mention that the first symptoms of discomfort in the slighter cases of varicose disease not infrequently show themselves at or about the termination of the climacteric period; a point to which I do not remember having seen allusion made elsewhere.

The first essential, then, in the commencement of the treatment of all cases of varicose disease of the lower limbs, is clearly to bring about a healthy condition of the secretions generally, to secure above all things regularity in the action of the bowels, and to correct as far as possible any defect, if it exists, in the menstrual function.

In a few of the cases seen when the disease is in its earliest stage, little further treatment may be called for, as the discomfort may be so effectually relieved, and indeed the manifest varicosity sometimes so far removed, that the patients are indisposed to be troubled with any appliance, or elaborate treatment, even if such be advised.

When the pain is excessive, and in all cases in which œdema is present, much good may, of course, be derived from complete rest in the recumbent position. An equal amount of benefit may be obtained, as a rule, excepting in the more advanced cases, by the elevation of the affected limb or limbs for short periods (say from half an hour to an hour in length) at definite intervals during the day.

This latter plan is most useful in slight cases, and especially as many patients will be found to consent to its adoption who would certainly decline to be entirely laid up.

If the relief thus obtained is insufficient or tardy, the disappearance of the œdema and pain may be greatly accelerated by the use of tepid douches over the elevated part, or by gentle friction with a well-oiled hand, in a direction passing from the toes toward the hip.

Four other points connected with the treatment of these

cases may be conveniently considered here, viz., exercise, massage, electricity, and baths.

Exercise.—In all cases of uncomplicated varicosity, active exercise, if discreetly used, is good, and the advice so commonly given to the more wealthy class of patient—that walking, riding, etc., should be given up and only carriage "exercise," so called, be taken—is distinctly bad, unless, of course, there is some co-existing disease or complication, such as morbus cordis, which necessitates such a course.

It is necessary to bear in mind that a very large number of these patients have been of active habits, and that the very act of laying them up tends directly to dyspepsia, sluggish liver, and constipation—conditions above all others to be avoided. Added to this is the fact that there can be no doubt that properly regulated exercise is distinctly beneficial to the varicose limbs.

If a good effect, however, is to result it is necessary—and this is the important point—that the exercise should constantly vary in character, the object being to bring about successive alterations in tension in the various groups of veins by the action of the surrounding muscles, and to avoid, as far as possible, throwing a continuous strain upon the same veins.

The reason that long-continued standing is so productive of discomfort in these cases is merely that persistent strain is thrown continuously upon the same vessels, in consequence of the limb remaining for a long time in a position which brings into continual action the same sets of muscles.

In a similar manner, active exercise of a monotonous kind, *e.g.*, excessive bicycle-riding, in which the same measured movements are repeated constantly without variation, throws precisely the same pressure again and again upon the same groups of veins, thus directly tending to increase the varicose condition in certain definite directions. On the other hand, a patient suffering from commencing or pronounced varicosity, who is constantly varying his manner of exercising—for intance, by alternately riding and walking, rather than habitually confining himself to one or other of those plans exclusively—or who is in the habit, let us say, of walking up and down hill rather than continually exercising on perfectly level ground, so distributes the strain successively over different groups of the defective veins that he is less liable to develop-

ment of local exaggerations of varix, and thus avoids one condition at all events which often leads to painful complications.

There is, so far as I can tell, no doubt whatever that moderate exercise, conducted upon the lines just indicated, is in a certain number of cases not only productive of comfort, but may even in some degree arrest the progress of the disease.

It is scarcely needful to say that the existence of complications such as ulcer, eczema, veins distended almost to bursting, etc., may altogether negative the possibility of exercise.

I have, however, seen, in more than one case, œdema decrease after a course of irregular exercise, which had been obstinate, and indeed intractable, while the patient was almost completely laid upon his back.

It is rather curious, at first sight, that the benefit derived from the practice now being discussed is, *cæteris paribus*, more marked in persons of somewhat advanced age than in young subjects.

I have recently had under my care a gentleman, about sixty years old, who for many years had suffered from varicose veins, with troublesome œdema. When I originally saw him he had been for months leading a life of absolute inaction, having been lying up for the greater part of each day, and taking only "carriage exercise." In spite of all kinds of supports, elastic and otherwise, any attempt at walking was followed by pain and increased swelling.

After a few weeks of perseverance, or short periods at a time, in irregular exercise, his condition was so much improved that the œdema, although to a slight extent present, was considerably less than before, and seldom caused pain unless the same form of exercise was persisted in for too long a time.

Although such a satisfactory result can hardly be expected in the majority of instances, it is certainly obtainable in some.

At the same time a mechanical support may be worn with advantage, and in most cases is essential.

Massage.—By this treatment the kind of exercise just alluded to can be obtained in a convenient manner, to the great advantage of patients who, by reason of their indolence, do not care to exert themselves, or who are precluded from so doing by the existence of local disease or general ill-health.

The benefit derivable from massage in many cases of vari-

cosity, whether early or advanced, which are associated with obstinate or painful œdema, is most marked.

It is often extremely useful when the pain is too great to allow of voluntary exercise being taken by any person who is not, from unavoidable circumstances, compelled to get about.

It is, however, not applicable in all cases, and must be practised with extreme care in cases where there is a tendency to eczema, or the distention of local collections of veins is very great, and especially in gouty subjects, in whom I have seen the massage process directly followed by thrombus. In anæmic subjects bruising is apt to follow the manipulation, however gentle these may be, under which circumstances the treatment must be at once suspended.

When thrombus of recent date is present it is hardly necessary to say that massage is entirely contra-indicated; indeed, I should not have mentioned this matter had there not come under my notice a case in which an accomplished "masseuse" had been manipulating a varicose limb in which the popliteal vein was blocked by a thrombus not more than two days old.

In the majority of cases complicated with ulcers, in all cases of abscess, and in limbs which are very tender, massage is obviously to be avoided.

Electricity.—This may, of course, be combined either with massage or with the treatment by voluntary exercise, or in the earliest stages of the disease be used independently. I cannot say that in my experience any very positive good has followed. In some patients, however, especially those who are at all neurotic, the crampy pain felt in the incipient stage of varicosity may be considerably modified by this treatment.

Baths.—In this connection the following case is of interest:

A middle-aged man of a distinctly gouty habit suffered from slight varicosity in the right leg, which gave very little inconvenience, excepting that it made the wearing of a bandage necessary. While in otherwise perfect health he went to his usual morning cold bath. Immediately upon putting his leg into the water he felt a sharp pain in the calf, and found a tender spot, which proved to be a thrombus.

The block disappeared in the ordinary way, and for a time the bath was used tepid instead of cold.

Ultimately it was determined to resume the use of the cold

bath, but at the first attempt a similar block occurred in another vein.

Although I have heard of only one other instance of exactly the same kind, it is quite clear that the formation of these thrombi was due to something more than mere coincidence. The cases speak for themselves, and are worthy of being borne in mind.

We now come to the consideration of the methods for affording relief to discomfort by means of mechanical support for the varicose limbs.

The mechanical appliances ordinarily used for this purpose may be roughly divided into two kinds: those which effect relief by localized pressure, and those which produce the same result by general compression of the affected part.

The use of appliances which exert local pressure only is confined to the following conditions: (1) Varix affecting either single veins or isolated collections of veins. (2) Cystic dilatations, whether individual and isolated, or whether occurring in general varix and producing the sole discomfort, as they sometimes do in such cases, by becoming over-distended. (3) A highly sensitive state of the limb, which may render the use of general pressure impracticable on account of the pain it produces. (4) In the event of the wearing of complete stockings or bandages being negatived by the occurrence of eczema, pruritus, or other complications directly connected with the use of those contrivances.

In connection with the cases which are suitable for treatment by local pressure, it is of great importance to understand that a cure may be effected in some, and great benefit produced in others, by operating in one of the several ways which will be described in the succeeding chapter.

Methods of Applying Local Pressure in Varix.

Strapping.—In many cases of discomfort due to distention of individual veins or circumscribed masses of varicosity, the most successful plan of obtaining relief is by the old-fashioned method of placing strapping across the affected vessels in the following manner: The strapping, which should be stout, and strongly adhesive, is cut in strips about half an inch wide, and

of a length equal to three-quarters the circumference of the limb at the point of application.

When the patient rises in the morning the requisite number of these strips are placed tightly across the axis of the vein, the centre of the strapping lying over the vessel; or in the case of a varicose mass the strapping is placed directly transverse to the longest diameter of the varix. Upon retiring to bed the strips are removed. In long veins, such as the saphena, and in elongated areas of varix, several strips are thus applied, at intervals, as a rule, of about one and a half or two inches.

In single cysts or small collections of varicose veins, a single piece of the strapping may be all that is necessary; in the former case, a small pad of felt or wash leather may be with advantage placed between the cyst and the strapping, so forming a kind of truss.

The objections to this method are the following: Although easily managed by any patient of ordinary intelligence, it is thought by many to be troublesome. The marks left on the skin give an appearance of uncleanliness. In irritable skins, eczema, or sometimes small pustules, may be produced along the track of the strapping. Wet as a rule so loosens the strips that they become useless.

At the same time, the actual relief obtainable by this plan is sometimes so marked that I have known a patient discard all other appliances in favor of it.

Trusses, Pads, etc.—In some instances of local varicosity situated about the leg and ankle, the discomfort caused by distention of the veins can be relieved by such gentle pressure that nothing more is necessary than a flat pad of wash leather, somewhat larger than the varix itself, which is worn inside a tightly fitting stocking, to which it may be fixed by a safety-pin. The stocking, it is hardly needful to say, must be suspended from the waist and not kept in position by means of garters.

If the pressure thus obtained is not sufficient, the most comfortable support is a slightly padded band of wash leather, which surrounds the affected part of the limb, and is made to lace on the side opposite the varix.

The form of varix to which a truss is most commonly applied is that which occurs at the saphenous opening (see

Chapter II.) The relief thus obtained is in some cases considerable; but generally the effect is *nil,* and occasionally the discomfort is distinctly increased by the truss. The only instances in which the truss is perfectly satisfactory are those in which the varix is for the most part, if not entirely, "subfascial"—the kind, in fact, which is least commonly associated with any symptoms of discomfort.

When the bulk of the varix is subcutaneous, the pressure of the truss often greatly increases the pain, by, I presume, compressing the dilated vessel against the margin of the rather rigid *fascia lata.*

In deciding as to the advisability of applying local pressure in these cases, it will be found as a matter of experience that if the discomfort felt by the patient is in any degree increased by outward rotation of the thigh, a truss will not only be useless but harmful.

In cases, however, in which pressure over the saphenous opening gives a feeling of comfort the best instrument to use is the ordinary French spring truss, with a very convex pad of ivory, boxwood, or vulcanite, which should be of about the same size as the prominent portion of the varix when the patient is in the standing position. Such a truss, if properly made, will keep in its place without any understrap, etc.

In stout subjects, a truss having a hinged pad is sometimes more easily adaptable to the parts; but a thigh-strap is necessary to keep the pad in its proper position.

For the application of truss-like pressure in the leg and about the knee, several plans are available.

In the case of a local exaggeration of varix above or below the knee, a knee-cap of wash leather, made to lace on the side opposite to that on which the varix is situated, and well padded over the part which corresponds to the varicose mass, is the most comfortable appliance.

In the leg, a very common source of trouble is a varicose condition of the long saphena and its branches, from the knee to the ankle or even lower.

If a stocking or bandage cannot, for some reason, be worn, much comfort is obtainable by the use of a truss composed of a spiral arrangement of clock-spring, which takes a turn or two round the leg, ending above and below in a small flat pad; the upper pad being fitted to press upon the dilated saphena

just below the knee, the lower one pressing on the vein a little above the ankle. If properly made, this truss keeps in position without any trouble; but it is rather difficult to get it fitted with sufficient accuracy.

Single cysts in the leg which give rise to pain from over-distention can usually be made comfortable by the pressure from a small pad fixed on a tightly-fitting well-suspended stocking, by a small spring truss made after the fashion of a bangle, or by a strap of kid which buckles round the limb and is provided with a pad which is adjusted over the cyst.

In addition to the fact which has been already mentioned, that nearly all the cases in which local pressure is useful are suitable for treatment by operation, it must be observed that relief is also obtainable, in most instances, by the ordinary complete bandage and stockings, unless the use of these is contra-indicated for some reason; the result being that local pressure will as a rule be required only when general pressure is intolerable.

Methods of Applying General Support to the Varicose Limb.

The appliances used, for this purpose may be roughly said to consist either of the following or of some modification or combination of them: bandages, stockings, leg-pieces, knee-caps, and thigh-pieces, which may in each instance be made of elastic or inelastic material as the requirements of the case indicate.

Bandages.—If properly used, these are far superior to the elastic stockings ordinarily supplied by the instrument-maker, especially on this account, viz., that the pressure can be regulated from time to time in accordance with the demands of the affected limb. In many cases it will be found that not only does the degree of pressure which is necessary for the support of a varicose limb vary from day to day, but that it varies at different times of the same day, so that the amount of pressure must be modified occasionally. This modification is more effectually attained by the reapplication of a bandage than by any other of the more expensive arrangements, in the form of laced stockings, etc., which are made for the purpose.

An objection is sometimes raised by patients to the use of

bandages on account of the supposed trouble in properly applying them; but in reality their adjustment is perfectly easy to the ordinary run of individuals. Moreover, if there is a little extra trouble sometimes in the management of the bandage, it is more than compensated by the relative additional comfort obtained.

In the slighter class of cases, especially if the skin be at all irritable, by far the best bandage is the "cotton-net," which, although containing no elastic medium, like india-rubber, is, from the manner of its manufacture, just sufficiently resilient to give a little "play" to the parts enveloped. It is also porous, and therefore cool. It further allows of exhalation through its interstices, which often prevents such complications arising as eczema, pruritus, etc. Finally, it is unaffected by wet or change of temperature, and is in every way cleanly.

In more advanced cases, when the pressure required is greater, nothing is equal in efficacy to the pure india-rubber ("Martin's") bandage, which is economical, cleanly, and, by reason of its elasticity, most manageable, as it adapts itself so accurately to the contour of the limb.

It is impervious to moisture, which, although a great advantage to patients who follow occupations entailing much exposure to wet, is often objectionable, inasmuch as no exhalation of any kind can take place through the india-rubber, which necessarily entails the confinement of all perspirations, etc., and is not uncommonly productive of eczema and other complications in patients whose skins are irritable.

The ordinary woven elastic bandage is quite effectual when new, but does not adapt itself to the limb so accurately as the pure rubber.

It also soon wears out, rapidly perishes when exposed to wet, or if often washed; so that it is expensive, and generally not too cleanly.

In the application of all bandages in varicose limbs two points should receive particular attention:

The bandage must not be too tightly applied at first. It is a very common mistake to bind the limb too tightly altogether. The amount of pressure as a rule required is very slight so long as it is uniform.

The bandage must be so adjusted that the pressure is greatest on the foot, which should of course always be included,

and gradually decrease toward the knee. The habit which some people have of making the last turn just below the knee the tightest—in order, as they suppose, to keep the bandage in position—is one of the commonest causes of the subsequent occurrence of pain, as it obstructs the circulation above and causes some swelling of the limb below under the loosest part of the bandage, which soon therefore becomes tight and causes distress. The result of this mismanagement often is that the bandage is discarded as being intolerable, when, if it had been properly arranged, much comfort might have been derived from its use.

The pure rubber bandage should never, excepting in those whose tissues are very insensitive to irritation, be worn next to the skin, but should be placed outside a closely fitting stocking (preferably silk). This is a good practical point to bear in mind. Many patients who are unable to wear this appliance actually in contact with the skin derive the greatest benefit from it when worn over the stocking.

For patients of the poorer class the Martin's bandage may be with advantage entirely relied upon, unless the irritability of the skin makes its use impossible.

With well-to-do people whose work or amusements entail exposure to wet it is equally suitable, and is quite perfect for use during the day. In the evening, after the day's work or pleasure is over, a light, easy-fitting elastic stocking may be substituted with great comfort.

Stockings—Anklets—Leg and Thigh Supports—Knee-caps, Elastic or Otherwise.—For the purpose of providing general support to the varicose limbs these are the appliances most commonly used. Theoretically, in all cases of varix in the leg in which a general support is necessary, the foot should be included in the appliance, at all events if that be of the elastic kind.

Practically, however, it will be found that a considerable number of patients derive more comfort from a support surrounding the leg only. Under those circumstances the material of which the support is made should not be elastic.

The number of persons who say that they are unable to bear the pressure of elastic stockings is quite curious. The reason of this intolerance is, in the vast majority of cases, either that the stockings are made altogether too tight, or

that they are improperly fitted; a point which will be referred to presently.

In slight cases, especially if the skin is irritable, the most effectual way to commence the wearing of this kind of support is by using merely an ordinary tight silk stocking. Frequently this will be sufficient in itself to produce the required pressure. If a more uniform and rather firmer support is necessary, a very thin silk elastic stocking, fitting rather loosely, may be worn. Subsequently, when the parts have become accustomed to elastic pressure, this thin elastic stocking may be replaced by one which is stouter and tighter. By such a plan a tolerance can often be brought about even in subjects with most irritable skins.

Generally, however, the whole question of tolerance on the part of the patient, and efficacy on the part of the stocking, depends upon the way in which the latter is made.

The main points to be borne in mind in this respect are those already mentioned in the description of the proper method of applying the elastic bandage.

The pressure should be greatest at the foot, and gradually diminish toward the knee.

On no account should the upper edge of the stocking fit more closely than the part below. This last is a common fault in the ordinary stocking supplied from the shops, the idea being that the tight upper edge should keep the appliance in its place; the result being that the patient is placed in the position of wearing an elastic stocking with a garter at its upper end—a condition naturally to be avoided. A properly-made stocking will keep its position without the constriction at the upper border, and is seldom, if ever, intolerable.

These remarks apply equally to all kinds of elastic supports, whether continuous or laced, and, so far as the arrangement of the pressure is concerned, also to every kind of inelastic appliance.

Of the supports which are composed entirely of elastic material, the most useful for ordinary purposes are the "Woven" and the "Spiral" forms.

The former are made of a continuous piece of woven elastic (cotton or silk), while the latter are made of long strips of silk elastic, which pass round the limb in spiral fashion (hence the name), the edges of the strips being stitched together,

thus forming a somewhat bandage-like arrangement of a size and shape which accurately meet the requirements of the part.

For rough wear, and in cases where great pressure is requisite, the woven form is on the whole the best; but in other cases, where less pressure is necessary, or if the skin is oversensitive, the spiral form is certainly better, since it generally can be made to fit more accurately, is cooler to wear, and less irritating; mainly, I fancy, because the line of stitching which connects the edges of the strip of material of which the support is made allows of a little ventilation and obviates the trouble which at times arises from the confinement of perspiration in appliances which are less pervious.

Either of the above forms may be made as a continuous arrangement, to pull on and off like an ordinary stocking, for example, or may be constructed to lace from end to end, which is a superior plan, inasmuch as it affords an opportunity for altering the pressure as occasion may arise; at the same time it is more costly, and of course requires rather more time and trouble in adjusting.

As a compromise between the continuous and laced, a most useful contrivance is the "Interrupted" stocking, which is merely the ordinary woven stocking, which, instead of being continuous from end to end, is interrupted by two vertical slits, on the inner or outer side of the leg, about two and a half or three inches in length, placed vertically one above the other, with an interval between them of three inches or thereabouts.

The edges of each of these openings are fitted for laces, which, as they are quite short, can be rapidly drawn tight or loosened without any trouble at all, since they need not be actually removed from the lace-holes.

When the stocking is first put on in the morning, the lower opening is laced quite tightly, the edges of the upper being left comparatively loose; later in the day, should discomfort arise from the pressure being too uniform in degree or direction, the lower lace is loosened, the upper one being drawn tight. Thus the incidence of pressure is sufficiently modified to afford complete comfort in many instances.

Patients who are unable to tolerate the ordinary kinds of complete elastic supports can often wear this interrupted stocking with ease.

Another convenient variety of elastic support is that which is only partly elastic.

Taking a stocking for example: the front is made of wash leather, buckskin, felt, or, better, some stout porous inelastic material, the back being formed of elastic.

Such appliances must be made to lace from end to end, and are particularly comfortable, especially if the varicose condition is circumscribed, in which case the elastic portion must be so arranged that it will lie over the varix when the support is in us

The elasticity in these particular contrivances is sometimes provided by an arrangement of small spiral springs, but there is no perceptible advantage in this modification, while it is more expensive than the silk elastic material commonly used.

A certain number of subjects are met with who cannot bear anything like elastic pressure, no matter how it may be applied.

In such patients the support may be composed of wash leather, buckskin, or kid, fitted accurately to the affected part, and lacing from end to end.

For the leg this is a most comfortable plan, as, the materials being so thin, the foot can be included.

A legging of soft felt moulded carefully to the contour of the limb, fastening with straps and buckles, is sometimes worn with more comfort than almost anything else, but as the felt is necessarily rather thick, a foot-piece cannot be conveniently adapted for wear inside the boot; at the same time it is not so essential that the foot should be included in the inelastic form of appliance, which acts merely as a passive support, as it is in the elastic form, which exercises active pressure on the parts.

So far as the appliances ordinarily used, whether elastic or inelastic, for affording support to the thigh in cases of varix are concerned, they will, as a rule, be found very difficult to retain in position, and so troublesome that the majority of patients prefer to discard them altogether as nuisances which give no relief at all proportionate to the trouble which is inseparably connected with their use.

The only thigh support which I have found really convenient is made in the form of very tightly fitting drawers of stout merino or thin elastic, continuous above with a light abdomi-

nal belt. It is, in fact, identical with the garment known at the better class of outfitters' as "belt-drawers," and if properly made it is a perfect as well as most comfortable arrangement in cases of varix, not only in the thigh but of the whole limb.

Bearing in mind the difficulties connected with the wearing of the ordinary appliances for the upper portion of the lower limb, it is satisfactory to know that, in a large number of subjects who are not too old, the discomforts produced by varicose veins in the thigh can be entirely relieved by one or other of the operative measures which will be described in the next chapter.

CHAPTER IV.

THE present chapter will be devoted to the consideration of the radical treatment of varicose veins by open operation, connected with which there are some points of practical interest of which I can find no mention elsewhere. This method of treatment, which, it is needless to say, is neither original nor novel, seeing that it was first practised centuries ago, has received but little encouragement from surgeons generally, until the past few years, on account of the risks which were formerly inseparable from it.

The apprehension universally felt on this account, even quite recently, is sufficiently attested by the fact that I was consulted only a few weeks since by a strong and healthy gentleman, upon whom I have since operated with the usual success, who was, as late as 1882, most strongly advised, by two of the first opinions in London, on no account to allow some varicose veins from which he was suffering to be excised, because the chance of "blood-poisoning" after the operation would be so great.

Fortunately, the recent advances in surgery, thanks to the better appreciation of the antiseptic system on the one hand, and improved methods of operating on the other, have so revolutionized practice that any surgeon possessed of ordinary dexterity is now in a position to advocate, in appropriate cases, this radical treatment, as being more certain and far-reaching in its results than any other method, while it presents at the same time, under ordinary circumstances, no appreciable risk beyond that which is connected with the administration of the necessary anæsthetic and the making of a clean-cut wound through the skin.

In varicosity of the lower limbs the treatment by operation must of necessity be applicable to a comparatively small number only of the cases of this affection which come under the care of the ordinary practitioner, since, as I have often pointed

out, the varicose veins are, in the majority of instances, so easily managed, with or without the aid of some of the various appliances to which I have before now called your attention, that the radical method, although quite justifiable, is generally not desired by the patients. Nevertheless, there occur both in hospital and private practice a fair number of cases in which so much benefit can be afforded by this radical treatment that the surgeon may with confidence recommend the operation, which, as I have already said, may, I fully believe, be practised without any risk worth serious mention if a due regard be paid to surgical cleanliness and some small technical details. Further, even if it be conceded for a moment that there is associated with the treatment some slight risk, the advantages gained in discreetly selected cases altogether outweigh any possible danger connected with the operation.

As a general rule, the question of the radical treatment does not present itself in cases of varicosity limited to the leg, in consequence of the extreme ease with which the affected veins are managed when they do not extend above the knee; so that the symptoms which from time to time arise are not, if proper care be taken, of a sufficiently serious kind to lead a patient to submit to the discomfort of an operation. It is also necessary to bear in mind that in the leg, excepting in certain cases to be specified hereafter, the operation very rarely entirely removes the necessity for wearing some kind of support, although in many cases the use of appliances may be made more tolerable.

In varix involving both the leg and thigh the matter is altogether different. As I have already shown, the wearing above the knee of an apparatus of almost any kind, if it be effectual, is in most instances so inconvenient and troublesome that patients will not infrequently submit with alacrity to any treatment which offers a reasonable prospect of relief from the intolerable nuisance at times connected with the use of such appliances. It is satisfactory to know, therefore, that by a simple operation, practically devoid of danger in any healthy subject, it is easy to remove in many of these cases all necessity for wearing any apparatus above the knee—an inestimable boon, especially with those to whom expense is an object and time of importance.

Apart, however, from the question of the relief from the necessity for the use of appliances in these cases, it is found, as a matter of experience, that many of the discomforts arising in varix of the leg are either entirely due to or much increased by the co-existence of varciose veins above the knee; and, further, that these discomforts are susceptible of great modification in many instances, and in a few can be completely removed, by the proper treatment of the affected veins of the thigh, especially when the disease involves more particularly the long saphena vein, or is associated with a circumscribed varix at the saphenous opening.

Operations Applicable.

Setting aside the various subcutaneous methods, which I do not propose to consider, as they may now be said to have entirely given place to the open method in the hands of most surgeons, the operations for the radical treatment of varicose veins may be divided into three kinds:

Those which aim at mere obliteration of some of the affected veins at one or more points.

Those which produce a more extensive alteration, in cases of general varix of the limb, by the removal of large or small portions of the dilated vessels.

Those which in cases of local varicosity effect a "cure" by the removal of the whole disease.

Technically, these operations, the first step in all of which is the formal exposure of the vein or veins by dissection, may be arranged in the four following classes:

A. The application, at one or more points, of single ligatures around a vein, thus obtaining as many centres of obliteration as may seem desirable in any given case, and so diverting to some extent the current of the circulation, producing at the same time an alteration in the incidence of pressure in different parts of the limb.

This proceeding, which approaches the ordinary subcutaneous method more nearly than any other of the open operations, but is more certain in its result, is suitable in a large class of cases of extensive general varicosity associated with one or more of the following conditions: intolerance of the use of appliances; distress from excessive distention of the dilated vessels; threatened bursting of thinned veins from over-

distention; and the co-existence with the varicose condition of intractable ulcers, eczema, and the like.

B. The division of a vein or veins between two ligatures.

This operation is applicable to the same class of cases as the foregoing, but is much superior to it, as not only is its absolute certainty of obliteration insured at the points of ligature, but, in consequence of the section of the veins, a certain amount of retraction of the divided vessels also occurs; a very important fact, since it gives entire relief to all pain and inconvenience which may be caused by the dragging, upon the parts above, of the heavy mass of full veins below. Hence the relief which follows this operation is certainly, as a rule, much greater than that obtained from the mere ligature of the vessels without division. This point, to which I can find no reference in the ordinary works on surgery, is particularly well seen in cases of circumscribed varix at the saphenous opening, in which, as I shall show later on, the affection often disappears after the division or removal of a portion of the long saphena vein in the thigh.

C. The removal from various parts of the varicose limb of short pieces of the affected veins, not exceeding one inch in length, each portion about to be excised having been previously included between two ligatures.

This method effects a similar result to operation *B*, but is probably more radical. It is particularly adapted to cases in which the varicosity, although excessive, is uniformly disposed so that no local exaggerations of varix offer themselves for extensive removal.

As many as six, eight, or even more of these short lengths of veins may be taken from one limb.

D. The formal removal by dissection of large portions of exaggerated varicosities in general varix.

All the ends obtainable by the two previous operations are better and more completely effected by this proceeding, which is, of course, also especially adapted for the complete cure of local varix of any kind, single or multiple cysts, solid tumors the results of ancient thrombi, phleboliths, etc.

E. The ligature of veins around large areas of exaggerated varix in general cases, or in local varicosities, with a view to so interrupting the blood supply as to cause a shrinking of the varicose mass.

I have ceased to practise this form of operation on account of its uncertainty, and because, while it is more uncertain, it is not less dangerous than the formal removal of the varix by dissection. The uncertainty depends upon the impossibility of making out which veins are really connected with the varicose mass. Upon this subject I can speak with some feeling, as I lost the confidence of a most valuable patient by assuring him that I could cure a varix, of very moderate size, on the inner side of the knee, by this operation; but although I ligatured no less than five large veins, apparently leading to it, the varicosity itself was not affected in any way whatever. Again, I have mentioned a case in the first chapter in which a large varix lying directly upon the long saphena was in reality altogether independent of that vein, which would certainly have been ligatured by any surgeon practising this operation, entirely without any good effect. I have seen other cases of the same kind.

Details of the Operations.

On this head it is not needful to say much. The strictest precautions as to surgical cleanliness must, of course, be observed in all cases, and on no account should the packing of the part in a wet carbolic dressing for at least four hours before the operation be omitted, the limb having been previously cleansed with ether and soap. In the mere application of a ligature, as in operation *A*, the vein is simply exposed through a small incision, and isolated by gently insinuating an aneurism needle or eyed probe beneath it, by which a ligature is carried round the vessel. This ligature is ultimately tied tightly, and its ends, of course, cut short. In operation *B* the vein is exposed for an inch and a half, more or less, and two ligatures applied as before, with an interval of an inch or thereabouts between them. The intervening portion of the vessel is then divided with scalpel or scissors, care being taken that the vein is not caught up by forceps—a point which, at first sight, may appear trivial, but which is, in reality, of some consequence, since a bruise of the vein may result in extensive thrombus, a complication to be avoided, as it may prove serious, especially if by any chance the attempts to keep the wound aseptic should fail. No sutures are necessary in either of

these operations, the careful application of the antiseptic dressing being all that is necessary. In the removal by dissection (operation *C*) of large areas of exaggerated varix in general cases, or isolated varicose masses, etc., a single or crucial incision is made over the mass; the cellular tissue having been laid open, and the veins in some part exposed, the whole varix may be quickly laid bare by running a director over and around the dilated vessels; an occasional touch with the scalpel is necessary, but the use of the knife is best avoided as much as possible, as there is less danger of wounding any of the veins—a mishap which, although not in any way serious, gives rise to some bleeding and prolongs the operation. As many as possible of the veins running into the varix are now divided one by one between double ligatures; the mass is then drawn out and removed, after any radicles which may have escaped division in the earlier part of the operation have been ligatured and divided. The complete exposure of the varix, at the commencement of this proceeding, is much facilitated by throwing the veins into full relief by tying a band around the limb above, sufficiently tightly to arrest the venous circulation to some extent. The margins of the wound, which are sometimes extremely thin, should be treated with as much delicacy as possible, being held, if necessary, by the fingers rather than forceps, to avoid bruising. The edges come together accurately, and are most conveniently sutured with chromic catgut, after all oozing from the surfaces has entirely ceased. The antiseptic dressing is finally applied, great care being taken to make equable and rather firm pressure, in order to secure early adhesion between the under surface of the skin-flaps and the deeper parts—a point of much consequence, as the integrity of the skin-flaps, when very thin, without doubt solely depends upon this early union between the parts mentioned. As a rule, the wound requires very little attention after the first dressing, which need not generally be changed for a week, by which time the healing will have probably been completed. I have seen in my cases neither fever, suppuration, nor untoward symptom. There is, however, one objection which a captious critic might make in connection with this operation, viz., that the final healing of the wound is sometimes rather tardy. Personally, I attach but little importance to this objection, since, so far as I have seen, even in

the slowest cases the parts are sufficiently sound at the end of ten days, or at the most a fortnight, to allow the patient to get about. One word as to the best kind of ligature is necessary. Chromic catgut is, on the whole, the most suitable material for all of these operations excepting the first, in which, as it is important that the ligature should not dissolve too soon, tendon is probably better.

In order to properly estimate the value of the radical method of treating varicose veins, and to ascertain the class of case which is likely to derive the greatest benefit from one of the several operations mentioned, it is convenient to consider the treatment as curative and palliative.

Curative.—The cases in which a "cure" in the true sense of the word (*i.e.*, entire obliteration or removal of the disease) can be effected must necessarily be somewhat limited in number, since they comprise the following conditions only: local and circumscribed varix, single or multiple cystic dilatations, solid tumors the result of ancient thrombi, or ordinary phleboliths.

A good instance of the first of these conditions was seen in a patient of mine, a railway-guard, who was in the Belgrave Ward, and from whom I removed a large mass of varicose veins situated at the inner side of the knee; the whole varix was confined to a tributary of the long saphena, which was itself quite uninvolved. The cure was complete, and the man now, some months after the operation, follows his ordinary occupation, no sign of recurrence having shown itself. I have already referred to another case of the same kind in which I removed fifteen inches of varicose veins from the calf of a man who has just recently returned to show himself without a trace of varicosity, seven months after the operation.

Of the second variety (cystic dilatation), I removed a good example from a girl who was in the Princess Ward; the cyst was a complete dilatation of the saphena just above the knee. The vein was exposed above and below, ligatured, and the tumor dissected out; it was about the size of a cherry. A second smaller dilatation existed lower down in the vein, but it disappeared spontaneously after the removal of the one above. This kind of cyst is most frequent in connection with the saphena vein, and is invariably developed near a valve. The pain to which these dilatations give rise when they be-

come distended is sometimes very great; I have had to remove several in private practice for this reason. In our museum there are some excellent specimens, especially one which was removed by Mr. Pick; it is as large as a walnut.

Of the solid tumors resulting from thrombi in these cysts, I dissected out a beautiful example from the leg of a gentleman forty years of age; it was said to have appeared suddenly, had existed for two months, was very painful, and as large as a filbert. It proved to be a venous dilatation, full of hard organizing clot, evidently the result of sudden coagulation at a former date.

Palliative.—Cases in which the radical treatment may be applied for palliative purposes are fairly numerous, and although in none of these can anything more than partial relief be, with absolute justice, claimed as the result of the method, because the actual disease is not entirely removed or completely obliterated, still the effect on the symptoms in many instances is so marked that, for all ordinary purposes, a cure may be said to be effected; indeed, the benefit derived by some patients is so great that it is difficult to persuade them that an actual cure, in the true sense, has not been produced.

Among the conditions wherein the relief obtainable from operation may be safely anticipated to be sufficient to justify the recommendation of this treatment, is that of general varícosity associated with some local excess of varix, or with distinct cystic dilatations. In many cases of extensive varicosity the whole of the serious discomfort experienced depends upon one or both of these conditions, which may be either in themselves painful, or by their presence interfere with the proper management of the varicose limb. Again, in similar cases, cysts, by repeated distention, increase until they are not only painful, but become so thin that they are liable to burst and may so give rise to serious bleeding.

The removal of these local conditions, when practicable, either completely or in part, is clearly not only expedient, but in some instances really necessary. Even in cases of general varicosity, in which the disease appears to be uniformly distributed, when there is much pain, the relief obtained by the excision of portions of the affected veins in different parts of the limb is very remarkable. I have myself seen an amount of relief which was almost incredible obtained by the removal of

considerable lengths of diseased veins from each leg and thigh in a case of double general varix.

As to the permanence of the relief in these palliated cases, it is difficult to give any very positive opinion, on account of the impossibility, as a rule, especially in hospital practice, of keeping the patients sufficiently long under observation to form a thoroughly reliable judgment on this matter. That the relief, however, lasts for long periods is certain. I occasionally see, for example, patients upon whom I operated five years ago, in whom the discomfort then relieved has shown no indication of recurrence. In the cases mentioned under the head of the "curative" treatment, I believe the cure to be quite permanent.

Among the cases in which the radical treatment can be with justice considered palliative only are two conditions which are of sufficient interest to merit separate consideration. One of these is the circumscribed varix at the saphenous opening; and the second is the "creeping" thrombus, to which I alluded in a former chapter.

Varix at the Saphenous Opening.—The effect of complete division, or, better, the removal of a considerable portion, of the varicose saphena vein in cases of this kind is remarkable, inasmuch as the operation is followed in many cases by a shrivelling of the varix at the saphenous opening. This rather curious fact, to which I have seen no allusion elsewhere, is well illustrated in the following three cases.

In 1885 I removed from the thigh of a gentleman about three inches of the long saphena vein, with a view to relieving some symptoms connected with extensive varix of the right limb. The relief afforded was all that could be desired, and, in addition, a result for which I was not in the least prepared, a large varix at the saphenous opening almost entirely disappeared—a most gratifying occurrence, as that condition was associated with much inconvenience, which the use of a truss had not in any way modified. When the patient was seen not long since, the varix in question was hardly perceptible.

A man in the Belgrave Ward, sixty-one years of age, whose case I have mentioned before for a different reason, had a large varicose mass above the knee and a varix as large as a pigeon's egg at the saphenous opening, with slight varicosity only of the long saphena vein itself. I removed the varicose mass at the knee and divided the saphena. The varix at the saphenous

opening rapidly diminished, and when the patient was last seen, four months after the operation, could hardly be distinguished.

A young woman from whom I removed a cystic dilatation from the trunk of the saphena, had also a well-marked varix at the saphenous opening, which three months after my operation was only just perceptible. I have seen other cases.

At first sight these results seem very peculiar, but in reality they are simple enough, and are easily explained in the following way: The existence of this varicose tumor at the saphenous opening depends, no doubt, originally upon dilatation of the proximal end of the saphena and adjacent part of the femoral veins, produced as I have described in my second chapter; subsequently, however, its size is much increased by the constant dragging upon the varix by the heavy weight of the distended varicose saphena and its tributaries in the limb below, so that the walls of the tumor at the saphenous opening are being continually drawn down, with the result that the small amount of contractility that they may possess is altogether overcome. The division, or, as is preferable, the removal of a portion, of the long saphena liberates its proximal end and relieves it from this continual downward traction, the effect being that the cystic varix above contracts and to a great extent disappears, especially if it receives for a short time after the operation gentle support from an accurately fitting but very weak truss.

Creeping Thrombus.—This condition is not altogether common. I have already mentioned a typical example, in which a patient in the course of several years has developed, in consequence of successive blockings, a thrombus which extends from the ankle to the middle of the thigh, and which must, if more blocks form (as is almost certain), sooner or later extend into the femoral vein. Now it seems to me that, as the coagulation always takes place upon the proximal end of the creeping thrombus, the rational treatment here is to separate this clot from the proximal patent end of the vessel by dividing the saphena between two ligatures above the thrombus, and this I propose to do if the patient will consent. In cases in which the creeping thrombus is small, I feel sure that the proper treatment is, at a time when the whole thing is quiescent, to remove the thrombus, with the vessels of course

containing it, and so do away with, as it were, the evil inclination to coagulation which is undoubtedly produced by the old clot, but which there is no real reason for supposing would necessarily occur in connection with the small new thrombus which may form as the result of the operation.

VARICOSE VEINS.

PLATE SHOWING THE FOLLOWING CONDITIONS BEFORE AND AFTER OPERATION.

MASS OF VARICOSE VEINS OVERLYING, BUT INDEPENDENT OF, INTERNAL SAPHENA, WITH LARGE VARIX AT SAPHENOUS OPENING.

FIG. 1.—Extensive varicosity of right lower limb in a hospital patient aged sixty-one. For two years he had been unable to work, on account of the pain caused in the mass above the inner side of the knee, and in the varix at the saphenous opening, by any exertion.

The mass above the knee was dissected out on September 27th, 1888, and was found to have no connection with the saphena, being entirely fed by veins coming from the deep parts of the thigh. The saphena above was also divided with a view to bringing about the subsidence of the varix at the saphenous opening.

The wound was healed soundly when dressed for the first time a week after the operation, with the exception of a small point at its upper end. By October 14th it was perfectly sound throughout.

FIG. 2.—The same limb three months after operation. There was no evidence of varix in the lower part of the thigh, and the tumor at the saphenous opening had shrivelled so much that it was imperceptible.

When last heard of, eight months after the operation, the man was working as a navvy.

This patient is the oldest subject upon whom I have operated for varix.

LOCAL VARIX AT INNER SIDE OF KNEE.

FIG. 3.—Circumscribed varix at inner side and rather below knee. The patient was a railway-guard, aged thirty-four, who suffered so much pain in the varix during his work, in spite of perseverance in the use of appliances, that he was on the point of giving up his occupation.

The affected veins were dissected out on November 22d, 1888, and he left the hospital perfectly sound on December 23d, the limb being in the condition shown in Fig. 4.

He immediately resumed his work in entire comfort. When the patient was last seen, nine months after the operation, there was no sign of a recurrence of varicosity.

In this case the varicose mass was fed solely by veins coming from the popliteal region and inner side of the leg. The long saphena was not involved.

CIRCUMSCRIBED VARIX IN CALF OF LEG.

FIG. 5.—Congenital circumscribed varix in calf of leg in a hospital patient aged 25. The man had been rejected as a recruit for the army on account of "varicose veins."

The mass was dissected out on October 4th, 1888, fifteen inches of tortuous and pouched veins being removed.

The varix was limited to the external saphena, with the exception of two small vessels which passed into the deep parts, one on each side of the upper end of the tendo Achillis. The recovery was uninterrupted, and on October 24th he left the hospital with the limb in the state shown in Fig. 6.

The patient was seen six months after the operation, when there was no sign of varix present, although he had been throwing considerable strain upon the limb in following the occupation of a gardener.

THE USES OF

ELECTROLYSIS IN SURGERY

BY

W. E. STEAVENSON, M.D. Cantab., M.R.C.P.

In charge of the Electrical Department at St. Bartholomew's Hospital; Physician to the Grosvenor Hospital for Women and Children; Late House Surgeon to St. Bartholomew's Hospital, and to the Hospital for Sick Children, Great Ormond Street.

THE USES OF ELECTROLYSIS IN SURGERY.

CHAPTER I.

ELECTROLYSIS.

It is difficult to draw a hard and fast line, or to define accurately in what cases the beneficial effects of galvanism are due to its electrolytic action, and those in which its stimulating properties, or influence as a nerve tonic, are of the greatest value. In all instances, no doubt, the electrolytic power is brought more or less into play, but this manual is devoted to those affections in which electricity is used as a remedial agent solely on account of its electrolytic properties, and in which it is, under certain circumstances, a useful mode of treatment.

No caustics or cauterizing agents used in surgery can be so accurately measured or so limited in their action, so quickly applied or so entirely under the control of the operator, as electrolysis and galvano-cautery; and yet for many purposes for which they are peculiarly adapted they are seldom or never employed.

The technical knowledge required, both of the apparatus and the manner of using it; the cost of the batteries and accessories; their number and diversity; the difficulty of keeping them constantly in order; and, above all, the amount of time required for their successful use, make it certain that electricity can never become one of the ordinary means of treatment to be employed in general practice; but there is no reason why the advantages claimed for it should not be made known, and those who have time to spare and wish to employ it should

not have the means afforded them of learning how it is to be used to the greatest advantage.

Some repetition takes place in discussing the process of electrolysis when applied to the different affections for which it is employed. This is due partly to the endeavor to make each description complete in itself, and partly to the fact that the work is to a great extent a collection of papers written and published at different times.

Electrolysis is the name given by Faraday to the property that electricity possesses of splitting up all chemical compounds, that are conductors, into their constituent elements or radicals. The chemical compound must be in a more or less liquid condition and is called the electrolyte. The points or surfaces at which the decomposition becomes manifest are called electrodes. The electrode connected with the positive pole of the battery is called the anode (or way up) and that attached to the negative pole is the cathode (or way down). The molecules of the electrolyte split up into elements or radicals and are called ions: those which appear at the anode are called anions, and those which appear at the cathode are called cations. The ions may be single atoms of an element or molecules which act chemically as radicals. "There is no direct attraction between the electrodes and the ions themselves, but the relation depends simply upon the temporary polarity they assume in the circuit."[1] When bodies composed of two elements are decomposed, one element turns toward the positive and the other toward the negative pole. The elements which separate at the positive pole are called electro-negative: they are the acid radicals, such as oxygen, chlorine, SO_4 (sulphion), etc.; while those which separate at the negative pole are called electro-positive, or basic radicals, as hydrogen and metals.

All the elementary substances have been arranged in an electro-chemical series:

Electro-negative.	*Electro-positive.*
*Oxygen.	*Hydrogen.
*Sulphur.	Gold.
Nitrogen.	Iridium.
*Fluorine.	Platinum (the least oxidizable metal).

Electro-negative.	*Electro-positive.*
*Chlorine.	Mercury.
Bromine.	Silver.
Iodine.	Copper.
Selenium.	Bismuth.
*Phosphorus.	Tin.
Arsenic.	Lead.
Chromium.	Cadmium.
Boron.	Cobalt.
*Carbon.	Nickel.
Antimony.	*Iron.
*Silicon.	Zinc.
	*Manganese.
	Cerium.
	Aluminium.
	*Magnesium.
	*Calcium.
	Strontium.
	Barium.
	Lithium.
	*Sodium.
	*Potassium (the most readily oxidizable metal).

Each element in the series is electro-negative to the one following it, but electro-positive to the one which precedes it. The first and last of the series, viz., oyxgen and potassium, are the two between which the greatest difference of electrical potential can exist.

A compound of any two of these elements when decomposed by a current of electricity splits up into the same proportions as those in which the elements combined; viz., their chemical equivalents, not into the proportions of their atomic weights. Thus in the electrolysis of water, there will not be one atom of hydrogen liberated at the negative pole for every sixteen of oxygen at the positive, but there will be two atoms of hydrogen to every sixteen of oxygen, that is, in proportion to their chemical quantivalency. In the decomposition of a solution of iodide of potassium, the iodine is liberated at the positive pole and the potassium at the negative. The potassium immediately decomposes the water, and hydrogen is evolved

at the negative pole. The presence of free iodine at the positive pole can be demonstrated by moistening some bibulous paper, previously steeped in starch, with a solution of iodide of potassium, and then applying both electrodes from a battery to the paper, on closing the circuit the action of the liberated iodine on the starch can be immediately seen at the positive electrode.

It has been pointed out that electrolysis splits up chemical compounds into the proportions in which their constituents have combined. Therefore, for every 127 parts of iodine liberated at the positive pole there would be 39 parts of potassium liberated at the negative pole; both elements being monads. In the decomposition of oxide of lead there will be 207 parts of lead liberated at the negative pole to every 16 parts of oxygen at the positive; while in the decomposition of chloride of tin there will be 118 parts of tin to (2×35.5) 71 parts of chlorine. Tin in this compound being a dyad. And further, in each cell of the battery employed it will be found that 65 parts of zinc have been dissolved. This is the case whatever number of cells are employed; therefore, whenever a sufficient number of cells has been employed to overcome the resistance of the electrolyte, any additional cells only accelerate the process, they do not increase the relative amount of decomposition that is taking place.

The theory advanced to explain the action of electrolysis is that the molecules of the substance electrolyzed turn themselves, under the influence of an electric current, so that the electro-negative constituent is directed toward the positive pole of the battery and the electro-positive constituent toward the negative pole. The molecule nearest to the positive electrode yields up its electro-negative constituent and it escapes free, but the electro-positive constituent acts upon the next molecule in the direct line between the two electrodes, attracting its electro-negative constituent and again repelling the electro-positive atom or atoms. This action continues on in each contiguous molecule and forms a direct chain of decompositions and re-combinations across the substance being electrolyzed until the negative pole or electrode is reached, and then the last molecule of the chain having yielded up its electro-negative constituent in the direction toward the positive pole, and there remaining no further molecules for the electro-

positive element to split up, it is therefore discharged free at the negative pole. Such is the theory of Grotthüss propounded in 1805, and, as applied to water, is explained by the accompanying diagram.

A is the positive electrode and B the negative one. In the upper line the molecules are seen arranging themselves so that the electro-negative element, the oxygen, is turned toward the positive electrode (A). In the lower line the molecules are arranged in a chain across the liquid. The atom of oxygen nearest the positive electrode is then liberated as free gas, and the two atoms of hydrogen combine with the atom of oxygen directed to it in the next molecule and form a new molecule. This action extends throughout the chain, and ultimately the two atoms of hydrogen of the last molecule, having no further

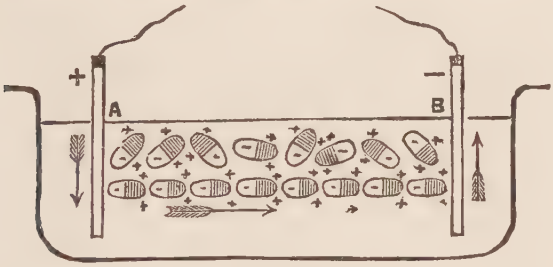


Fig. 1.

molecules to split up, are liberated as free hydrogen gas at the negative pole (B). Therefore the volume of gas liberated at the negative pole is double that liberated at the positive. No appreciable action or change can be seen or detected at any point across the fluid; the gases are liberated only at the points of contact with the electrodes. A similar diagram would explain the electrolysis of hydrochloric acid, only for the oxygen (the electro-negative atom) would have to be substituted one atom of chlorine and for the two atoms of hydrogen only one would be liberated by the decomposition of each molecule, because chlorine and hydrogen combine in equal volumes to form hydrochloric acid gas.

The primary effect of passing an electric current through an electrolyte is to split it up, the acid constituent passing toward the positive pole and the base or metal toward the negative pole. In this way Sir Humphrey Davy discovered

sodium and potassium very soon after it was found that electricity would split up water into its two constituent elements. Potash and soda were up to the time of Davy supposed to be elementary substances, but were then found to be oxides of the hitherto undiscovered metals sodium and potassium. The oxygen was liberated at the positive pole and the metal at the negative.

In very complex substances, such as those of which the human body is composed, double decompositions take place. The tendency of the galvanic current is to split up the tissues into their most simple elements, but some of these elements when in a nascent state seize readily upon any other elements in their immediate neighborhood with which they can easily combine. This makes the subject of electrolysis when applied to the human body such a difficult one. It is impossible in the present state of our knowledge to say how many decompositions, and fresh chemical combinations take place at either electrode. Animal tissue itself is such a complex substance, and the combinations of elements are so various, and in so many different proportions, that it is almost impossible to make a correct analysis; decompositions and re-combinations must vary with every description of tissue experimented on. Tissues that we call by the same name vary in their composition in different parts of the body, the same elements combining in different proportions in different situations. For instance in the case of fat: stearic and palmitic acid are present in greater proportions in one kind of fat than in another. Therefore any attempt to explain the exact decompositions and re-combinations which take place would fail, and if correct for one tissue would not hold good when applied to any other or to the same tissue in another situation. The main facts of electrolysis only can be stated.

By the investigations of Althaus and others it has been shown that at the negative electrode the potassium and sodium contained in animal tissue are liberated, as they would be in any solution of an inorganic compound that might contain them when subjected to a current of electricity. The freed potassium and sodium while in a nascent state seize upon the oxygen in the watery constituents of the body, and form caustic potash and soda, liberating hydrogen, which appears as bubbles of gas. The bubbles of hydrogen are said also to act

mechanically, and by insinuating themselves between the cells or fibres of the tissue being electrolyzed, split them apart, and thus accelerate their disintegration. The caustic potash and soda which are formed combine with any fatty or other constituent of the tissues with which they can form soap, and a deliquescent eschar is the result which gives an alkaline reaction to test paper. The action around the negative pole extends farther into the surrounding tissues than that around the positive pole, and therefore, if the current is continued for a long time, a slough is produced as it would be produced by the action of caustic potash or soda. No doubt other and numerous chemical changes take place at the negative pole, and to these other decompositions are due the better results which follow the treatment of unhealthy and ulcerated surfaces by electrolysis than when treated by simple caustic alkalies.

At the positive pole oxygen and chlorine are liberated and the reaction to test paper is acid. The tissues become oxidized, are deprived of their moisture, and are more or less charred, as after cauterization with a strong acid, such as sulphuric acid or nitric acid. Metals are decomposed, and by the theory of Grotthüss have a tendency to pass over and be deposited at the negative pole. The composition of the needles or electrodes employed in electrolysis is therefore a matter of consequence. The negative pole has no action upon metals, therefore an electrode attached to it remains as clean and bright after as before an operation. This is not the case with the positive needle or electrode, for the metal itself suffers decomposition during the process of electrolysis. This action of the positive pole is useful under certain circumstances. For instance, when electrolysis is used to destroy the fungoid growth or ulcerated surface of a cancer of the uterus, then if the electrodes be made of zinc, chloride of zinc is formed at the positive pole, and exercises its own peculiar cauterizing effect in addition to the oxidizing action of the electric current on the tissues. In the electrolysis of aneurisms additional advantage is obtained by the needles being composed of steel, for a chloride of iron is formed if the needles be made positive, and in addition to the coagulating effect of the positive pole we have the coagulating influence of the chloride of iron. Should the electrode applied to the part to be electrolyzed be plated, then if the electrode be made positive all the plating will be re-

moved at once and deposited in the tissues. This is important; and when the additional action of a metallic salt is not required, it is best to use the negative pole in electrolysis, especially in parts difficult of access, such as the os uteri, nasal ducts, urethra, or œsophagus; and also where it is desired to use a caustic, the action of which is somewhat analogous to that produced by the caustic alkalies. This can only be obtained by the use of the negative pole. When it is essential to use the positive pole, and the action of a metal is not required, it is best to employ an electrode made of platinum, for of all metals it is the most difficult to oxidize, and therefore affected less by the action of the current.

Other changes take place in animal tissue subject for some time to the passage of an electric current. The chemical constitution of some of the cells is so altered as to render them capable of reabsorption. This is one of the reasons why fibroid tumors and the gristly thickening about strictures are found to gradually decrease for several days after the application of electrolysis. Some of the constituent parts of these adventitious growths are so changed that they can be again absorbed; and during the passage of an electric current, if not afterward, ordinary osmotic action is increased in the direction from the positive to the negative pole. This would also to some extent account for the more ready reabsorption of any matter in a condition for being so disposed of.

Writing on this subject in a former work the following sentences occur:[3] "It is probable that changes are induced in the ultimate tissue cells of a part exposed to a constant current of electricity, analogous to the chemical action produced in the electrolysis of water. If the current is weak the process does not go so far as splitting up the watery parts of the cell into oxygen and hydrogen, but produces some sort of activity in the cell not present there before. It increases or alters the character of the secretion of the cells composing secreting glands, as evidenced by the increase of saliva, and metallic taste in the mouth produced by the application of a continuous current of electricity anywhere in the neighborhood of the salivary glands. This probable increased cellular activity (the quickening of the building up and destruction of cells never-ceasingly going on in the living body) is sufficient to account for the increased demand of blood required for these changes,

and the resulting increased supply afforded by the dilatation of the capillaries. The capillaries do not dilate by any power possessed by the constant current to cause muscular relaxation, but secondarily through nervous influence excited by the demand produced in the cells for more blood. The action of the constant current upon muscular tissue, if anything beyond besides inducing these probable changes in the ultimate muscular elements leading to increased activity in the ultimate cells, increased nutrition and therefore increased tone (as it is called), is probably to induce contraction rather than relaxation."

"In considering these changes in the cellular elements of the body and in the blood supply, the osmotic power of electricity must not be forgotten. It has been found that if two fluids of different densities be divided by a porous diaphragm and an electric current be made to pass through them, osmosis takes place in the direction of the current. If the current passes from the lighter to the denser fluid the natural osmotic action is increased; but if the current passes in the reverse direction the osmotic action is reversed, the denser fluid passing through the diaphragm into the less dense. The osmotic power of electricity probably explains the influence of galvanism in causing the absorption of fluid effused into joints or serous cavities when applied in such cases."

The theory of Grotthüss, in which the electrolyte must be fluid and in which no action takes place along the course of the current but only at the surface of the electrodes, is not quite applicable to animal tissue. In fact, the action of electricity applied to the human body is not primary electrolysis but a combination of primary and secondary electrolysis.[4] The several compounds forming the tissues acquire different polarities, and act and react upon each other. It was shown by Dr. Stone in his recent lectures before the Royal College of Physicians,[5] that the human body, after the passage of a continuous current of electricity for some time, becomes to a certain extent a kind of secondary battery, and gives off a current for some hours that is capable of deflecting the needle of a galvanometer, in fact, a certain amount of electricity becomes stored up in the animal tissues by polarization taking place in the cells of which the several structures of the body are composed, and which are traversed by the current.

The human body contains about fifteen elements (marked with an asterisk in the list of elements which appear on pp. 796, 797) combined in different proportions to form the several tissues. Each of these elements preserves its polarity to the next marked element in the series; being more markedly electro-positive or electro-negative according to the number of intervening elements that appear in the original list. In the electrolysis of animal tissue the electro-negative elements are freed or appear at the positive electrode, forming at once new chemical compounds, and the electro-positive elements are liberated or undergo similar re-combinations at the negative electrode.

It would require a most accomplished analytical chemist to decide what actual changes take place at the relative poles, but what we know is that oxygen and chlorine are liberated at the positive pole and give an acid reaction; and that potash and soda are formed, and hydrogen liberated at the negative pole. Some of the most likely bodies to be found in animal tissue are: albumin, which contains C.H.N.S.O.; mucin (C 52.2, H 7.0, N 12.6, O 28.2 per cent); gelatin (C 50.4, H 6.8, N 18.3, S+O 24.5 per cent); keratin (C 50.3–52.5, H 6.4–7.0, N 16.2–17.7, S 0.7–5.0, O 20.7–25.0 per cent) (Hermann's "Physiology"), and the saline solutions of the body containing potassium and sodium phosphates. In these compounds "the molecules of the simpler bodies, as OH, CH_3, NH_2, C_6H_5, occur in the most varied and intricate combination." "Of these complex substances," which occur in the human body, "only a few can be obtained in a pure condition; the remainder cannot, either because they are too unstable, or because they are not cystallizable; with regard to the majority, we do not therefore know even their composition by weight, much less their constitution. The greater the number of atoms which combine to form a compound, the greater becomes the complexity of its composition, so that elementary analyses are insufficient clearly to indicate its formula. The formulæ of the substances are for this reason unknown to us" (Hermann's "Physiology"). All we can say with regard to the electrolysis of these bodies is that the oxygen and sulphur have a tendency to be liberated at the positive pole and the hydrogen and potassium would traverse the space between the two electrodes, and appear upon the surface of the negative electrode.

This is the case with a simple compound and constitutes primary electrolysis. When the compound is more complex, what is called secondary electrolysis takes place. This would be the case in a solution of sodium sulphate, Na_2SO_4. We should obtain at the anode, from the splitting up of the water, an equivalent of oxygen, O, and an equivalent of free sulphuric acid, H_2SO_4, while at the cathode we do not obtain sodium, Na, but we have instead of it hydrogen, H, and in the solution we have also caustic soda, NaHO. This results also from the secondary splitting up of the water (Sprague, "Electricity," 1884). When we are dealing with mixed electrolytes, such as the substances composing animal tissue, the decompositions which take place are still more complicated.

It is difficult in the present state of our knowledge to say how many secondary electrolyses take place in the passage of the current through the body. That it meets several elements presenting different states of polarity toward each other is certain, but whether chemical change takes place at the points of junction of each of these heterogeneous bodies it is at present impossible to determine. As a rule, electrolysis does take place between any two substances of different electro-chemical polarity, at the point where they touch one another, if they are capable of conducting electricity. If in the treatment of a uterine fibroid we use an external potter's clay electrode, a water rheostat to regulate the current, and an intra-uterine electrode, we know that electrolysis takes place at four points: (1) in the cells of the battery; (2) in the water in the rheostat; (3) between the clay and the skin of the patient; and (4) at the point where the internal electrode touches the mucous membrane of the uterus.

That electrolysis does take place in the tissues is probable from the fact that storage of electricity takes place in a patient subjected to electrolytic treatment. The patient becomes a secondary battery, and is capable of giving off a current in an opposite direction to the one used from the primary battery. This can be demonstrated by disconnecting the rheophores from the battery immediately after an application of electricity has ceased and uniting them to each other, still keeping them connected to the electrodes in contact with the patient; a reverse current will immediately be shown on the galvanometer, at first about twelve milliampères, but rapidly

decreasing to eight and four. No storage of electricity could take place unless some decomposition of the fluids contained in the tissues electrolyzed had taken place. The liquids bathing the cell elements of the tissues must become split up, and their constituents accumulated on the opposite surfaces of contiguous tissue cells. It is this only that could produce a polarizing current. Becquerel and Faraday have shown that polarization results from the depositions caused by the passage of the current.

In the recent discussions on the electrolysis of fibroid tumors it has been asked: If the passage of an electric current has an effect upon the tumor, how is it that the normal structures, such as the bladder, muscles, skin, adipose tissue, etc., which are included between the two electrodes, are not also affected? It is quite possible that they are affected; that in them the normal tissue changes always in progress are accelerated, and that assimilation or "progressive metamorphosis" is encouraged. This from analogy would appear to be the case; for when a galvanic current is applied frequently to an atrophied and palsied muscle very often its nutrition is improved and its bulk increased; and Dr. Apostoli has drawn attention to the extraordinary increase of subcutaneous adipose tissue which takes place in the abdominal walls of patients subjected to the electrolytic treatment.

The theory suggested by Dr. Inglis Parsons, that the electric current has a modifying influence on the cells of a tumor which from their lower vitality could not recuperate themselves so well as the normal cells of the body, is also not altogether improbable. In fact, it may be that in the tumors subjected to a current of electricity a "retrograde metamorphosis" is set up.

Electrolysis must be distinguished from galvano-cautery. In galvano-cautery we use the power that a larger quantity of electricity has of heating a platinum wire to redness, white heat, or even to the fusing point, and its action on the tissues is exactly the same as the actual cautery. The tissues are really burnt—destroyed without any relationship to their chemical composition. But with electrolysis the chemical action of electricity is used. We employ an electric current of "high tension," as it is called, that is, possessing a great current strength, which, when a great external resistance has to

be overcome, as in the decomposition of water or the human body, depends upon a high electro-motive force. Chemical combination and decomposition are always accompanied by a rise of temperature, slight in some combinations, greater in others. The heat evolved in electrolysis is the same as heat evolved in other chemical processes, such as that which is familiar to us in the combination of sulphuric acid with water. But no greater heat is evolved in electrolysis than is dependent upon the activity of the chemical decomposition. In the electrolysis of water the heat produced is inappreciable, except with the most delicate tests, and in the decomposition of nævi appreciable heat is only evolved when the action is very rapid. Electrolysis is really a chemical decomposition by means of electricity, while destruction of a tissue by galvano-cautery is accomplished by actual heat, without any regard to the chemical composition of the parts destroyed. This much is necessary to clear up a very common error that prevails, viz., confounding the two processes of electrolysis and the use which is made of the heating power of electricity which is called galvano-cautery.

The battery required for electrolysis is very different to that employed for galvano-cautery. For electrolysis it is necessary to have a battery of high electro-motive force, for the purpose of overcoming the great resistance which is offered by all substances to their chemical decomposition by electricity.

The electro-motive force of a cell is the difference of potential between the positive element and the negative, and depends upon the composition of the elements and the nature of the exciting fluid in which they are bathed, and which acts chemically on one or both of them. The difference of potential thus established between the elements of a cell produces electro-motive force, and electro-motive force is that force which tends to move electricity in a circuit, when the circuit is closed. The volt is the standard of electro-motive force, and is very nearly the electro-motive force produced in one Daniell's cell.

The unit of resistance is the ohm. An ohm is the amount of resistance offered by 48½ metres of copper wire of one millimetre diameter. Dr. Stone, of St. Thomas's Hospital, has shown [5] that the human body offers a resistance of from 900 to 1,000 ohms. This is when two large surface electrodes are

both applied to the skin. If the electrodes are small the resistance of the body will appear greater. And if one large surface electrode is applied to the skin, and another electrode to some internal mucous membrane, the resistance will be very much less. If both electrodes are introduced beneath the skin the resistance will be still further diminished.

The ampère is the unit of current strength and is produced by one volt through the resistance of one ohm. In medicine one thousandth part of an ampère or a milliampère is the unit used. The current strength produced by a battery is determined by the accumulated electro-motive force of the cells divided by the resistance. This is what is called Ohm's law, and is expressed by the following formula:

$$\text{C. S.} = \frac{\text{E.}}{\text{R.}}$$

The current strength (C. S.) equals the electro-motive force (E.) divided by the resistance (R.). The resistance (R.) opposed to chemical decomposition is very great, and therefore the numerator (E.) of the fraction has to be proportionately greater, so that the current of electricity may be of sufficient strength to overcome it. To insure this a large number of cells have to be coupled together "in series," that is, the positive element (zinc) of one cell coupled to the negative element in the next cell, and so on, leaving a negative and positive pole at either end of the series of cells to which to attach the rheophores. The rheophores are the conducting cords leading from the battery to the electrodes. In this way the electro-motive force of each cell is added to that of the next, and with a large number of cells we thus insure a high current strength. It is therefore necessary to have a battery composed of a large number of cells; and small cells are as efficacious as large ones as regards their electro-motive force, but they become more quickly polarized.

If there is more resistance in the circuit only a smaller amount of electricity can get along it in the same time, and therefore there is less chemical action in the cells. The chemical activity in a cell depends upon the strength of the current of electricity, and the strength of the current depends upon the facility with which it can get round the circuit, *i.e.*, upon the resistance offered to its progress; therefore if the resist-

ance is great the chemical activity in the cell is reduced. Beyond a certain limit large cells (containing large elements) are of no advantage. The internal resistance is so small as compared with the external resistance that it may be ignored, but if the external resistance is also very low, then any slight reduction in the internal resistance materially influences the strength of current. A cell composed of certain elements is only capable of a certain electro-motive force, no matter under how favorable circumstances it is placed. The difference of potential produced in each cell of a battery composed of the same elements when joined "in series" is the same, therefore if one large cell be interposed among a dozen small ones the chemical action taking place in it when the circuit is closed will not be greater than that taking place in the other smaller cells.

The requirements of a battery to be used for electrolysis are: that it shall be fairly portable; that the individual cells shall be made of such materials and in such a way as to possess a high electro-motive force; that there shall be some provision when the battery is in action to prevent as much as possible polarization taking place; and that there shall be some arrangement for bringing the cells consecutively into the circuit without any make or break of the current.

Polarization is a term used to designate a condition which is induced, more or less, in all batteries when in action. This condition is the tendency to the formation of a gaseous film on the surface of the elements produced by the chemical changes that take place in the battery. This film, besides increasing the internal resistance of the cell, produces a slight electro-motive force in the opposite direction to that produced by the battery, and acts in antagonism to it. The more violent the action in the cell, that is, the greater the strength of the current that is passing through it, the more rapidly does this polarization tend to take place. A film of hydrogen is deposited on the negative element and a film of oxygen on the positive. The hydrogen is electro-positive to the oxygen, and therefore sets up a backward electro-motive force. To prevent this polarizing action different devices are employed in different cells. One of the commonest in sulphuric-acid cells, wherein an excess of hydrogen is liberated on the formation of sulphate of zinc, is to add bichromate of potash ($K_2Cr_2O_7$).

The oxygen is liberated, and forms water with the hydrogen which is displaced.

Many operators prefer to use all the cells of their battery at once, for the purpose of lessening the chance of giving a patient a shock by any interruption of the current that might take place when the cells are added one by one to the circuit. The strength of the current is then regulated by a rheostat. Another advantage in employing a battery in this way is that all the cells get an equal amount of work. A current is obtained of high electro-motive force but not of great quantity, so that the action in each individual cell is not very great.

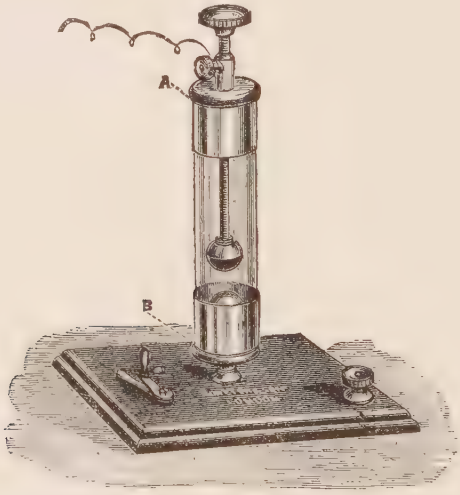


Fig. 2.

When a strong current is obtained from a few cells those cells perform a relatively greater amount of work and sooner become exhausted.

Rheostats are made in different ways. They are simply arrangements by which an additional amount of resistance can be introduced into the circuit, and this resistance can be gradually reduced, with the result of a corresponding increase in the current strength. The best form of rheostat is made with bobbins of German silver wire, each bobbin having a known resistance in ohms. By including a greater or less number of these bobbins in the circuit the resistance can be increased or diminished. Another form of rheostat is made

by including in the circuit a certain length of badly-conducting composition, and an arrangement exists for including a greater or less length of this composition in the circuit. One objection to this form of rheostat is that the actual amount of resistance at any time in the circuit cannot be accurately and readily determined. A third form of rheostat is used which consists of a column of water inclosed in a glass tube, through the cover of which a metallic rod connected to the battery can be raised or lowered so as to approach or recede from a piece of metal at the bottom of the tube which is in onnection with the electrode; the current, therefore, has to traverse the water. The amount of resistance is directly proportionate to the distance of the metal rod from the metallic base of the tube (Fig. 2).

An objection to the employment of a rheostat to regulate the current in performing electrolysis is that an additional instrument is introduced, and therefore an additional point at which some difficulty may occur through the instrument not working properly; and a water rheostat offers such a very high resistance that with all the cells of a thirty-cell battery in use we sometimes cannot obtain as high a current as we require, and therefore have to employ batteries of such power and size that they cease to be portable.

The batteries most commonly used in medicine and surgery are:

Daniell's, electro-motive force,			.	1.079 volts.
Smee's,	"	"	.	1.62 "
Leclanché's,	"	"	.	1.5 "
Chloride of silver,		"	.	.915 "
Stöhrer's,	"	"	.	1.825 "
Grove's,	"	"	.	1.956 "
Bunsen's,	"	"	.	1.964 "
Bichromate of potash,		"	.	2.000 "

The Leclanché and Stöhrer batteries are the best for electrolysis.

The Stöhrer's battery is the best for hospital practice, as it lasts longer, its electro-motive force is greater, and it less frequently gets out of order. The Leclanché battery is perhaps more suitable for the consulting-room and for carrying to patients' houses, as it is more portable than the Stöhrer's, but

the electro-motive force of each cell is not so great, and they more quickly become polarized.

A Stöhrer's battery is composed of "single fluid" cells, and is a modification of the Smee battery. The negative elements of the latter (platinized silver) are replaced by plates of carbon. The elements are immersed in a solution of sulphuric acid and water, and to diminish polarization some bichromate of potash may be added. This form of battery possesses a high electro-motive force, it is fairly constant, and not quickly destroyed by being "short circuited," as is the case with the Leclanché. The fluid and zinc elements can be easily replaced when exhausted. The fluid consists of one part of sulphuric acid to ten parts of water; to this one tenth part of a saturated solution of bichromate of potash may be added. The elements

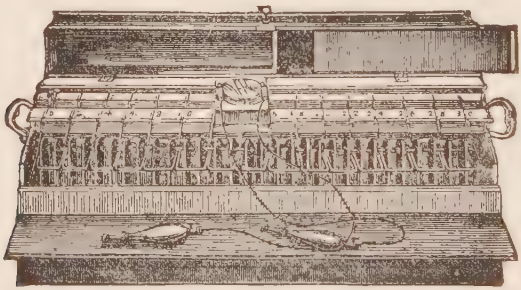


Fig. 3.—Stöhrer's Hospital Battery.

are coupled "in series," that is, the zinc of one cell is connected with the carbon of the next. The cells are brought into the circuit two at a time by a travelling commutator, which can be pushed along a wooden frame, to which all the elements are attached. The cells, made of glass, which should be about two-thirds filled with the exciting fluid, are arranged in a tray which can be raised to the elements when it is wished to put the current in motion.

Theoretically, no action ought to take place when the elements are immersed until the circuit is closed, but practically chemical action does take place on account of impurities in the zinc; and if the tray is not lowered after the battery has been used, these local circuits destroy its action by consuming the zinc and "killing" the exciting fluid by reducing it to a solution of sulphate of zinc. This tendency to local action in

the cells is lessened by the zinc being amalgamated, that is, its surface covered over with a layer of mercury. Amalgamated zinc is also more positive than ordinary zinc, and therefore renders it a better source of electricity. Amalgamation is performed by cleaning the surface of the zinc, then dipping it in a solution of sulphuric acid and water (1 in 10), and afterward pouring over its surface some pure mercury, which is to be well rubbed in with a piece of wood covered with some linen rag, until the surface of the zinc has a bright metallic lustre; the zinc is then to be set aside to allow the superfluous mercury to drain off. The zincs of a battery, when constantly in use, should be frequently amalgamated. A small pinch of sulphate of mercury added to each cell will help to preserve the amalgamation.

A Stöhrer's battery, when freshly charged, and with newly amalgamated zincs, has an electro-motive force of nearly two volts (1.825) per cell. The commutator consists of a cylinder of ebonized wood, the ends of which are covered by metal sheaths, which overlap toward the middle, but do not touch one another. These metal ends are marked respectively + and —, to indicate the poles. The cylinder can be half rotated by a small handle, and the metal ends are thus alternately made to touch one or other of two side springs, which communicate with two side brass rails underneath the travelling platform on which the cylinder is placed. By this means the poles can be reversed. The side brass rails, when the platform is moved, traverse a groove in the frame to which the elements are attached, and touch successively each pair of thick brass wires that support the elements. These metal rails are long enough to touch two pairs of brass element holders at the same time, so that when slid along the groove a fresh pair of brass wires are touched before the previous ones cease to be in contact. By this means the strength of the current can be increased gradually without causing a make and break. It is of great importance to be able to recognize the poles of the battery. It will not do to trust alone to the + and — signs on the commutator. Occasionally, when the battery has been sent away for repair, it will be returned with the travelling commutator reversed, and then the signs will indicate the opposite to what is correct. The error can always be rectified by remembering that in each cell the direction of the current

is from the zinc to the carbon, and, outside the battery, from the carbon to the zinc. Therefore the zinc, although the positive element, will give the negative pole. By tracing up the brass wire which conducts the current from the zinc to the commutator, the negative pole can always be determined, and if this does not correspond to the sign on the commutator, the commutator should be taken off, turned round, and then replaced. I have often known an accident to occur by the commutator being placed the wrong way, such as the decomposing of the plating on a metal electrode in the urethra, by the

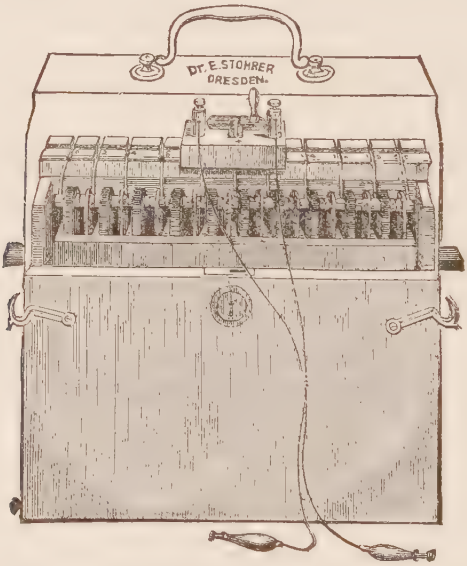


FIG. 4.

positive pole being substituted for the negative, or a large slough produced on the face, when destroying a mole, through the negative pole having been used instead of the positive.

There are many other sources of accident in the use of these batteries. The connection of the rheophores to the battery may not be secure, or the rheophores themselves may be imperfect in some part of their length or where attached to the electrodes. Oxidation may have taken place between the side springs and the cylinder of the commutator, or the side springs may have become bent so as not to touch the commutator. The same may be the case with the side rails underneath the

commutator, so that they do not touch each pair of wires bearing the elements, or not be capable of touching two pairs at the same time. The carbons and zincs may not have been properly screwed together. One of the carbons may have been broken in moving the battery, and when the travelling commutator reaches it an interruption of the current will take place and the patient receive a severe shock. The same would occur if one cell had not received sufficient fluid or had been left altogether empty. A zinc or carbon may become inserted in an adjoining cell to the right one. Under any of these circumstances a patient may receive an unpleasant and sometimes a dangerous shock. All these faults have been known to occur in batteries, it therefore behooves the operator to examine his battery carefully before using it and to know where errors may exist and to be capable of rectifying them. The use of a water rheostat will prevent many of these accidents, but it is subject to the objections already mentioned. Leclanché batteries are also used for electrolysis, but more cells are required on account of their lower electro-motive force and greater internal resistance, and the number of cells has to be gradually increased to keep up the required current strength on account of the rapid increase of the internal resistance due to polarization. A Leclanché battery is more portable than a Stöhrer's and therefore can be more easily taken to a patient's house.

For those who frequently employ electrolysis, and who attempt to keep their own batteries in order, a voltmeter is necessary for the purpose of taking the electro-motive force of the cells. It often happens that a battery when in use very rapidly loses its strength, and a battery that has been used for some time will often be found to have deteriorated. It is very annoying after the application of the current to a patient has commenced to find that the battery will not give the required current strength. The knowledge also of the electromotive force of the battery enables us to calculate the resistance offered by the patient. A voltmeter is really a galvanometer possessing coils with a resistance of several thousand ohms, in comparison with which the internal resistance of a battery may be neglected, therefore the current strength sent through it will represent very closely the electro-motive force of the cells of the battery employed and can be shown on the

instrument if it is properly graduated. A voltmeter graduated to show from one to twenty volts is the most useful for medical batteries.

It was discovered by Oersted in 1819 that a current of electricity passed along a wire in the vicinity of a suspended magnetic needle caused the needle to alter its position. If the current was passed above the needle from north to south the needle was deflected to the left, and *vice versâ*. This phenomenon has been utilized for the purpose of constructing an instrument called a galvanometer, with which the strength of a current can be measured. A needle is suspended by a cocoon fibre or silken thread, or is balanced on a sharp point, in the centre of a coil of wire. When at rest the needle is parallel to the coil and in the magnetic meridian of the locality in which the instrument is used. If an electric current is passed through the coil the needle is deflected to the right or left according to the direction and strength of the current. By allowing the needle to swing over a graduated scale the strength of the current can be read off. Each galvanometer has to be separately calibrated by comparison with a standard galvanometer. "Equal deflections on the same galvanometer always indicate currents of the same strength. But it must be carefully kept in mind, 1st, that the angle of deflection is not proportional to the current, and 2d, that it differs for the same current strength from galvanometer to galvanometer, depending on the length or number of turns of the wire, and upon the position of the coil with reference to the needle."[6] "When the deflections are small (*i.e.*, less than 10° or 15°), they are very nearly proportional to the strength of the currents that produce them. Thus, if a current produces a deflection of 6° it is known to be approximately three times as strong as a current which only turns the needle through 2°. But this approximate proportion ceases to be true if the deflection is more than 15° or 20°, for then the needle is not acted upon so advantageously by the current, since the poles are no longer within the coil, but are protruding at the sides, and, moreover, the needle being oblique to the force acting on it, part only of the force is turning it against the directive force of the fibre; the other part of the force is uselessly pulling or pushing the needle along its length. It is, however, possible to 'calibrate' the galvanometer—that is, to ascertain by

special measurements, or by comparison with a standard instrument, to what strengths of current particular amounts of deflection correspond. Thus, suppose it once known that a deflection of 32° on a particular galvanometer is produced by a current of $\frac{1}{100}$ of an ampère, then a current of that strength will always produce on that instrument the same deflection, unless from any accident the torsion force or the intensity of the magnetic field is altered."[7] The magnetism of the earth has an influence upon a horizontal galvanometer which has to be taken into consideration when it is calibrated. A galvanometer which is graduated to read correctly in Paris will not do so in London. When calibrated to a scale of milliampères in the former place the error would amount to about 4 in every 198 milliampères in the latter. Therefore horizontal galvanometers have to be calibrated for the locality in which they are to be used. These details are arranged by the instrument makers. A vertical galvanometer is free from this source of error, for if the magnetization of the needle remains uniform a given current will deflect it to the same extent at whatever part of the globe it may be used. The needle of a vertical galvanometer also settles more readily when influenced by a current, and can be more easily read from a distance. But the magnetization of the needle is not constant, it gradually becomes demagnetized, and is therefore not so readily influenced, and after it has been used for some time will indicate a weaker current than is actually passing. It is therefore necessary from time to time to remagnetize the needle of a vertical galvanometer.

To Dr. de Watteville we are indebted for the introduction of the milliampère as the unit of current strength for medical purposes. It is the most useful that could have been adopted, for it represents very appropriately the strength of the current usually used in medicine and surgery. The human body under various circumstances offers a resistance of from a few ohms up to several thousands, according to the description of electrodes used and to the part to which, and the mode in which, they are applied. In some cases we want to register single milliampères and in others several hundred; it is therefore necessary to have two or three galvanometers, one calibrated to a low scale for weak currents, and another graduated higher for stronger currents, or some arrangement in the

galvanometer for allowing the current to pass through shorter coils of wire, and therefore deflecting the needle less, for stronger currents. Such a galvanometer is Edelmann's, supplied by Mr. Schall, of Wigmore Street (Fig. 5). It is particularly useful for electrolysis, for it will register from 4 milliampères, the strength of the current used for the electrolysis of the Eustachian tube and lachrymal canals, up to the 250 milliampères used for the electrolysis of fibroids, or the four or five hundred sometimes used in the destruction of nævi when needles attached to both poles are introduced beneath the skin. "The enormous variations in the resistance offered by the human body to the current, according to the individual and the details of the application, make even an approximate estimation of the current strength by the number of cells used, impossible. The electro-motive force of the cells themselves, and their internal resistance, differ from battery to battery, and from time to time in the same battery. The deviation of the needle alone can give us any information as to what is going on in the circuit, and allows us to compare the currents and insure their uniformity when required."[8] The adoption of a standard measurement "enables us to not only compare the results obtained by ourselves on the same galvanometer with one another, but also with those obtained by other observers on galvanometers of any construction, but graduated in absolute units."[9]

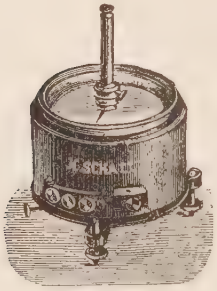


Fig. 5.—Edelmann's Galvanometer.

By means of the galvanometer we can also approximately estimate the resistance offered by the human body or that part of it included in the circuit.

One volt through one ohm gives one ampère.

By Ohm's law the—

$$\frac{\text{Electro-motive force}}{\text{Current strength}} = \text{Resistance.}$$

Therefore if the electro-motive force of the battery is known (as taken by a voltmeter), and the current strength known as registered by the galvanometer, we can calculate the resistance in the circuit.

If twenty Leclanché cells (E. M. F. 30 volts) give a current strength of 10 milliampères—

$$\frac{30 \text{ volts}}{0.010 \text{ ampère}} = 3.000 \text{ ohms.}$$

If we deduct the resistance of the battery (about 90 ohms) and the galvanometer, we have the resistance of the body. The resistance offered by the galvanometer is generally stamped upon it.

With the help of a resistance coil, the resistance of which is known, say one of 500 or 1,000 ohms, we can by the galvanometer also ascertain the electro-motive force of a battery. For this purpose we must take Ohm's law thus:

Current strength × resistance = electro-motive force.

For instance, if the current strength is 10 milliampères, and the resistance of our coil and galvanometer 1,050 ohms, then the electro-motive force would be—

0.10 ampères × 1,050 ohms = $10\frac{1}{2}$ volts.

The readings of a galvanometer can be verified by a very simple water voltameter made by Gaiffe, of Paris (Fig. 6). It consists of two glass tubes, the inner one graduated so that each small division represents 50 cubic millimetres. Both tubes are filled with water; the outer one serves as a reservoir. The inner tube has two openings at *o o* by which it can be replenished. Inside the inner tube at the bottom are two platinum wires, separated from each other by a piece of glass, and connected respectively with the two binding screws on the base of the instrument. These screws can be connected respectively by rheophores with the positive and negative poles of a battery. When the circuit is closed oxygen is given off from the platinum wire connected with the positive pole, and hydrogen is given off from that connected with the nega-

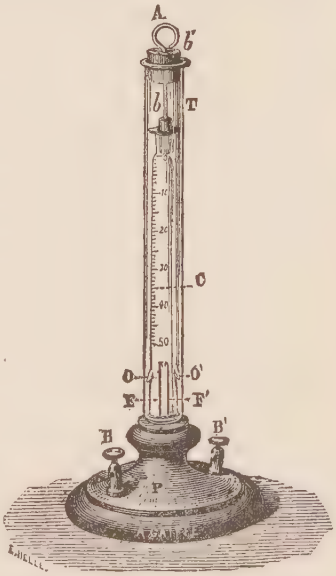


Fig. 6.

tive pole through the decomposition of the water. The gases accumulate at the upper end of the inner tube, and the level of the water sinks. When an observation is completed fresh water can be admitted into the inner tube through the apertures, *o o*, by raising the cork at the top which is attached to a brass wire that passes through the cork in the larger tube. The inner cork can thereby be raised without removing the larger one.

A current strength of one ampère liberates in the electrolysis of water 114.6 cubic millimetres of hydrogen and 57.3 cubic millimetres of oxygen per second, or 171.9 cubic millimetres of mixed gases; say 172 cubic millimetres per second, or 10,320 cubic millimetres per minute. Therefore one milliampère will set free $\frac{10320}{1000}$ cubic millimetres per minute, or 10.3 cubic millimetres.

Each small division on the inner tube of the voltameter represents 50 cubic millimetres, therefore, if one of these small divisions is filled with mixed gases per minute, the current strength employed would be 5 milliampères. We have therefore to divide the number of cubic millimetres of mixed gases produced per minute by ten to obtain our current strength in milliampères.

In the same way as with the galvanometer, knowing the current strength produced and the electro-motive force of a battery, we can with the voltameter calculate the amount of resistance in the circuit, and knowing the amount of resistance by the use of resistance coils of a known number of ohms, and the current strength calculated by the voltameter, we can ascertain the electro-motive force of any battery that we may use.

One Daniell's cell will not accomplish the electrolysis of water. The resistance of water to the passage of electricity is so great that if a circuit between the two poles of a Daniell's cell be completed by its intervention no current will pass at all. It is only when by the combination of two or more cells we obtain a sufficient electro-motive force to overcome the resistance of the water that the needle of a galvanometer will be deflected. I tried a short time ago with some Leclanché cells which had been used, and it required four before the slightest deflection of the galvanometer needle could be detected, and that showed barely a current strength of a quarter of a milliampère, and the very smallest bubbles of hydrogen gas

could be seen escaping at the negative pole. It will, therefore, be seen that for the efficient accomplishment of electrolysis in the human subject, which offers nearly as much resistance as water, a battery composed of a fair number of cells is required, and these cells, if few in number, should have been recently charged.

A certain combination of elements, such as compose the cells of any particular battery, is only capable of producing a current of a certain electro-motive force. The electro-motive force produced depends upon the constitution of the cell, not on its size. Sulphuric acid will attack or decompose zinc only at a certain rate, sixty-five grains of zinc replacing every two grains of hydrogen in the sulphuric acid. However large the cells may be, this scale of interchange goes on, and anywhere else in the circuit where a chemical compound has to be traversed it is split up exactly in the same ratio; thus, if water should be the compound, directly a current strong enough to overcome the resistance of chemical affinity is used, sixteen grains of oxygen and two grains of hydrogen are liberated in the water for every sixty-five grains of zinc consumed in the battery. Therefore a sufficient number of cells have to be used to overcome the resistance. We all know that by Ohm's law current strength equals the electro-motive force divided by the resistance. In the Daniell's cell the electro-motive force (E. M. F.) = 1 volt; and one volt through the resistance of one ohm gives a current strength of one ampère, and would take a second to pass. If we take water as offering a resistance of 1,000 ohms (it must be remembered that the resistance of water varies very much according to its temperature), then one volt passed through water would give the current strength of a milliampère, and would take 1,000 seconds to produce the same amount of mixed gas as one ampère would produce in one second. If the resistance is very great the current strength is very small, and the chemical action going on in the battery cell takes place very slowly. When electrolysis is applied to animal tissue chemical changes take place in it which theoretically exactly counterbalance the chemical changes which are taking place in the battery. When sixty-five grains of zinc are decomposed in the battery a corresponding number of grains according to their chemical quantivalency are decomposed among the elements which form the substance electrolyzed.

By the recognition of these facts we can determine the current strength that it is advisable to use according to the result it is wished to accomplish. If we wish simply to destroy an unhealthy surface we use a weak current, if we wish to dissolve a gristly stricture we use a slightly stronger current, or a weak current for a longer time; and if we wish to produce profound tissue changes in a uterine fibroid we use a very strong current.

The exact current strength employed cannot be accurately gauged by the number of cells that are included in the circuit. To obtain this information it is necessary to use a galvanometer or a voltameter.

The galvanometer is the more handy instrument, as it has already been graduated, and it is only necessary to read off the number of milliampères that are being used. With the voltameter some calculation is necessary before a knowledge of the current strength can be arrived at. The pole to which an electrode is attached, and the metal of which it is composed, is of some consequence, and varies in medical practice according to the diseased condition for which electrolysis is employed, and the effect it is wished to produce. Unless the action of the metal itself is also required, it is necessary when using the positive pole to have the electrode made of platinum, because platinum of all metals is the least oxidizable, and therefore does not itself enter into the new combination of elements formed around the positive pole.

CHAPTER II.

ANEURISM.

ELECTROLYSIS has been occasionally used now for many years in the treatment of aneurism. The results up to the present time appear to have been very uncertain, but the operation has proved successful in rather less than one-third of the cases in which it has been tried. It must be borne in mind that it has as a rule been tried only in inter-thoracic or abdominal aneurisms, which would no doubt be the most difficult to treat by compression or ligature, even if such procedures could in these regions be effectively accomplished.

The operation has been performed in various ways, by needles attached to both poles of the battery, or by needles attached to the negative or positive pole only. Sometimes weak currents have been used and sometimes very strong ones. The duration of the operation has also varied in different cases. Sometimes insulated needles have been employed and sometimes the reverse.

Galvanism was first used for aneurisms by Phillips in 1832, but was unavailing until Pétrequin (1845) and Ciniselli improved the apparatus and methods of employing it. It is also used only for aneurisms that cannot be successfully treated by other means, such as internal aneurisms, thoracic and abdominal, or those situated at the root of the neck. Varicose and cirsoid aneurisms can also be often more successfully treated by galvano-puncture than by any other method. Mr. Holmes says an internal aneurism is best adapted for this treatment when it is pressing on the parietes, but has not absolutely perforated them. Again, the sacculated condition of the aneurism and the relative smallness of the opening are essential conditions for a case which is to be looked on as really favorable." Aneurisms of this kind generally (but not always) have a distinct bruit, which may be looked upon as a symptom favorable for electro-puncture.[10] Ciniselli says

that when large vessels issue from an aneurismal sac galvano-puncture is useless. Ciniselli measured the current he used by a voltameter, that is by the amount of gases liberated in the electrolysis of water in a given time (see Fig. 6, p. 819). The current strength would equal by our present mode of measurement about 40 or 50 milliampères; and that would be a very suitable current to employ. The time occupied by each operation has greatly varied in the hands of different surgeons, and has ranged from ten to thirty minutes to an hour or longer.

From the physical laws of electrolysis which have been previously given, and from the experience already obtained, it would seem that it would be best to employ needles attached only to the positive pole of the battery, for it is with this pole that we obtain a more dense and hard coagulum. The needles should be insulated to within a short distance of their points and inserted so that all the uninsulated part is within the aneurism. A weak current should be used at first and gradually increased without any break, so as to avoid as much as possible a shock being given to the heart. Internal aneurisms are sufficiently near the heart for that organ to be powerfully influenced by any current that may be passed through them.

It is an advantage that the needles should be made of steel, for, in addition to the coagulating effect of the positive pole, the needles themselves become dissolved, and we have the additional coagulating effect of the oxide and chloride of iron.

It would seem advisable that the first operation should not be prolonged for more than twenty minutes, and if the patient bore it well, subsequent applications might last from half an hour to an hour. Intervals of six or eight days should be allowed between each application of the current. When the needles are used attached only to the positive pole, a large surface electrode attached to the negative pole should be placed on some indifferent part of the body, a current strength sufficient to decompose the steel needles can then be obtained without any injury to the skin. The author has employed an external electrode moistened in warm salt water, measuring nine inches by twelve.

If needles attached to both poles are thrust into the aneurism, a sufficient current strength can be obtained with a very

much smaller number of cells. Of all the constituents of the human body blood is the best conductor of electricity. It has been estimated that blood in an aneurismal sac offers a resistance of about 8 ohms (Bartholow).

With needles in an aneurism attached to both poles the resistance is thereby very greatly reduced, a double clot is formed, and coagulation takes place much more rapidly. This method of procedure is therefore often adopted. Great care should be taken that needles attached to opposite poles do not touch, otherwise the current would be short-circuited. The insulating substance on the negative needles is often partially destroyed by the alkalies set free at that pole. The number of needles to be used depends on the aneurism. It is generally sufficient to have two or three connected with each pole.

In the case of cirsoid aneurisms three or four needles can be used, each one in a different convolution of the enlarged vessel and all attached to the positive pole.

Coagulation takes place round the needles in the shape of a cone; around the positive needles the base of the clot is about half an inch in diameter, and around the negative needle about three-quarters of an inch. The negative clot is frothy and alkaline; the positive clot smaller, firmer, darker in color, and has an acid reaction. Albumin, fibrin, fat, acids, and chlorine are separated at the positive pole; watery and alcoholic extracts, alkaline and earthy bases, iron and coloring matter appear at the negative pole (Heidenreich). The gas which is produced in the electrolysis of aneurisms is in too small bubbles to be troublesome. No occurrence of embolism has ever been reported as following the operation. The removal of the needles requires some care, as those connected to the positive pole become fixed in the clot formed around them. An attempt should be made to gradually disengage and withdraw them by employing a semi-rotatory movement; should this not be successful, the current should be reduced to zero, and then the commutator reversed and the current increased again and allowed to flow for a few minutes with the positive needles made negative. This soon causes their disengagement from the clot and facilitates their withdrawal, at the same time it lessens the chances of hemorrhage from those needles which, during the course of the operation, have been negative by making them positive for the last few

minutes, and thus coagulating more firmly the blood in the neighborhood of their points of exit. Bleeding is more likely to follow the removal of the negative needles, or the positive needles if suddenly pulled out. Should any hemorrhage occur, it is only necessary to cover the orifices of exit by small pieces of lint or cotton-wool with or without collodion.

It has not been found necessary by most operators to anæsthetize their patients; such a necessity would materially add to the difficulties of the operation if the current was employed for an hour at a time. To allay apprehension a hypodermic injection of a quarter of a grain of morphia has been used a short time before the operation; and to reduce the sensitiveness of the skin to the pain caused by the puncture of the needles it has been previously frozen by ether spray or by the application of ice. With some patients and with some aneurisms, especially cirsoid aneurisms, an anæsthetic is always found necessary.

The performance of electrolysis often gives great relief to pain and reduces the pulsation and thrill; it sometimes also retards the bursting of the aneurism. Dr. Ralfe, of the London Hospital, reported a case of aneurism, treated by electrolysis, before the Pathological Society in November of last year (1888), in which these results were obtained. The relief experienced by the patient was so great that he asked for a repetition of the operation. In all it was performed thirteen times, and the man died subsequently from exhaustion from the gradual oozing of blood through one of the punctures. Sudden rupture was avoided by the increased thickness of the walls of the aneurism caused by the electrolysis.

The advantages of the electrolysis of aneurisms may be summed up as follows: it relieves symptom, checks the growth of the tumor, prolongs life, and sometimes ends in cure.

The introduction of soft iron wire into the sac of an aneurism would probably be a great additional advantage to the ordinary treatment by galvano-puncture. The author has suggested [11] such a modification of Moore's method of treating aneurisms, which consists of passing a large quantity of steel wire into the aneurismal sac and by this means producing coagulation. In the modified operation it is suggested "that the wire used should be of soft iron instead of steel, of No. 29 gauge according to the Birmingham scale. That the

introduction should be accomplished by mean of a perforated needle fitted to a handle in a similar way to that used by Mr. T. Smith in the operation of staphyloraphy. That the wire should not, as in the latter instance, be previously wound upon a reel or bobbin, but should be paid out from a tolerably large coil or skein so as to reduce the chance of kinking. The amount of wire to be used would vary in accordance with the size of the aneurism. That the perforated needle should be a straight one made of ivory (or vulcanite)[12] and much longer than those ordinarily employed for cleft palate. That the needle should be introduced to such a depth that the point would be well within the aneurism. When sufficient wire has been introduced, it should be cut off near the hank or coil from which it was paid out, the handle detached from the needle, and the needle left in the aneurism with the end of wire protruding from it. That the wire should then be connected with the positive pole of a strong galvanic battery, a sponge, or tin and amadou plate electrode, moistened in salt and water, should be attached to the negative pole and placed on some indifferent part of the body. That the circuit should be closed and a current of from fifteen to twenty milliampères passed through the aneurism until it became consolidated; or for thirty minutes. The pad connected with the negative pole should be moved to different parts of the body when found necessary, so as to avoid any particular spot becoming blistered or sore. That at the conclusion of the operation the remaining piece of iron wire protruding from the needle should, if not already detached by the decomposition which had taken place in the sac, be pushed through the perforated needle by a blunt trocar into the aneurism, and the small puncture hole closed by a piece of strapping. By the wire being attached to the positive pole of a battery coagulation would be encouraged within the aneurism, and the wire itself would undergo decomposition, with the formation of chloride and oxide of iron, which would in addition exercise their own specific coagulating properties.

The clot resulting from electrolysis is distinctly softer than an ordinary clot. This is no doubt partly due to its rapid formation, and it would probably shrink and consolidate and possibly become as firm as any other clot when of a similar age. In the suggested operation the clot produced by electrolysis would have a long and extensive origin and be entangled at

innumerable points by the coils of the iron wire. Another probable reason why the coagulum formed by electrolysis "appears very frequently not to be stable, but to be liable to melt down again into the blood stream,"[13] is because needles attached to both poles, or to the negative pole only, have been used. Around the negative pole the blood is coagulated into a yellow friable alkaline mass composed to a great extent of gas bubbles, which very quickly breaks down. It would seem probable that a combination of Moore's treatment of aneurism with that by galvano-puncture, as here sketched out, would offer a very fair chance of a successful result.

For some forms of aneurism electrolysis is perhaps the most applicable mode of treatment. I allude to cirsoid aneurisms, aneurisms by anastomosis, those large tumors composed of cavernous tissue fed by large arteries and emptying

Fig. 7.

themselves into dilated veins, and traumatic arterio-venous aneurisms.

In a recent paper "On the Value of Electrolysis in Angioma and Goitre,"[14] Dr. John Duncan, of Edinburgh, writes: "I have no hesitation in saying that the only justifiable method of treatment for cirsoid aneurism is electrolysis. Its perfect safety and reliability ought to determine its use, even though the tumor be not enlarged."

In the case of a cirsoid aneurism of the left temporal artery operated upon by the author, the patient was made to lie upon a large surface electrode, measuring twelve inches by nine (Fig. 7), covering the greater part of the back between the shoulders. This electrode was attached to the negative pole of a Stöhrer's battery. The patient was placed under the influence of an anæsthetic and four insulated needles attached to the positive pole were thrust into different convolutions of the diseased vessel. The uninsulated part of the needles was buried in the artery. A strong current of from 150

to 220 milliampères was passed for fifteen minutes. The needles were greatly corroded, and on one occasion the destruction of the metal was so great that about a quarter of an inch of one of the needles was left in the aneurism. The operation was repeated three times, when the aneurism was found to be consolidated. Some of the large trunks forming the aneurism had been previously ligatured, but this had not succeeded in arresting the pulsation.

Electrolysis can also be used for the occlusion of varicose veins. Needles attached to the positive pole only should be inserted, because the clots formed by them are firmer, and embolism less likely to follow.

Varicocele may also be treated in the same way. The mode of procedure in both cases in the same as that described for the treatment of aneurism.

CHAPTER III.

NÆVI.[15]

One of the most frequent applications of electricity to surgery is in the treatment of nævi. Nævi are treated in many different ways, such as subcutaneous ligature, setons, injections of perchloride of iron, caustics, the actual cautery, excision, and electrolysis. Nearly every surgeon has his own method, which he prefers, but the employment of electrolysis is becoming more and more prevalent. In some situations, no doubt, electrolysis gives the best results, especially where it is desirable that the subsequent scarring shall be as little noticeable as possible, but it also possesses certain other advantages over most of the modes of treatment. Except in very vascular nævi, electrolysis can be performed without causing bleeding. The action of the current can be better and more easily localized than that of acids or caustic alkalies, because the action ceases directly the current is broken, and no caustic material is left behind to consume the tissues until it is neutralized. The cauterization produced by electrolysis is only proportionate to the strength of the current, and is in consequence of the current. Twenty-three parts of nascent sodium are produced at the negative pole for every sixty-five parts of zinc consumed in the battery; and sixteen parts of oxygen are produced at the positive pole. The sodium at once forms caustic soda at the negative pole, which combines with the tissues, and hydrogen is liberated. The oxygen oxidizes an amount of tissue in proportion to the quantity that is disengaged, therefore the caustic effects of the battery can be limited at will, which is not the case with more or less fluid chemical caustics. Electrolysis is not so prone to cause constitutional disturbance as the seton and ligature, and it is less painful, the pain ceasing immediately the current is broken. This is a most remarkable fact, and is no doubt due to the destruction of the ends of the nerves, which is not the case

when a nævus is excised. This freedom from after-pain would alone recommend the operation by electrolysis to mothers who have to nurse their children afterward; and as a rule less scarring is left after the use of electricity than after any other method, but the treatment by electrolysis is more tedious, requires more skill, and is more expensive to the operator; therefore, in parts where a scar is of very little importance, ablation may be performed, and is much more expeditious and less troublesome to the surgeon, though causing more after-pain to the patient.

Nævi may be divided into three classes: Cutaneous, subcutaneous, and mixed. Some will remain stationary or may disappear spontaneously, while others have a tendency to grow. John Duncan, of Edinburgh, recommends that nævi the size of a pin's head on the face should be destroyed, as they are very persistent, and will sometimes grow. When small they can be removed with the least appreciable amount of pain, and in about the space of half a minute, leaving no mark. Those larger nævi, which persist after puberty, are not likely to disappear spontaneously. Every growing nævus should be attacked at once, as the difficulty of dealing with them increases with their size. It is only possible to destroy nævi by galvano-cautery or electrolysis without leaving a scar when they are entirely subcutaneous. When this is the case the nævoid tissue can be coagulated, and gradually absorbed and obliterated without injury to the superficial skin. Subcutaneous nævi of the cheek or lips, and in other favorable situations, can be attacked through the mucous membrane from the inside. Nævi of the mixed varity, cutaneous nævi, and port-wine stains cannot be removed without leaving some scar, for the discolored and diseased skin has to be destroyed.

Needles may be made of steel, platinum, or gold, but it must be remembered that when attached to the positive pole the metal itself is also decomposed. This may be an advantage in very vascular nævi if steel needles are used, for a chloride of iron is formed which assists in the coagulating process. Gold needles used under these circumstances only become permanently discolored; it is best, therefore, to reserve them for use with the negative pole. Platinum of all metals is the least affected by electricity, and needles made of this metal are therefore most usually employed. The action of the

current appears to extend for a greater distance around platinum needles, and they do not become so tightly fixed in the tissues as the steel needles do. Steel takes the best point, and

Fig. 8.

the needles made of this metal are very frequently spear-shaped (Fig. 8). The trocar-pointed needle is the easiest to

Fig. 9 *a*.

Fig. 9 *b*.

insert, and is usually the shape in which the gold needles are made. Of all three metals platinum takes the worst point, and these needles are usually made with a sharp rounded point, like a pin (Fig. 9 *a*).

Dr. Althaus has suggested the use of several fine gilt needles fixed together like the prongs of a fork (Fig. 10, No. 3), and these are sometimes employed; but most frequently separate needles are used. If more than one are used with either pole,

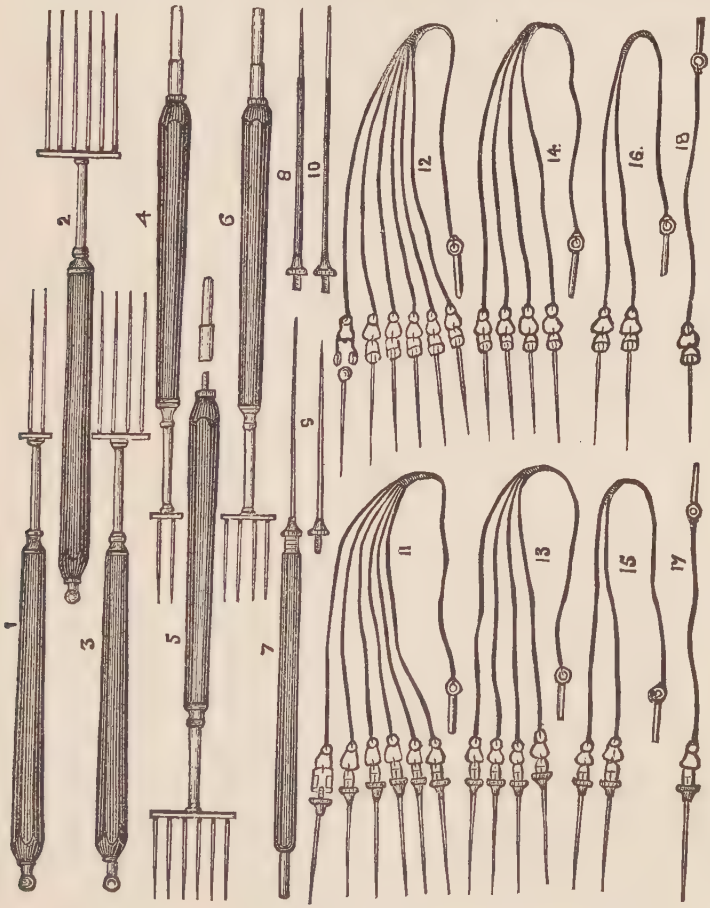


FIG. 10.

they are attached to it by a *serres-fines* conductor (Fig. 10 Nos. 11, 13, etc.), which is a rheophore containing several strands of wire, united at the battery end, but divided usually into six tails at the other end, to which the needles are attached by small clamps or wire holders (Figs. 11 and 12). The needles are bare at the end connected to the rheophore,

but insulated in their stems up to within a variable distance of their points. When a nævus has to be treated, needles are chosen with uninsulated points of a length to correspond with its size. The uninsulated ends must be completely buried in the nævus, especially with the subcutaneous variety, otherwise the skin will be destroyed around the point of entrance of each needle. Different materials are used for insulation, such as shellac, gum elastic, and vulcanite. An operator can coat his own needles with shellac, but it very quickly comes off. The number of needles to be used, and the pole to which they should be attached, depend upon the size and situation of the nævus. As a rule, platinum needles should be used with the positive pole, except in the case of very vascular nævi before alluded to; but it does not matter of what material the needles are made when used with the negative pole; gold needles are often employed to distinguish them from those

Fig. 11.

Fig. 12.

which have been made positive. If the nævus is situated on a conspicuous part of the body, such as the face, where the smallest possible amount of scarring is desirable, it is best to use platinum needles attached only to the positive pole. A moistened sponge or carbon electrode covered with chamois leather, or a moistened pad attached to the negative pole, is applied to some indifferent part of the body, such as the back, arm, or leg, and a current is passed of about seventy milliampères for the space of ten minutes. At about the end of five minutes, if the needles have not been inserted very deeply, the tissues around them become blackened, and the needles may be moved and pushed into another part of the growth. It is often possible to use the same place of entrance by pushing the needle in another direction. This is often desirable, as it is sometimes impossible to prevent some mark remaining at the point of entrance of the needles. When large mixed nævi are to be treated, on parts of the body where scarring is not of so much consequence, needles may be used attached to the negative or both poles. The action is then very greatly in-

creased in rapidity. A current strength of 100 milliampères may be used, and the position of the needles frequently changed. A small nævus may be destroyed with needles attached to both poles in two, three, or five minutes, according to its size. If the current strength is increased to 200 milliampères, great heat is evolved from the rapid chemical decomposition that takes place, and if the negative needles be kept too long in the same spot a slough is produced. The effects are not due to the development of heat, but to chemical decomposition. If the needles are moved at the proper time, a dry blackened surface is present where the positive needles have been used, and an emphysematous discolored swelling at the negative needles, with the surrounding skin greatly congested. If a needle slips out, some bleeding may sometimes occur. This may usually be arrested by inserting the needle again, and increasing the strength of the current; the negative pole appears to be as efficacious for this purpose as the positive. The growth of a nævus may often be more thoroughly arrested by passing the needles into some of the larger vessels or blood spaces, but of course the liability to hemorrhage is increased. A small compress of lint may be placed over the point of the needle as it is withdrawn, and kept pressed to the puncture by another compress and bandage, until all likelihood of bleeding has ceased. The circulation through a nævus is often thus materially obstructed and its vitality impaired. Needles attached to the positive pole sometimes stick, and are with difficulty removed, so that on withdrawal bleeding takes place, but if the needles are previously loosened by twisting them round, and then quietly and slowly removed, this chance of hemorrhage is reduced unless the tumor is very vascular.

The electrolysis of a nævus should be stopped when every bright red or morbid part has changed color. It may be that some few vessels escape destruction, and after the greater part of the destroyed surface has healed these veins may recommence growing, and require another operation. No untoward circumstances as a rule accompany the electrolysis of nævi, except perhaps the destruction of more tissue than is necessary by the current being used too strong or for too long a time. It is very difficult to estimate the amount of action necessary, so that the blood in the vessels may be obstructed,

and the viability of the nævus destroyed, and that the whole mass may not slough. It is wonderful how nature will repair parts that appear to have been thoroughly destroyed. Often, if the first scab is knocked off too soon, a deep excavated sore will be seen, which sometimes greatly alarms the child's parents. I have seen the scalp destroyed to the bone over an area the size of a sixpence, and yet for it to fill up and skin over, so that a very small visible scar was left. The whole septum or tip of the nose has appeared sometimes to be destroyed, but nature has replaced them accurately. The tip of the nose is a very common place for a nævus to occur, and it is difficult to treat. It is better to do too little than too much. The operation can be repeated, but a part may be destroyed beyond repair. Often, after part of a nævus has been destroyed, other vessels which have escaped may become strangled and obliterated by the contraction which takes place during the process of healing.

An anæsthetic is necessary for a nævus of any size, for the operation while it lasts is acutely painful, and is sometimes prolonged for eight or ten minutes. Alarming pallor may occur during the electrolysis of nævi on the head. This is much aggravated when the child is under the influence of chloroform. Therefore the current has to be put on very gradually. This collapse and faintness is produced directly the circuit is completed, and is possibly due to the patient not being sufficiently under the influence of the anæsthetic, and therefore suffers from shock; but I should not advocate, in view of this theory being probably correct, that a patient should be profoundly anæsthetized, for if the current still produced its characteristic effect under these circumstances, the result might be fatal. It is best to increase the strength of the current gradually, and at the same time, as prudence dictates, to push the anæsthetic. The younger the infant the more grave the symptoms. The pulse can be felt immediately as influenced, and sometimes becomes almost imperceptible. The breath, which before the circuit is closed was normal, becomes crowing and shallow, showing that the pneumogastric, and especially its recurrent laryngeal branch, is profoundly affected. A very favorite place for nævi is at the vertex, just over the anterior fontanelle. The current is therefore passed direct through the brain. When a nævus is in that situation,

it is also essential not to get suppuration, on account of the underlying meninges; therefore, it is always preferable to use needles attached only to the positive pole. This greatly prolongs the operation, and often necessitates a second, and even a third, application of the electricity, which also increases the risk. A nævus, therefore, over the anterior fontanelle is not trated by electrolysis without some danger. The same may be said of any other form of treatment that may be adopted.

With large nævi the operation by electrolysis is extremely tedious and troublesome, but with perseverance and patience the largest and worst may in time be cured, with more or less resulting scar, according to the skill of the operator. For some pendulous nævi, or for certain parts of nævi, galvano-cautery is sometimes preferable to electrolysis. The number of times it is necessary to operate depends upon the size of the nævus, and the effect produced by each operation. A month or six weeks may intervene between one application of the current and the next, except in the case of very large nævi, in which a distant part may be attacked after the lapse of a fortnight, so as to let the part first operated upon continue the process of healing undisturbed.

Nævi situated on the dorsum of the tongue can best be removed by galvano-cautery. One or two curved needles are thrust through the base of the nævus so that their ends extrude on either side of the growth. The platinum wire of the galvano-cautery instrument is placed beneath the ends of the needles and so as to include the whole growth, and, when raised to a dull red heat, is gradually drawn through the tissue of the tongue just beneath the growth and the transfixing needles so as to remove them both together. A seared dry surface is left on the tongue, which does not bleed if the operation has not been performed too rapidly; and this burnt surface gradually heals.

Port-wine stains and superficial moles can also be treated by destroying a small portion of them at a time, when the mark is not small enough for the whole to be attacked at one sitting. Some port-wine stains have little nodular excrescences upon them which can be previously removed by the galvano-cautery. I have thus removed two or three little prominent points on a port-wine stain at the same sitting. When healed, these spots are covered by small islets of healthy skin.

For the flat part of the port-wine stain or mole I use electrodes suggested to me by Mr. R. W. Parker, Surgeon to the Grosvenor Hospital for Women and Children. They consist of metal plates of various sizes, covered with platinum foil. The different sizes are used so as to fit on to different portions of the undestroyed discolored skin (Fig. 14). No part of the metal plate is allowed to touch healthy skin. They are used with the negative pole of the battery, a pad attached to the opposite pole being placed on some indifferent part of the body. The surface of the port-wine stain beneath the metal plate is moistened with salt water, and a current of about forty to fifty milliampères allowed to pass until the skin is destroyed to a sufficient depth. The patient must be under chloroform. A frothy, slimy débris soon collects under the metal plate, and if attention is not paid to the electrode it is likely to slip off

Fig. 13.

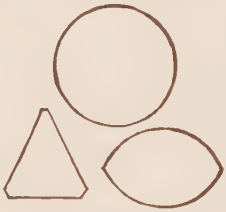


Fig. 14.

the part being operated upon, and more or less destroy neighboring skin. The part of the stain that has been destroyed subsequently becomes a dry scab, and comes off, leaving beneath it a layer of more or less healthy skin. The process can then be repeated, usually after an interval of two or three weeks, on a fresh part of the stain. I have treated hairy moles in this way, and when the skin has been destroyed to the depth of the follicles from which the hairs grow, all the hairs will come out with the greatest ease. If, after the scab has separated, a few hairs still remain, they can be removed by the process about to be described.

The Removal of Superfluous Hair.

The removal of superfluous hair from the face, and sometimes from other parts, or of little hairy warts, and other disfiguring marks, is often of the greatest importance to ladies.

Many are the nostrums that have been sold for the purpose of effecting this desirable object, and much time has been fruitlessly wasted, sometimes every day, in removing by the tweezers these obstinate and ever-returning hairs; each time they grow again stiffer and longer than before. The only effectual way of permanently removing these hairs and marks is by electrolysis.[16] It is best that the patient should be lying down in a good light. A pad soaked in salt water, and connected with the positive pole of a battery, is placed beneath the collar. Each hair as it is to be extracted is seized by a depilatory forceps, and a needle, usually made of platinum (Fig. 15), mounted on a suitable handle, is thrust down in the follicle beside the hair, as far as the papilla from which the hair grows. The depth of the follicle depends upon the hair, and is deeper for large hairs than for small ones. It will be found generally necessary to insert the needle from one-sixteenth to an eighth of an inch. The circuit is then closed; this can best be done by an arrangement on the handle holding the needle (Fig. 17), and a current of about five milliampères is passed for twenty or thirty seconds. The hair all the time is held just tight by the forceps. When the papilla is dissolved away the hair will slowly slip out without any extra force being used. The exact time occupied by the process depends upon whether the needle is close to the papilla or not. During the time the current is passing, a small amount of froth is seen to rise up by the side of the hair. A slight bump, like a flea-bite, remains at the spot from which the hair has been removed for about two hours, and sometimes a little speck will remain for a week or ten days; this then falls off, and no mark remains. It is best only to remove the longest and darkest hairs, and those chosen should not be too close together. About twenty or thirty hairs are sufficient to remove from a lady's face at each sitting. The operation may be repeated at the end of every ten days or a fortnight until all the hairs have been removed that cause disfigurement. The number of times that it is necessary to perform the operation depends upon the abundance of the growth. No anæsthetic is required, as the process is not a very painful one. A slight twinge is experienced at the removal of each hair, such as would be caused by a spark from the fire alighting on the skin. With the greatest amount of care it will be found that one or two hairs es-

cape total destruction, and may return; but these can be removed on a subsequent occasion. It is said that the sensitiveness of the skin may be reduced by painting it over with a five-per-cent solution of cocaine. I have never really found this necessary. I have operated on a large number of ladies,

Fig. 15.

Fig. 16.

Fig. 17.

Fig. 18.

and none of them have complained of the pain being more than trifling. It ceases immediately each hair is removed.

The operation is sometimes required in men to remove the hair that occasionally grows on the bridge of the nose, or there may be a bushy growth between the eyebrows, with hairs of such a length as to occasionally get into the line of

sight. In-growing eyelashes (trichiasis) can also be effectually removed by this method.

A wart, or a small hairy tuft or mole, having a dozen or sixteen hairs, can be removed at one sitting.

Keloid scars after the healing of suppurated strumous glands or cicatricial tissue about the junction of healed wounds following the removal of the breast, etc., have been removed by electrolysis. A needle connected with the negative pole has been thrust into the nodular mass and a current of about 50 to 70 milliampères allowed to flow for two or three minutes or until the hardened mass has softened down. Several of these keloid masses, or as many as exist, can be attacked on the same day.

Small tumors and growths can be treated in the same way. More than one needle may be required, according to the size of the tumor. When several needles are used, they are thrust into the base of the tumor in a radiate form. Where the sloughing out of the tumor is not objectionable, some of the needles may be attached to either pole. In the case of a small tumor, for instance, on the upper lip, where as little disfigurement as possible is desired, needles should be used attached only to the positive pole. When this is the case, the operation will take a much longer time. The current should be arrested when an obvious change has taken place in the tumor. An anæsthetic in these cases would always be necessary.

Sebaceous cysts of the head and cystic tumors of the eyelid may be attacked in this way.

Lupoid ulcerations of the face, and serpiginous ulcerations on the hand or other parts, may be destroyed by electrolysis and a healthy base to the sore produced which will ultimately heal.

CHAPTER IV.

STRICTURES.

THE same electrical action has been used in the treatment of abnormally narrowed and strictured passages of the body, such as stricture of the urethra, rectum, and œsophagus, stenosis of the os uteri, obstruction of the lachrymal canals and of the Eustachian tube.

STRICTURE OF THE URETHRA.

In 1839 Crussel began his investigations on the use of electrolysis in surgery and appears to have been the first who

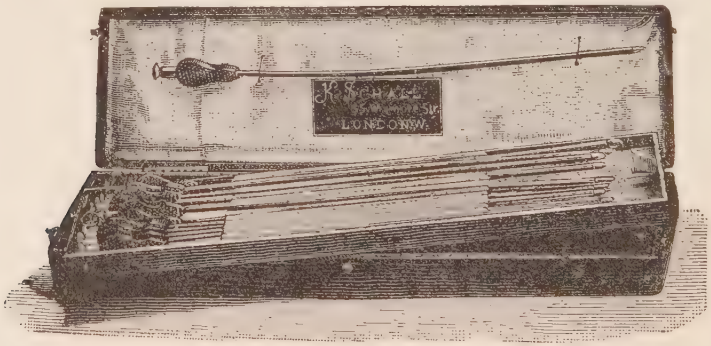


FIG. 19.—Case of Urethral Electrodes.

suggested its use in the treatment of stricture of the urethra. The method was revived in 1867 by Mallez and Tripier in Paris, and about the same time by Althaus in this country. During the last decade it has been developed and improved by Dr. Robert Newman, of New York, to such an extent that it has now become one of the recognized modes of treatment of stricture.

Soon after my appointment to the Electrical Department at St. Bartholomew's Hospital in 1882, my attention was

called by Dr. Shelly, of Hertford, to the success achieved in America by this mode of treatment. I then had electrodes made for the purpose of carrying out the treatment, but had no patients to try them on until I obtained the assistance of Mr. Bruce Clarke in the autumn of 1885. Since then we have had numerous cases which have established beyond a doubt that electrolysis is one of the most efficient and satisfactory modes of treating stricture of the urethra.

In the treatment of stricture of the urethra by electrolysis [17] the best form of electrode is a catheter-shaped gum-elastic bougie, having an olivary-shaped metal end (see Fig. 20, B) connected by a copper wire (seen at C) with a Brodie's handle, on which is a binding screw (A). A common stem has been

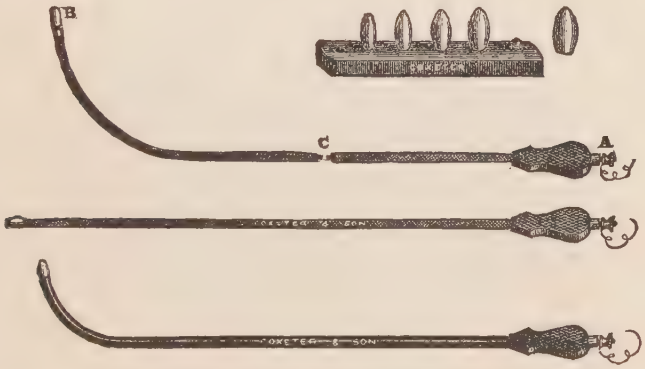


Fig. 20.

made so as to carry different-sized olivary ends, as seen in the diagram. These electrodes, consisting of an insulated copper wire, are as flexible as ordinary gum-elastic catheters, and can be bent to any curve that will suit the operator; but a set of firm electrodes are also made, which can be directed as accurately as a silver catheter; these latter are pictured in the bottom figure of the diagram. The olivary ends in all these electrodes are nickel-plated, there is no necessity to employ platinum, as they are always used attached to the negative pole of the battery, and therefore not affected by the current. In one or two cases so little sensation was produced in the patient by the passage of the current that it was difficult for him to believe that any decomposition of his stricture was taking place. The current was therefore once or twice re-

versed, by means of a commutator, with the result of immediately removing the plating from the electrodes. The shock of making and breaking the current soon satisfied the patient that some uncommon force was present in his urethra.

An electrode attached to the positive pole is placed under the patient's back, or on some other indifferent part of the body. The best electrode for the purpose is one made of tin, on to which is stitched some amadou or house flannel, for the purpose of retaining moisture (Fig. 21). This pad is moistened in salt and water, and then placed in a cover, one side of which is made of waterproof, so as to protect the bed-clothes or couch. It is best to have the patient lying on his back.

The position of the stricture is first ascertained by the passage of an ordinary bougie, on which a mark is placed to show

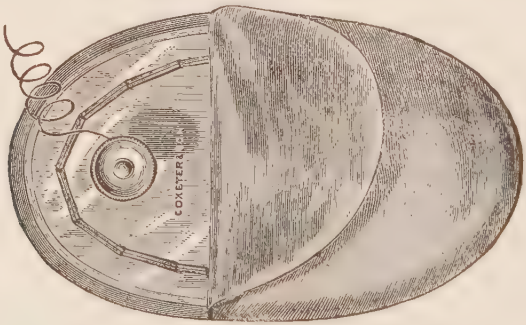


Fig. 21.

the distance from the meatus. A similar mark may be made on the electrode it is intended to use, so as to make certain when the metal end is in contact with the stricture. An electrode is used which is one size larger than the calibre of the stricture will admit. If no instrument can be made to pass the stricture, then it is best to use an electrode of about No. 5 or 6 English scale. In these cases two or three applications of the current may be necessary before the electrode will pass the stricture. The most rapidly successful cases are of course those which could be treated by ordinary dilatation. It is perhaps an unfairly severe trial of the method to select only those cases which resist all other forms of treatment. They are also of course the most difficult to treat by electrolysis. When the electrode it is intended to use is in position against the stricture, it is attached to the negative pole of the battery

by a wire from the screw (A) on the handle. The circuit is then closed and the strength of the current gradually increased to five or six milliampères. A current of greater strength will produce pain. To ascertain the strength of the current it is necessary to include a galvanometer in the circuit, either between the negative pole and the bougie electrode or between the positive pole and the pad on the back. Sufficient accuracy cannot be attained by simply considering the number of cells employed, as cells vary in strength according to their composition and the length of time they have been in use. No anæsthetic is required; in fact, it is an advantage that the patient should be conscious, so that he can say if the current appears to be too strong. The electrode is kept gently pressed against the stricture, no force is to be used, but the electricity allowed to do the work. If the electrode is forced through the stricture the operation amounts to little more than ordinary rapid dilatation. The attention must be kept continually on the electrode so as to guide it in the right direction, otherwise a false passage may be dissolved into the side of the urethra. Immediately the electrode passes the stricture the current should be turned off and the bougie withdrawn. The time occupied in dissolving the stricture varies according to its density and length, but is usually from ten to five-and-twenty minutes. If the electrode has not passed the stricture before the expiration of twenty-five minutes, it is best to turn the current off and continue the treatment at some later period. Even if the electrode has not passed, great benefit is often derived from the use of the current; the difficulty in passing the water is relieved and the size of the stream enlarged. When the electrode is withdrawn the patient is left undisturbed for a week or ten days, sometimes for a fortnight. It is then found that a bougie larger than the electrode which passed on the last occasion will now pass the stricture. If the French scale is used, two sizes larger will often be found to pass; taking the English scale, about one size larger can be used each time. The operation is then repeated with the larger-sized electrode. The number of times it is necessary to perform the operation depends greatly on the calibre of the stricture when the treatment was commenced. The great success achieved by Dr. Newman, of New York, is mainly due to the employment of weak currents and the frequent repetition of the operation.

There is no comparison between the treatment of stricture of the urethra by the ordinary methods and its treatment by electrolysis. Should the permanency of the good results prove to be as a rule not so great as those recorded, still the calibre of the stricture remains enlarged for a longer space of time than after any other form of treatment.

In the "New England Medical Monthly" for December last an editorial article was published under the heading "What is the Present Status of Electrolysis in the Treatment of Urethral Stricture?" The article says: "Not long ago physicians and surgeons of repute flouted the treatment of urethral strictures by electrolysis. Now it is so generally and successfully practised that scarcely any one opposes it.

"This change of opinion is undoubtedly due, first, to the better understanding of the electrolytic treatment as distinguished from the 'galvano-caustique'; secondly, to the successful treatment, without relapse, of a large number of cases (fully reported) by many physicians of high repute.

"It is undeniable that the method now adopted was first grasped and put forward by Dr. Robert Newman, of New York, who, despite the misrepresentations and abuse of the ignorant, has zealously labored for eighteen years to perfect the instruments used and the technique of the operation, until by extraordinary success the most sceptical are convinced.

"Experiments in the treatment of stricture with electricity have been made since 1847, and until 1872 without any method, except such as destroyed tissues by too strong currents. Mallez and Tripier called their method 'galvano-caustique,' showing that they used a current with caustic, not electrolytic action, and therefore they naturally failed.

"The present method is electrolysis, with weak currents applied at long intervals, resulting in galvano-chemical absorption, known and recognized as Newman's method. Newman not only introduced and perfected the electrolytic method, but also invented and perfected instruments for use in the operations, so that failure in the operation is hardly possible.

"In England eminent surgeons so fully comprehend and acknowledge the great value of this method, that it is taught at the medical schools as one of the ways of treatment of urethral strictures. In St. Bartholomew's Hospital an additional department has been established for treatment in this

way [the Electrical Department at St. Bartholomew's has not been established solely for the treatment of strictures.—W. E. S.], and many successful cases have been reported by Dr. W. E. Steavenson and Mr. W. Bruce Clarke.

"The appended bibliography of articles, reports, and lectures which have been published, will prove of value to the student and practitioner who is interested in this subject—and who is not?[18]

Stricture of the Rectum.

Stricture of the rectum can, like all other strictures, be treated by electricity. The amount of success achieved by this means depends upon the nature of the obstruction. In some cases cure can be effected, in others relief only can be obtained;

Fig. 22.

but in these latter cases, where the stricture is due to cancer, life can be prolonged for a variable period, and made more endurable by the relief of pain; and that last and terrible expedient of colotomy can be postponed and perhaps dispensed with altogether.

The steps of the operation for the electrolysis of stricture of the rectum are as follows:[19]—A galvanic battery of from 20 to 30 cells is required. The number of cells to be employed depends upon the composition of the battery, and the length of time it has been in use. The current strength that it is most advantageous to maintain is one of five milliampères, which can be gauged by means of a galvanometer. A pad moistened in salt and water is connected by a conducting wire with the positive pole of the battery, and placed on some indifferent part of the body, and a specially-made bougie electrode (Fig. 22) is connected with the negative pole, and held against the stricture. The bougie electrodes are made in sizes

corresponding to the ordinary rectal bougies (Fig. 23). It is first necessary to ascertain, by means of an ordinary bougie, what is the calibre of the stricture. An electrode bougie of one size larger is then taken and attached to the battery as already described, and held against the stricture until it has dissolved away enough to enable it to pass. The circuit is then broken and the bougie withdrawn. The patient is left ten days or a fortnight before any further examination is made. It is then generally discovered that a rectal bougie two sizes larger than the one originally used will pass the stricture. An electric bougie of a larger size is then again taken and the operation repeated. The number of times that this process is necessary, depends upon the size of the stricture at the commencement. If on each occasion the electric bougie

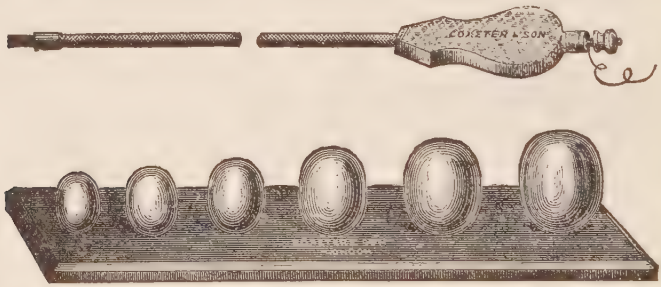


Fig. 23.

does not pass in about twenty minutes or half an hour, it is best to desist, as too large a slough may be produced at the seat of the stricture; but after these apparent failures, it will be found, at the end of ten days or a fortnight, that a bougie one size larger than that used will pass the stricture easily.

This method of treatment of stricture has these great advantages: The patient is not incapacitated from following his ordinary occupation, provided his general health in other respects is good enough for him to be about. No anæsthetic is required; the pain is very trifling, and the strength of the current is regulated to a great extent by the feelings of the patient. It is therefore necessary that he should be conscious. There is usually no bleeding, if the operation be properly performed, and no necessity for any antiseptic precautions, as the procedure itself is aseptic. In the majority of cases there is no contraction or return of the stricture; but if due to can-

cer, a fresh growth of diseased tissue is very likely to take place, necessitating a recourse to the treatment. Successive applications of electricity are far better than the dernier ressort of colotomy, and may keep the intestine patent as long as the disease allows the patient to live.

Stricture of the Œsophagus.

Stricture of the œsophagus has also been treated by electrolysis with success. A long flexible electrode, like an ordinary œsophageal bougie, is required, to which can be attached olivary metal ends of various sizes as in the case of some of the electrodes used for stricture of the urethra and rectum. The electrode is connected with the negative pole of the battery, that connected with the positive pole being placed on some indifferent part of the body. A current strength of five milliampères is generally employed, and the current allowed to flow for fifteen or twenty minutes, unless the electrode passes the obstruction in a shorter time. It will be found, as in the case of strictures in other parts, that recontraction does not take place so rapidly as after dilatation, and that usually after a week's interval a bougie one size larger can be passed than that used on the former occasion. A perforated electrode has been used which will pass over a celluloid guide so as to diminish the risk of its passing into a pouch by the side of the œsophagus, or of its decomposing laterally too much of the cancerous tissue of a malignant stricture and by this means making an opening into the posterior mediastinum. The guide is sufficiently small to pass through the stricture and thus direct the passage of the electrode. As with other forms of treatment of œsophageal stricture, electrolysis gives more prospect of success with the fibrous variety than with the malignant.

Lachrymal Obstruction.[20]

The great difficulty of effecting anything like a cure for obstinate cases of epiphora due to obstruction in the lachrymal passages is known to all. The usual treatment by astringents, probes, styles, and slitting up the canaliculi are all open to grave objections, and often of little benefit to the patient. Mr. Power, in his lectures at the Royal College of Sur-

geons of London, drew special attention to the anatomical conformation of the lachrymal passages and the great harm that might be generally done to them by the passage of the large probe so often used.

The treatment of obstruction of the puncta and the lachrymal canals by electrolysis is only another example of the great advantages that the employment of electrolysis possesses in the treatment of stenosis and other affections occurring in parts difficult of access. The smaller the passage the greater the difficulty of performing an operation. The application of caustic alkalies in the treatment of stricture of the urethra has been used and abandoned on account of the difficulty experienced in limiting the action of the caustic to the part it is wished to destroy. This objection does not exist in the application of electrolysis, because its action is entirely under the control and direction of the operator. The site of its influence can be limited to the smallest point. The duration and extent of its cauterizing action are also absolutely at the command of the operator, and depend on the strength of the current employed and the time it is allowed to flow.

For the removal by electrolysis of organic material of any description which has caused an obstruction or stricture in any of the passages of the human body, the negative pole is used, because the obstructing material is by its means as it were melted down and dissolved and the passage thus made larger. The electrode which is used, if in the first place it is not very much too large, is enabled to pass the obstruction and can be removed again with ease. If an electrode connected with the positive pole is used it becomes glued to the surface of the passage and cannot be removed without some difficulty. The negative pole has no effect upon the metal of which the electrode is made, and when the electrode is withdrawn it is as bright as when introduced, but at the positive pole the metal is dissolved as well as the tissues, and enters into the new combinations which take place; this, in the case of stenosis or stricture, is undesirable.

The material to be decomposed by electrolysis in stenosis of the lachrymal canals is not very dense and does not offer such a very great resistance, and therefore a battery composed of a comparatively smaller number of cells is sufficient. A galvanometer should be introduced into the circuit for the

purpose of measuring the current, for different batteries are possessed of different electro-motive force; a difference also exists in cells which have been recently charged, or which have been in use for a long time. Two to four milliampères will be found a sufficiently strong current to enlarge any narrowed lachrymal canal. Some patients offer very much more resistance than others; whereas in one patient three or four cells will give the required current strength, in another it will require six or eight cells. This difference in resistance is what is so little understood, but it is the factor which plays, perhaps, the most important part in electro-therapeutics. No reliable information can really be derived from reports of cases in which the number of cells only is given, so much de-

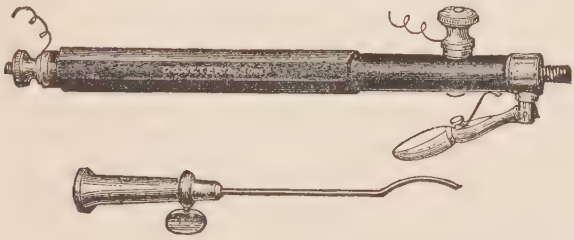


Fig. 24.

pends upon the resistance of the patient and the strength of the cells.

For stenosis of the lachrymal canals a probe small enough to be inserted into the punctum is introduced and passed along the canal to the nasal duct. The probe is fitted into a socket which is screwed on to a handle (Fig. 24), and is attached to the negative pole of a battery. A flat plate electrode (Fig. 21), covered with some substance which will retain moisture, is connected with the positive pole, and generally placed on the back of the neck beneath the collar, having been first moistened with salt and water. The handle of the negative electrode possesses a mechanical arrangement for completing the circuit. Four cells of a Stohrer's battery will usually be found sufficient with the resistance they have to overcome, to give a current strength of four milliampères. A Leclanché battery will do equally as well for such a small operation, but perhaps one or two more cells would have to be used. With a current strength of four milliampères the enlargement of

the canaliculus takes about thirty seconds. A lower current strength would accomplish the object desired, but then the electrode would have to be held in the canal for a longer space of time. No anæsthetic is necessary. A sharp burning sensation is produced in the canal during the time the current is actually passing, but this is usually easily borne by the patient, especially when he is assured beforehand that it will only last for half a minute. During the operation a little froth collects by the side of the electrode and oozes out of the punctum. In a very few seconds after the circuit is closed, the probe, which was first gripped tightly in the canal, can be made to move backward and forward along the passage with great ease. The neighborhood of the punctum, the inner canthus, and the surrounding integument to about an inch in circumference, remain slightly congested and suffused for about two hours after the operation. Care must be taken in all cases in which electricity is applied to the head, owing to its often producing faintness in the patient. The probes used should never be greater than 1.5 mm. During the operation great care must be taken to keep the patient's head fixed, as sudden pain is felt at making and breaking the circuit; the probe must also be kept away from the eyeball.

The advantages attending this treatment by electrolysis are chiefly due to the fact that so little displacement or alteration of the normal channels is effected, and by it we have the means of increasing the lumen of the puncta and canaliculi without any excessive stretching, which must necessarily alter the conditions of the surrounding muscular and other structures.

Nasal Duct.—The same mode of treatment has been tried for closure of the nasal duct. The probe must be insulated nearly to its end, and connected with the negative pole of the battery. A weak current of from about two to four milliampères only should be used. Tripier is said to have operated by electrolysis with success in this affection.

Anterior hypertrophies on the inferior turbinated bones have also been treated by electrolysis; two or three negative needles are thrust into the hypertrophied tissue. Posterior hypertrophies (or adenoid growths) about the orifice of the Eustachian tube are better treated by galvano-cautery, but they can also be removed by electrolysis.

Obstruction of the Eustachian Tube.

The great success which followed the treatment of stricture of the urethra and other mucous passages by electrolysis suggested the idea that an obstructed Eustachian tube might be opened up by the same means. At the latter part of 1887 the author had several bougie electrodes made with the object of carrying this idea into practice.[21] The instrument which was found most suitable consists of a vulcanite Eustachian catheter and an electrical bougie (Fig. 25). The bougie is made of a number of fine copper wires about seven or eight inches long, insulated by vulcanite to within an eighth of an inch of their ends. The ends of the wires are soldered into a small nickel-plated metal cap. The bougie is small enough to pass along the catheter and exceeds it in length by about one

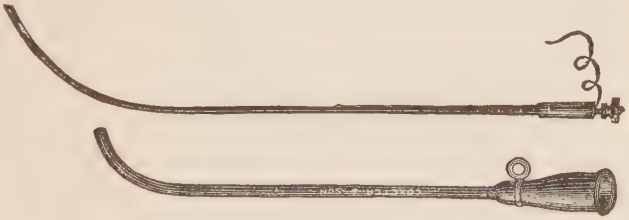


Fig. 25.

inch. The handle end of the bougie is provided with a binding screw, to which the insulated copper wires are also attached, for the purpose of connecting a rheophore from the battery. On this end of the bougie an inch is marked off divided into eighths. Each eighth of the inch passes into the catheter as one eighth protrudes at the other end. It is therefore possible to tell, when the catheter is in the orifice of the Eustachian tube, how much of the bougie is in the canal. On the catheter there is a metal ring, or some other mark, as on all catheters, to indicate the position of its end when it is being inserted.

Electrolysis of the Eustachian tube is performed in much the same way as the electrolysis of the other mucous passages which has previously been described. A pad connected with the positive pole of a battery is moistened and placed at the back of the patient's neck. The Eustachian catheter is then passed along the nostril and guided into the tube; the bougie,

already attached to the negative pole of the battery, is passed along the catheter and Eustachian canal as far as it will go until it meets an obstruction. The current is then closed. A galvanometer should be included in some part of the circuit, and the strength of the current increased until a strength of four milliampères is obtained. A frizzling noise will be heard by the patient in his head, usually likened to the frying of fish, and the operator, by approaching his ear to the catheter, can often hear the crackling produced by the frequent breaking of minute bubbles of gas. The electrolysis is kept up for four minutes, and usually before the expiration of that time, if it is possible that the obstruction can be removed, it will be found that the bougie can be pushed on for a small distance, sometimes for its full length. Generally on the first occasion the Eustachian tube is rather sensitive, but it seems to acquire toleration for the process, and at no time is so much discomfort experienced as might be expected from setting up chemical decomposition in the middle of the head. The operation has now been performed a large number of times without any unpleasant experiences, nor has the treatment caused anything more than very temporary discomfort to the patients. In those cases in which the deafness has been due to a simple obstruction of the Eustachian tube the results have been most encouraging. Mr. Cumberbatch, the Aural Surgeon to St. Bartholomew's Hosital, has written the following concerning the operation: "Our experience is at present too limited to be able to say what cases of chronic catarrh of the middle ear are most likely to be benefited by this new method of treatment. That strictures of the Eustachian tube, which do not yield to the ordinary methods, can be cured by the use of the electric bougie we have proved. In many cases of chronic catarrh with obstruction of the Eustachian tube, there is no actual ankylosis of the ossicular joints; and in such case restoring the patency of the tube, and thus relieving the pressure on the membrana tympani and the chain of ossicles, must act beneficially on the hearing. In cases also where catarrh has spread to the labyrinth, the distressing tinnitus, when due to circulatory disturbance rather than to any lesions of the nervous elements, is likely to be removed. When auditory vertigo is caused by undue pressure on the labyrinth owing to strong retraction of the membrana tympani, it is

possible to relieve it (of course by restoring the patency of the Eustachian tube) by means of the continuous current, as has been proved by several cases which have been thus treated. In conclusion, I may add that, if after three or four trials a patient experiences no benefit, the probability is that further treatment by this method will be useless."

CHAPTER V.

DISEASES OF THE URINARY ORGANS.

THE treatment of stricture of the urethra by electrolysis has been previously discussed when speaking of the treatment of strictures generally.

In the treatment of gleet electricity has been found very successful.[22] It is used somewhat in the same way as medicated bougies. The metal part of a vesical electrode, or elec-

FIG. 26.

trical bougie (Fig. 26) such as is used in the treatment of stricture, is held against the sore unhealed surface which keeps up the discharge and which is frequently to be found on the bladder side of a stricture. The electrode is made negative and is moved slowly backward and forward over the sore surface for about two or three minutes with a current strength of five milliampères. The electrode connected with the positive pole is placed on some indifferent part of the body, but by preference over the lumbar enlargement of the cord, as possibly the effect of the electricity upon the nervous supply of the urethra may be beneficial. In the treatment of gleet no doubt the electrolytic property of the current is the chief agent at work. The unhealthy ulcerated surface on which the gleet depends is decomposed or altered in such a way as to put it into a condition in which it will heal.

Vesical Calculus.

For the last eighty years attempts have been made from time to time to dissolve vesical calculi by electrolysis. Experimentally it was found that all calculi could be dissolved by a galvanic current in a concentrated solution of nitrate of potash. Platinum electrodes were applied on either side of a calculus immersed in this solution, chemical decomposition of the nitrate of potash followed, nitric acid appeared at the positive pole, and caustic potash was formed at the negative. The phosphatic stones were dissolved by the nitric acid; and the uric-acid, and urate of ammonia calculi were dissolved by the caustic potash. Dr. Bence Jones also found that oxalate of lime calculi could be dissolved by the same means; but these attempts have never been successful in the human bladder.

Sir Henry Thompson says,[23] "I must not omit to name the agency of electricity, which has also been locally employed, both for uric-acid and for phosphatic stones. Prévost and Dumas (1823) attempted direct solution of a stone in the bladder by the galvanic current, a plan which was more fully developed by Dr. Bence Jones (1852). The amount of instrumental manipulation, however, necessary to bring the wires into contact with the stone and to maintain them there during the period necessary for its solution, is considerably greater than that required to crush the stone by the modern method of lithotrity, and must therefore be regarded at present as inapplicable."

Sir Geo. H. B. Macleod, in his Address in Surgery before the British Medical Association at Glasgow, in August, 1888, said, "When chemistry had demonstrated the composition of urinary calculi, it was hoped that their solution within the body would be shortly accomplished with the same ease as it was attained in the laboratory. After many and varied attempts in different countries, this has been abandoned, as has also the use of galvanism alone or with chemical solvents. At present this hopeful field of research may be said to remain barren of results."

Enlarged Prostate.

The treatment of enlarged prostate by the means usually employed has on the whole yielded most unsatisfactory re-

sults. Palliative measures frequently prove useless and operative measures disastrous. Mr. Harrison, late of Liverpool, in writing on enlargement of the prostate has said,[24] "how slight is the prostatic barrier in some of these instances, but yet how complete is the obstruction which is thus occasioned both relatively to the act of spontaneous micturition and to the introduction of an instrument to relieve it. When we consider how extremely limited physically is the nature of the obstruction in many of these cases, it is not difficult to understand how readily this state may be remedied by the timely adoption of mechanical means. How easily the mere outlet of the bladder may be improved in some instances of obstruction by a large prostate is shown by the undoubted success which frequently followed the practice of Mercier in making a linear incision through the prostatic bar, and more recently by the means advocated by Gouley, where no more than a small portion of the obstruction is punched out by an ingenious instrument contrived for this purpose." He also says that often in persons with large prostates "the regular expulsion of urine from within outward is capable of preventing obstruction by the formation of grooves or channels in the hypertrophied mass."[25]

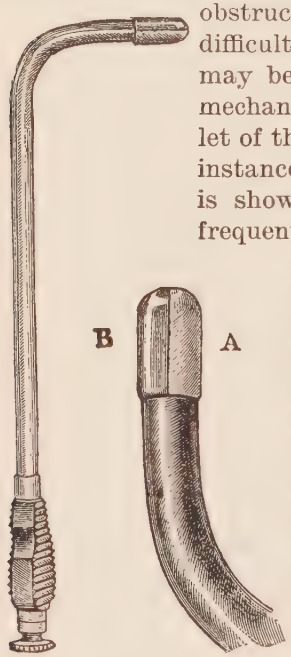


Fig. 27.

This conservative process I propose may be imitated by the means of electrolysis. For this purpose I have had some electrodes made after the manner of those used for the electrolysis of stricture, but instead of the ends being made entirely of metal I have had them made of ivory with phalanges of metal embedded in one side (see B, Fig. 27), so that the metal would be on the convex surface of the ends of the electrodes. By this means we insure that when the electrode has been passed along the urethra the metal phalange shall come in contact with the prostatic bar or obstructing hypertrophied part of the prostate. The electrode is connected with

the negative pole of the battery, the positive pole being placed on some indifferent part of the body. The circuit is then closed and a weak current of five milliampères is employed; by this means a furrow or groove is made on the surface of the enlarged prostate by the action of the current.

I have only had an opportunity for using these instruments in one or two instances, and these not very bad cases, but the results have been such as to encourage the hope that this may prove a very useful form of treatment. The operation should not be prolonged for more than twenty minutes, and on the first few occasions for about half that time unless the electrode passes into the bladder sooner. Should this be the case I would advise that all further attempts be suspended for at least a week, and then repeated if no indication to the contrary had arisen.

The mode of treatment by electrolysis above described is a much less serious procedure than that which has been more frequently tried. The success, such as it is, which has attended the treatment of fibroid tumors of the uterus by electrolysis has led to experiments being tried upon the hypertrophied prostate gland, and galvano-puncture has been performed both from the rectum and urethra.

Dr. Bredert reports five cases of enlarged prostate in which he used electrolysis. He thrust a needle, insulated to within a short distance of its point, into the enlarged gland and attached it to the negative pole of a battery, while an electrode attached to the positive pole was placed on the abdomen. The prostate was rapidly reduced in size; in one case after the third application.

Dr. Casper, before the Berlin Medical Society in April, 1888, read a paper on the radical treatment of hypertrophy of the prostate and prostatic tumors by electrolysis. He reported four cases in which he performed galvano-puncture from the rectum. The bowel was on each occasion previously washed out with a one-per-cent solution of sublimate. After the insulated needle had been thrust into the prostate, at the end of five minutes it was partly withdrawn and then thrust into the gland in another direction, but no fresh hole was made through the rectum. This change in the direction of the needle was made again at the end of the second five minutes. The whole operation lasted fifteen minutes. The cur-

rent strength used was from 10 to 25 milliampères. A slight burning and pricking was felt in the glans penis when the current strength exceeded 12 milliampères. The operation required repetition about twenty times in each case. In one case a permanent recto-vesical fistula was produced, but two of the cases greatly improved under the treatment.

I have operated most successfully from the rectum in the case of a small pedunculated fibroid tumor of the uterus attached to its posterior wall. Per vaginam it felt like a retroverted uterus. The tumor caused obstruction to defecation. The patient could feel it fall back and obstruct the gut when she moved from the prone to the erect position, and also when straining at stool. It became impossible to pass any natural motion and it was necessary to continually take med-

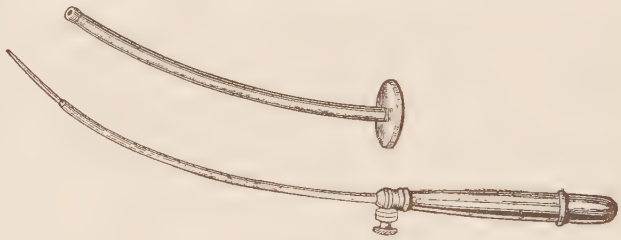


Fig. 28.

icine to keep the dejections liquid. Under these circumstances, as life became almost a burden to the woman, I consented to perform galvano-puncture from the rectum. Every antiseptic precaution was employed. The rectum was syringed out with carbolic solution (1 to 80) before each operation; the instruments were immersed in carbolic-acid solution, and after each operation of galvano-puncture the rectum was again washed out with the antiseptic fluid. The washings out were repeated every day for a week. The operation was performed three times. A trocar (Fig. 28), protected to within about an inch of its end, was passed into the rectum guided by the left fore-finger, and the trocar was thrust into the most prominent and hard part of the tumor, any pulsating vessel having been previously sought for and avoided. A current strength of 20 to 30 milliampères was employed for seven minutes on each occasion. The trocar was made negative and the positive pole was applied to the abdomen through the interven

tion of a potters' clay electrode (see p. 870). Each operation was accompanied by some pain and throbbing, but not sufficiently severe to require an anæsthetic. A week after the first operation a puckered nodule could be felt on the tumor where no doubt the trocar had entered. Ten days later, at the date of the third operation, it was difficult to reach the tumor with the finger, it had become adherent to the wall of the rectum and had been raised higher up in the pelvis. An uneven surface could be detected at the site of the punctures. The peduncle of the tumor could still be felt from the vagina, but there was no feeling then as of a retroverted uterus. There had been no rise of temperature after any of the operations.

A month later the patient reported herself as quite well. There was no difficulty in passing the motions and the bowels acted regularly every day. The tumor could be felt diminished in size, with its rounded surface flattened and the mucous membrane of the intestine adherent. This had produced the desired result—although the tumor had not much diminished in size it had altered in position and become adherent to the rectum, and the obstruction to defecation had been removed.

Some such result might be hoped for in treating enlargement of the prostate from the rectum by electrolysis. With a current less severe than that which would produce suppuration in the gland we might set up adhesive inflammation sufficient to make the prostate become adherent to the bowel, and by this means its position might be so altered as not to obstruct the orifice of the urethra. Such an operation is certainly worthy of trial, for with careful antiseptic precautions it does not appear that the patient runs any great risk.

Dr. Robert Newman, of New York, who has treated stricture of the urethra so successfully by electrolysis, does not employ it in the treatment of enlarged prostate, but uses in preference the galvano-cautery.

CHAPTER VI.

DISEASES OF WOMEN.

THE number of caustics and agents used for cauterization in gynecological practice is extremely numerous, but no decision has been arrived at as to their relative values. No doubt physicians and surgeons have their own reasons for preferring one agent rather than another, but there appears to be no detailed account of the actual effect which each agent has upon animal tissue.

What are the relative values of nitric acid, chromic acid, nitrate of silver, potassa fusa, potassa fusa cum calce, and other caustics that we use? For what reasons do we decide to use the actual cautery, Paquelin's thermo-cautery, or the galvano-cautery? Mr. Robert Ellis, in a paper read before the Obstetrical Society in 1861, said that "cauterizing agents have a different ultimate value, and that sufficient regard had not been paid to that fact in determining the nature of the caustic selected by the obstetrician." He goes on to say, "Some caustic substances are found to produce a sore of greater, and some of less vitality, a result by no means explicable simply by referring to the different chemical nature of these substances. The character impressed by the cauterizing agent on the living tissue differs to a greater or less extent in each case. That resulting from the actual cautery" shows a "very remarkable contractility in the process of healing and afterward. The cicatrix of a burn, built up of cells of feeble vitality, shows a singular inaptitude for extension, and after a time contracts in a very remarkable manner. It might be conjectured that this result—observed in no other instance to the same degree—is due to the nervous shock communicated to the living structures by the burning substance, and ultimately affecting the nutrition of the newly-formed parts." The nutritive changes are no doubt really

due to the destruction of the ends of the nerves; this also accounts for the smaller amount of pain which follows operations performed by the galvano-cautery when compared with those performed by the knife.

It has often been stated that caustic potash produces a less contractile scar than any other caustic, but I cannot find any authoritative ground for the assertion; I do not deny that such is the case, but I cannot find that the fact has been proved. It may be said that the opinion rests upon clinical observation. Dr. Matthews Duncan says that there are no grounds whatever for saying that the scars produced by caustic potash are less retractile than any others, but I believe most gynecologists prefer in practice caustic potash or potassa cum calce, although it is not determined on what grounds that preference is based. Dr. Galabin says that the best potential cautery is the potassa fusa cum calce, because it is less superficial than nitric or chromic acid, while its action is more easily limited than the potassa fusa or chloride of zinc. All the good effects of the caustic alkalies can be obtained by the use of an electrode connected with the negative pole of a continuous current battery without any danger to the surrounding parts, and with the extent of its action absolutely under the control of the operator.

I can remember a controversy some ten or twelve years ago on the value of nitric acid, so extensively used by Dr. Lombe Atthill, of Dublin, in gynecological practice. An objection was raised against its use on the ground that it produced a hard, dense, and contracted cicatrix. Dr. Duncan says that the opponents to the use of nitric acid as employed by Dr. Atthill had no grounds for saying that it produced more contraction—they could not ascertain that fact, as the acid was applied to the internal surface of the uterus for endometritis.

Paquelin's cautery, galvano-cautery, and cautery irons, require the presence of an assistant, and therefore cannot be well used at all times in the consulting room; and they all produce the disagreeable smell of burnt flesh, which is an objection when the operation takes place in a private house. The Paquelin's cautery and cautery irons have to be kept hot, and when used are introduced hot into the vagina. The heat cannot be diminished and raised at pleasure as with the galvano-

cautery. If a wire loop is used with a galvano-cautery battery it can be placed *in situ* around a growth or other substance it is wished to remove before the circuit is closed. The amount of heat can be regulated, and its action commenced and arrested at the will of the operator. These details are under his control in a more thorough and complete manner than with any other form of cautery known to surgery.

The use of galvano-cautery in uterine surgery is an extensive enough subject to deserve a paper entirely devoted to its discussion. I have here to restrict myself mainly to the employment of that property of electricity known as "electrolysis."

The use of electricity both for caustic and cauterizing purposes has been very much overlooked by the profession. I am inclined to advocate that electrolysis should take the place of all other caustics in gynecological practice for the same reasons that galvano-cautery should take the place of all other cauteries. Electrolysis finds its widest field for usefulness in internal parts—situations that are difficult of access, and where it is wished to apply caustics or other local applications. Hence its frequent employment in the treatment of stricture in various regions, and in those diseases of women in which local applications are necessary. It is a more efficient and elegant way of applying caustics than any other that we possess. It can be most accurately localized at the part that it is wished to affect; the amount used and the extent of tissue to be destroyed can be regulated to a nicety; and its action can be arrested at any moment at the will of the operator, the slight pain ceasing immediately the current is cut off.

Stricture of the Urethra.—For stricture of the female urethra, as in the male, a bougie electrode is used of a size larger than the largest one that can be passed. It is of no consequence of what the metallic end of the bougie is made, for it is used with the negative pole of the battery, and when withdrawn from the urethra is as bright as when introduced. A flat electrode connected with the positive pole is placed on some indifferent part of the body. The bougie electrode is passed until arrested by the stricture, against which it is held without any appreciable pressure, but sufficient to enable it to pass into the bladder when the obstruction has melted away from before it. When the bougie electrode is in its place

against the stricture the circuit is closed, and a current of the strength of about five milliampères is allowed to pass for five, ten, or fifteen minutes, or until the obstruction is overcome. No anæsthetic is necessary, as a patient's own feelings are a guide to some extent as to the strength of the current to be used. If five milliampères cause pain the strength of the current can be reduced by taking one or two cells out of the circuit. Directly the bougie electrode has passed into the bladder the current must be broken, and the electrode removed. No further interference with the urethra should take

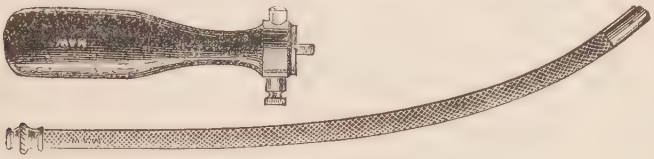


Fig. 29.

place for ten days or a fortnight, and then most probably, instead of there being recontraction, as is usually the case when strictures are treated by dilatation, it will be found that bougies two sizes larger than the electric one at first used will pass easily into the bladder. If the urethra is still of a smaller size than it naturally should be, the operation has to be repeated, commencing with a bougie electrode one size larger than the improved calibre of the passage will then admit.

Dysmenorrhœa and Sterility.

In the same way stenosis of the os uteri or cervical canal can be treated by electrolysis for the relief of dysmenorrhœa and sterility instead of incisions or by the tents and dilators now usually employed. The treatment by electrolysis takes a much shorter time, requires less frequent visits of the patient, and the result is more permanent.

I will not enter into the question as to whether or not stenosis is the cause of the painful menstruation, but a common mode of treating the affection, even by the opponents of the obstructive theory, is to dilate the os uteri. With electrolysis no previous dilatation is necessary, and therefore the operation can be done much more easily and comparatively without the slightest pain. No doubt after the operation the

cervical canal is larger from the chemical action which takes place around the electrode.[27]

A uterine sound adapted for connection with a battery is used with the negative pole, a pad being placed on the back or thigh as in cases of stricture of the urethra. The sound must be encased in some insulating substance to within an inch or an inch and a half of the extremity, and can then be used without a speculum and without injuring the walls of the vagina or the vulva. It is usually only necessary to apply the electrode to the uterine canal for about two or three minutes, and the gain in calibre remains permanent. Different-sized sounds, of course, are necessary according to the size of the os when the operation is commenced. About five milliampères will usually be found a current of sufficient strength, although stronger currents can be borne without pain in this situation than in the urethra. For the membranous

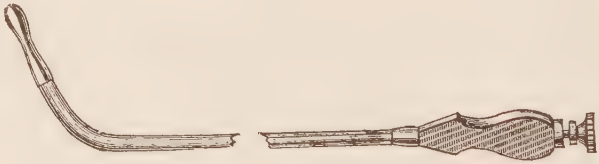


Fig. 30.—Uterine Electrode for Electrolysis, used in the treatment of chronic cervical catarrh, dysmenorrhœa, and sterility, etc., in place of caustics, tents, and dilators usually employed.

variety a stronger current has to be used, and if possible the whole of the internal mucous membrane of the uterus touched with the electrode. This is therefore not such a simple operation as for spasmodic dysmenorrhœa. The patient has to be more or less undressed so that a large surface electrode may be placed upon the abdomen. The current strength used should approach as nearly as possible to 100 milliampères, and the time occupied by the application should be prolonged by a minute or two.

In most other forms of treatment that have been adopted it is necessary to previously dilate the os uteri. This is not necessary in treatment by electrolysis and an operation requiring chloroform is thereby avoided.

The atresia of the uterine canal which sometimes follows amputation of the cervix by galvano-cautery can be easily rectified by electrolysis in the same manner as described for the treatment of spasmodic dysmenorrhœa.

Menorrhagia.—Menorrhagia can be treated most successfully by intra-uterine electrolysis in the same way as membranous dysmenorrhœa, but the electrode used internally must be made positive. The current strength to be used should be about 100 milliampères and the applications are made every four to seven days. Good results are obtained in the relief of this symptom in the treatment of fibroid tumors by Apostoli's method which will be described later on.

Abrasion of the Os Uteri.

Perhaps the most common affections for which gynecologists resort to the use of caustics, and caustics of all sorts and descriptions, are abrasions of the cervix uteri, chronic cervical catarrh, and leucorrhœa. When one caustic does not succeed another is tried. As I have said before, electrolysis is a more elegant and efficient way of applying caustic to internal parts than any other we possess. It destroys unhealthy surfaces and promotes healing in a more reliable manner, and its action

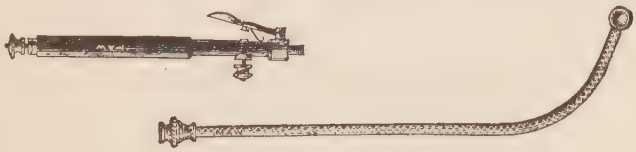


Fig. 31.

can be more easily regulated. For abrasion of the cervix I have used an electrode with a rounded end similar to that used for faradizing the vocal cords. This electrode is attached to the negative pole of the battery; the pad connected with the positive pole, having been previously moistened in salt and water, is placed on some indifferent part of the body. A speculum having been introduced, and the abraded surface brought fully into view, the metallic knob of the negative electrode is lightly moved over the unhealthy surface with a current strength passing of about five milliampères. A white froth resembling fine soapsuds is formed wherever the electrode touches. This froth is alkaline to test paper. All the unhealthy surface will usually be sufficiently destroyed in about two or three minutes. The electrode is then withdrawn, and all chemical action and the slight tingling produced ceases at once. There is no after-pain. If the erosion appears to ex-

tend up the cervical canal, then a uterine sound electrode (Fig. 30) as already described is passed into the canal for about a minute or a minute and a half. Two or three applications of the battery in this way will often make an abraded surface heal which has for months, or sometimes years, resisted all other modes of treatment.

For chronic cervical catarrh and leucorrhœa the uterine sound electrode only is required. In all cases it is to be attached to the negative pole of the battery.

Uterine Fibroids.[28]

The treatment of fibroid tumors of the uterus by electrolysis was practised before 1878 by Dr. Ephraim Cutter, of New York, and the results reported to the American Medical Association. A *résumé* of the cases can be found in the "London Medical Record" of August 15th, 1878. Other cases were reported in the same year by Dr. J. M. Freeman, and this mode of treatment has since been attempted from time to time by other surgeons in America, on the Continent, and in our own country, but has never been widely adopted. Recently this mode of treatment has again been revived by Dr. Apostoli, of Paris, who has modified and improved the manner of performing the operation. He has devised a large surface electrode, made of potters' clay, which is placed over the abdomen of the patient and adapts itself closely to the skin; by this means a stronger current can be employed than was formerly the case, except when needles attached to both poles were thrust into the tumor. Needles or trocars are now only used when it is found to be impossible to introduce the internal electrode into the uterus through the cervical canal, and experience has gradually led to the punctures being made less frequently, less deep, and the duration of the flow of the current much shorter than used formerly to be the case. The current that is used is also weaker than when needles attached to both poles were employed. From all these causes the operation is much less frequently disastrous. By the means at present adopted an amount of success has been achieved which has caused the treatment of fibroids of the uterus by electrolysis to be well recognized, and led to its adoption by many physicians and surgeons. The technical knowledge required and

the expense of the batteries and apparatus will no doubt limit its employment to a great extent to those who make electricity a more especial study, and to patients who are in a position to pay for a costly and elaborate operation which, of necessity, has to be frequently repeated. But it has been demonstrated that electrolysis when properly performed is the most successful way of dealing with fibroid tumors short of their actual removal by abdominal section. The tumor seldom entirely disappears, but the diminution in size is sufficient, and the relief of distressing symptoms is so marked that few would be willing to subject themselves to the risk of an abdominal section for the removal of a tumor when so much benefit can be derived from a comparatively safe and almost painless operation.

By the theory of Grotthüs electrolysis consists of a series of decompositions and re-combinations transmitted from one molecule to the next as if by a chain across the electrolyte, no appreciable change taking place except at the incidence of the respective poles; the electro-positive elements are liberated at the negative pole and the electro-negative at the positive pole. This may be true of simple compounds, but in a complex material such as that of which the human body or a fibroid tumor is composed, it is hard to believe that no change whatever takes place through the intervening tissue when a current of electricity is passed, but only at the surface of the electrodes. It would seem from clinical observation that by the passage of an electric current adventitious tissues which have been deposited are changed into a condition which allows of their being re-absorbed. This re-absorption will continue for several days after a strong current has been used. The denser the structure traversed by the current the less capable of re-absorption is it rendered. It must be borne in mind that during the passage of a current of electricity there is no doubt that the ordinary osmotic action which is continually going on in living animal tissue is accelerated in the direction from the positive to the negative pole.[29]

A Stöhrer's battery (see p. 814) is the best to use for the electrolysis of fibroids, because the electro-motive force is high and the internal resistance low, and polarization does not take place to such an extent as in many other forms of battery. A battery of thirty cells will be found sufficient, and

when the cells have been freshly charged and the plates newly amalgamated it will seldom be necessary to bring more than eighteen or twenty-two cells into the circuit to give a current strength of 100 milliampères when the external large surface electrode of potter's clay is used for covering the abdomen.

Leclanché batteries are also used for this operation, but more cells are required, on account of their lower electromotive force and greater internal resistance, and the number has to be gradually increased to keep up the required current strength, on account of the rapid increase of the internal resistance which takes place in consequence of polarization. A Leclanché battery is more portable than a Stöhrer's, and therefore can be more easily taken to a patient's house.

The current strength is regulated by a galvanometer. The one used by Dr. Apostoli is horizontal, and graduated so as to indicate at least 250 milliampères. A vertical galvanometer, as made by Dr. Stöhrer, can also be used, and will register a current strength of 1,000 milliampères (or one ampère). A Stöhrer's galvanometer possesses a marked binding screw, which must always be connected with the positive pole of the battery, and then, by lifting the right-hand peg, a resistance of about 100 ohms is introduced into the circuit, and the needle swings to the left and will register up to 40 milliampères. If the middle peg is removed as well as the right-hand one, a resistance of nearly 1,000 ohms is introduced into the circuit, and much smaller currents can be easily registered. To detect strong currents, such as are used in the electrolysis of fibroids, it is necessary to keep the middle and right-hand pegs in their proper holes and to remove the left-hand peg, the needle then swings to the right; the graduated spaces each indicate 100 milliampères, and extend up to 1,000.

The potter's clay electrode introduced by Dr. Apostoli, and which is used as the external electrode for covering the abdomen of the patient, is prepared by moistening the clay to the consistency of soft putty and spreading it out half an inch thick on a piece of tarlatan or net, the dimensions of which should be about twelve inches by twenty-six. The clay is spread over about half this surface, that is, about nine inches by twelve, the remainder of the tarlatan or net is folded over so as to cover the upper surface of the clay, and to prevent as much as possible the smearing of the patient's clothes. A hole is cut in

the upper layer of the net, and a piece of soft metal, such as tin, possessing a binding screw upon its upper surface, is passed under the net and allowed to rest on the surface of the clay. To the binding screw is attached one of the rheophores leading from the battery. No more convenient electrode has as yet been devised that will adapt itself so closely to the inequalities of an abdomen often enormously distended by a tumor; but the potters' clay electrode has many disadvantages—it is very difficult to keep it warm, and therefore often when first applied strikes very cold to the patient; its preparation is disagreeable and messy work, and the clay more or less oozes through the meshes of the material on which it is

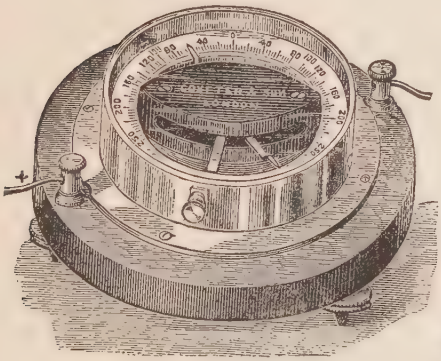


FIG. 32.—Gaiffe's Galvanometer.

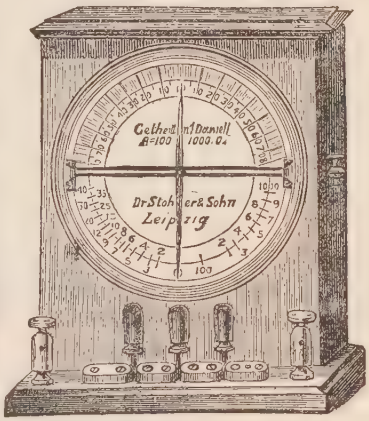


FIG. 33.—Dr. Stöhrer's Galvanometer.

spread, and adheres to the patient's skin and clothes. Some better form of electrode, that will diffuse the current equally over a large surface, is much to be desired.

The electrodes used for applying the current internally are of different patterns, according to the description of tumor that is to be dealt with, and whether or not a sound can be passed into the uterus through the os and cervical canal. In the former methods of treating fibroid tumors by electrolysis, galvano-puncture was practised much more frequently than is now the case. Many of the punctures were made through the abdominal walls, and careful directions were given, in descriptions of the operation, as to how the needles were to be introduced and removed. The former operations were from

this cause much more serious and risky, and on the whole, even when no unfavorable symptoms occurred, were not so satisfactory in their results as these operations are at the present day.

When it is possible to pass a sound into the uterus Dr. Apostoli uses a platinum rod about a foot long, blunt at one

Fig. 34.—Apostoli's Intra-uterine Electrode.

end and pointed as a trocar at the other. This rod is about the size of an ordinary sound, and can be passed into a canulated handle, and fixed by a screw swivel on its side. Either end can be thus fixed according to the requirements of the operation. At first the whole rod was made of platinum, but now the trocar half is made of steel, as steel takes a better point, is not so expensive, and can be used equally as well as platinum with the negative pole—the pole always employed

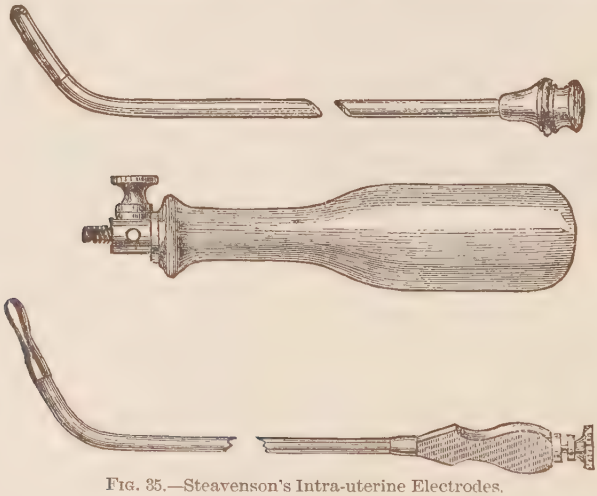


Fig. 35.—Steavenson's Intra-uterine Electrodes.

when galvano-puncture is practised. A vulcanite or celluloid sheath is used to protect that part of the sound electrode that is in the vagina or against the vulva.

The electrode used by the author is much more flexible. It consists of a copper wire insulated by a No. 5 or 6 English gum-elastic catheter. The last inch consists of a piece of

platinum of the same size as the catheter, welded on to the end of the copper wire. The electrodes are made so that they can be used with an ordinary handle, or are provided with a Brodie's handle possessing a binding screw, to which can be attached the other rheophore leading from the battery. This form of electrode is very much less expensive, on account of the smaller amount of platinum that is used in its manufacture. The flexibility is an advantage, as it enables it to be passed into the uterine cavity through the cervical canal in many cases in which it would be impossible to do so with the rigid electrode used by Dr. Apostoli. When a fibroid tumor is present, the uterus is often so tilted in one direction or another that the opening and course of the cervical canal are

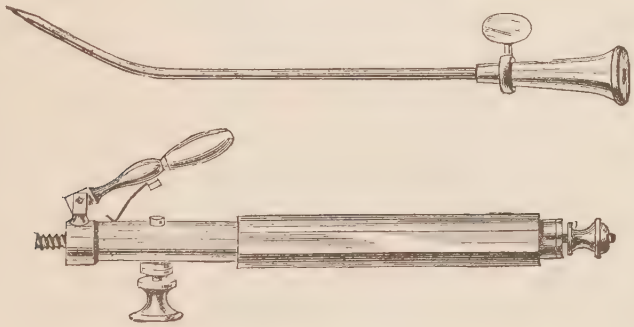


Fig. 36.

very far removed from the position in which they would naturally be found And again, with the celluloid or vulcanite sheath used by Dr. Apostoli it is almost impossible to shield that part of the platinum rod which is in contact with the cervical canal and os uteri. With the more flexible electrode it is quite easy to get the unprotected end of the platinum well within the uterine cavity; the gum-elastic part of the stem only being in the cervical canal.

The needles for puncturing the tumors are made of platinum or steel of the shape shown in the diagram (Fig. 36), and are fitted on to a handle. They are intended to be used with a speculum, but might be used without if the greater part were insulated, leaving only about an inch of the metal bare at the end (see also Fig. 28, p. 860).

Before commencing an operation, the size of the tumor

should be accurately gauged and measured, so that comparison can be made during the course of the treatment, or when sufficient relief has been obtained so as to render its continuance unnecessary. In this relationship it must be remembered that in the great majority of cases, and especially in women of about middle age, as the distressing symptoms are relieved and the general health improves, there almost invariably takes place a deposition of subcutaneous abdominal fat, which would seriously invalidate the measurements if not taken into consideration. The capacity of the uterus, as evidenced by the distance to which a sound can be introduced, is one of the best guides as to increased or diminished size. The other most important measurements to be taken are:

From the tip of the xiphoid cartilage to the centre of the umbilicus.

From the xiphoid cartilage to the symphysis pubis.

From the umbilicus to the symphysis.

Girth at umbilicus.

From one anterior superior spine to the other.

From the right anterior superior spine to the umbilicus.

From the left anterior superior spine to the umbilicus.

If the tumor is at all prominent, additional measurements may be made:

From the tip of the xiphoid cartilage to the highest part of the tumor.

Girth over the most prominent part of the tumor.

From the highest part of the tumor to the symphysis; and, if possible, the greatest width and length of the tumor taken with calipers.

After electrolysis has been performed several times, both the size and shape of the tumor will be found to be altered.

The Operation.—The vagina should be syringed out with an antiseptic solution, such as dilute carbolic acid (1 in 80), and the patient placed in the dorsal position on a couch or operating table, with the buttocks brought well down to the edge. If the table does not possess supports for the feet, a couple of chairs should be placed on which the feet can rest. The abdomen of the patient should now be uncovered and sponged with warm salt and water, or a warm 2½-per-cent solution of chloride of zinc. This helps to reduce the resistance of the skin. A large surface electrode of potters' clay should

now be placed upon the abdomen and pressed gently with a towel so as to make it adapt itself perfectly to the skin. Should there be any scratch, abrasion, or acne spot on the abdomen it should be first covered with a small piece of oil-silk or plaster before the electrode of potters' clay is applied, as any such imperfection in the cuticle offers less resistance to the passage of the current, and, as the strength is increased, would cause great pain, so that the patient would not be able to bear such strong currents as she otherwise would.

The electrodes, needles, or trocar, intended for internal application, as well as the handles to be used with them, should be placed by the side of the operator in a porcelain dish containing an antiseptic solution. The operator should place himself between the thighs of the patient, and having rinsed his hands in the antiseptic solution, should introduce the forefinger of the left hand into the vagina and seek for the os uteri. Should he be able to reach it, the sound electrode (Fig. 35) is passed along the finger, already in the vagina, and its end tilted up so as to make it enter the cervical canal. If possible, the internal electrode should always be passed into the cavity of the uterus, but in some cases it is accomplished with great difficulty, and occasionally is altogether impracticable, on account of the uterus being tilted and displaced by the size and weight of the tumor; but every manœuvre should be practised by which a sound can be introduced. The os can sometimes be pulled down by a hook, or pressure on the tumor through the abdominal wall will so alter the direction of the uterus that an electrode can be passed into the canal. The employment of the more flexible electrode is in these cases a great advantage. With the index finger in the vagina, the end of the electrode, when it reaches the os, can very often be turned so that it enters the aperture.

In the majority of cases the internal electrode should be attached to the negative pole of the battery, for instance, when the tumor is of a dense and fibrous nature, accompanied by dysmenorrhœa, amenorrhœa, or the ordinary pressure and bearing-down symptoms due to its size and weight, or when reflex neurotic symptoms predominate. When the tumor is of a soft and more vascular nature, and inclined to be associated with menorrhagia, then the internal electrode should be attached to the positive pole on account of its hæmostatic

action. When used with the positive pole the metal, intra-uterine portion, of the electrode should be made of platinum, because of all metals it is the least oxidizable. With the negative pole any metal may be employed, as the current does not then oxidize, and the electrode will be removed from the uterus with the metal portion as clean and bright as when introduced. When it is found necessary to puncture the tumor, for instance in those cases when a sound cannot be introduced, the needle or trocar is attached to the negative pole, and, as far as the current is concerned, may be made of any metal. Steel takes the best point, and is, therefore, the metal usually preferred.

The needles are most frequently employed with the object of making an artificial passage into the uterine substance, which is subsequently used for the introduction of the sound electrode; but sometimes punctures are made into dense hard tumors, when it would be quite possible to introduce an electrode through the cervical canal, so as to expedite the process of disintegration. Sometimes the punctures are used in conjunction with the intra-uterine application. All the punctures are made from the vagina, and into the most prominent part of the tumor, or into that part which will lead most readily to the uterine cavity, of course taking care that the needle does not injure any neighboring parts, such as the bladder or Douglas's pouch, or enter into any large vessel which might be indicated by the presence of pulsation. The punctures are not made so deep as formerly, but now are usually only about half an inch in length. They may or may not communicate with the cervical canal or uterine cavity; if not, the channels made may be used for subsequent introductions of the electrode when the current is employed, or fresh punctures may be made from time to time on "suitably presenting parts of the tumor."[30] The punctures are made through a speculum when the needles are used; with Dr. Apostoli's trocar (Fig. 34) a speculum is not necessary. The needle or trocar is allowed to remain in the puncture for about five minutes, and 100 milliampères is the current strength which most patients can bear.

The actual cautery has not infrequently been used in the treatment of hypertrophied cervix, and by Dr. Greenhalgh to favor the enucleation of fibroids; the puncture and production of a hole in the cervix uteri by electrolysis is in no way a more

dangerous proceeding. The application of electrolysis is itself antiseptic, but in order that no precaution may be omitted, it is usual to have the vagina syringed out with some antiseptic solution both before and after the operation. When it is necessary to perform galvano-puncture this precaution is perhaps the more desirable.

When everything is ready and in position,—that is, the potters' clay electrode accurately adapted to the skin of the abdomen and connected with the positive or negative pole of the battery according to circumstances; the internal electrode selected and placed in position and connected with the other pole; and the galvanometer arranged so as to be included in the circuit,—the circuit is closed, and the current gradually increased in strength without any break. If when strong it gives rise to any complaint on the part of the patient, the strength must be gradually reduced again by eliminating one or two cells from the circuit, but the patient must be encouraged to bear as strong a current as she can. It will often be found that at the first application a current of more than 70 or 100 milliampères cannot be tolerated. On subsequent occasions the current can be borne stronger, and sometimes reaches as high as 250 or 300 milliampères. Anything over 100 may be considered quite strong enough to obtain results in case of non-galvano-puncture. The time occupied by each application varies from five to eight or ten minutes. The current should be gradually reduced in strength, cell by cell, in the same way as it was increased, and the greatest care taken not to produce any shock, which would be the result if the circuit were suddenly broken.

It is as well that the patient should remain in bed and quiet for twenty-four or forty-eight hours after the first application, and on subsequent occasions if there be any pain or tenderness of the abdomen; but generally a patient may, after an hour or two's rest, walk about, and on the following day pursue her usual occupation.

The resistance the patient offers to the current varies very much with the condition of the skin at the time of the application, and with the size and moisture of the external electrode. In one case, when a current strength of 140 milliampères was used, and given by 10 cells of a Stöhrer's battery, the resistance was calculated to be about 130 ohms. In another case,

with a current strength of 120 milliampères from 16 cells, the resistance was about 225 ohms. In each case the electro-motive force of the battery was taken by a voltameter previously to the operation. In some American cases the resistance offered has been reported as low as 60 ohms; this seems almost incredible, but is probably true if the current used in any way approached in strength those recorded, viz., 500 to 1,000 milliampères.

The number of applications of the current that are necessary will vary with the character of the tumor. For bleeding fibroids four or five applications are sometimes sufficient to obtain relief, although the majority require ten or twelve. Hard, dense fibroid tumors diminish in size very slowly, and may require as many as thirty applications of intra-uterine electrolysis. It is difficult to fix any exact time at which the applications should be discontinued, as the tumors seldom disappear entirely. When the patient has been relieved of all distressing symptoms and the tumor is reduced sufficiently in size to cease to be an annoyance through its bulk and weight, the applications may be reasonably stopped, or only had recourse to occasionally, should any symptom return to justify it.

The frequency with which the applications should be made is also a matter which depends upon various circumstances, such as the nature of the tumor, convenience, menstruation, the effects produced, and other conditions. In some cases the applications may be made as often as twice a week, or every four days, in others intervals of ten days or a fortnight may be necessary.

The great danger inseparable from abdominal section and the uselessness of all medicinal treatment for the removal of uterine fibroids, makes this mode of treatment by electrolysis the more acceptable and of greater importance. There are few fibroid tumors which do not cause some inconvenience, and more or less distressing symptoms. In some cases they render life almost intolerable; the ever constant weight and dragging pain; the interference with digestion, defecation, and sometimes with micturition; the reflex neurotic symptoms and depression; the ever constant ill-health with exacerbations at the ordinary monthly periods, sometimes with downright acute pain or profuse hemorrhage, which incapacitates

the patient for several days or a week; the drain on the vital powers produced by this incessant pain and sometimes constant loss of blood; all these call for some relief beyond the usual exhortation to wait for the menopause or a useless visit to Kreuznach or Horncastle. Enucleation is often impracticable, and is never unattended with danger. In electrolysis we have a means of relief, the application of which is not difficult to those who understand the medical and surgical uses of electricity. It is not unduly painful. It is, if properly applied, practically free from danger. If the tumor is not much reduced in size the distressing symptoms are almost invariably relieved, and the patient's health improved, and she is not in a worse condition for more heroic measures, should they be deemed advisable, than before the application of electricity. The treatment is spread over rather a lengthened period of time, but after the first or second application no enforced confinement to bed is necessary or imposed upon the patient.

In the St. Bartholomew's Hospital Reports for 1888 I have given the details of "Thirty Cases of Fibro-myomata of the Uterus treated by Electrolysis." Twenty-three of these cases were benefited by the treatment. In eight the result may be described as very good; the patients having greatly improved and the tumors diminished in size. Fifteen may be said to have improved, the symptoms being relieved. In one of these cases the tumor was larger at the end of a year, but the distressing and exhausting symptom of loss of blood, for which treatment was primarily sought, was relieved and has not recurred. In one case the tumor was extruded from the os and removed by the écraseur. In one case the occurrence of pneumonia cut short the treatment. Two cases of the fifteen were improved at the time they left the hospital, but have not been heard of since. In four cases improvement took place. Two improved while under treatment, but relapsed. One case ended fatally by ulceration taking place into a large vessel. Such a catastrophe could hardly be anticipated or guarded against, but the possibility of its occurrence must be taken into account when weighing the relative value of this mode of treatment. Uterine fibroids resent interference. In more than one case already reported, enucleation has been induced by the application of electrolysis—not altogether an unfortunate occurrence if the patient makes a good recovery;

should the reverse be the result, the electricity has to bear all the blame of the misfortune. The tumors do not disappear so rapidly or so completely as we have been led to expect, or perhaps as British gynecologists supposed would be the case. I do not think that the electrolytic treatment will supplant the necessity for abdominal section or removal of the uterine appendages in such cases as those in which these operations would be appropriate, but I do think that in the use of electricity we have a means of treating fibroid tumors which is superior to any we have formerly employed, short of their entire removal. I have treated a large number of cases, and in nearly all the symptoms have been relieved, in many the tumors have decreased in size, and in some the decrease has been considerable. The hemorrhage has usually been arrested; the dysmenorrhœa has been relieved; the pain and discomfort have decreased; the rectal and bladder troubles have also been lessened, and the patients are able to get about and walk with much less difficulty. All these favorable symptoms are probably due to the relief from pressure caused by a slight diminution in the size of the tumor. Nearly all the patients I have operated upon have expressed themselves as feeling better—better in health and better in spirits. In those whose tumors were accompanied by menorrhagia no doubt the improvement in health was greatly due to the arrest of the hemorrhage. That adventitious tissue does decrease and get reabsorbed under the electrolytic treatment I feel sure, and have been able to observe it in several cases of stricture of the urethra.

Hypertrophy of the Cervix.—This affection has often been treated successfully by electrolysis, and sometimes when other means have failed. One or two needles connected with the negative pole are thrust for about half an inch into the hypertrophied tissue and a current strength of eight to ten milliampères used for five to eight minutes. The positive pole is placed on the abdomen, or, if the patient is on her side, on some indifferent part of the body. This operation can be performed twice between each menstrual period, not fixing an operation too near them, and allowing ten days or a fortnight to elapse between the applications of electricity.

Fungoid growths may also be destroyed by electrolysis by employing the galvano-puncture.

Attempts have also been made to encourage reabsorption in *ovarian cysts* by means of galvano-puncture, but the results collected and published by Dr. Paul F. Mundé, of New York, are not very encouraging.[31] The deaths following the employment of electrolysis were 17.66 per cent, and the cures only 55 per cent.

Electrolysis has also been used to reduce the thickening which sometimes remains after inflammation of the cellular tissue included in the pelvic folds of the peritoneum (*pelvic cellulitis*). Interference by electricity in the acute stage is to be deprecated, although it has been advised by some authors.

Negative galvano-puncture is performed sometimes into the thickened mass and sometimes into the enlarged uterus or hypertrophied cervix. Chloroform is not as a rule necessary. A previous injection of morphia may be employed to allay apprehension and quiet the patient. The strength of the current used must be regulated by the feelings of the patient, and may vary from 50 milliampères even up to 250. The higher currents cannot as a rule be used without an anæsthetic. The time occupied at each application of electricity should be from five to eight minutes; and the applications should be made midway between the menstrual periods. The number of applications necessary depends upon the case. The punctures should be made by preference into the posterior part of the uterus, avoiding all pulsating vessels, but choosing as far as possible the most prominent part of the enlarged organ. The punctures should not be deep (a quarter to half an inch). Antiseptic precautions should be employed. The vagina should be syringed out both before and after the operation by some antiseptic solution, such as carbolic-acid solution 1 in 80. The instruments to be used should also previously be immersed in a similar fluid. After the puncture a tampon of iodine and glycerin may be used.

Metritis and *endometritis* are often treated by the chemical galvano-cauterization of the mucous membrane of the uterus or the cervical canal, which is, in fact, the electrolysis of the mucous membrane. It is performed by passing an uterine electrode into the cervical canal or into the cavity of the uterus. Antiseptic precautious should be employed. In cases associated with much hemorrhage, the positive pole is used internally and the uncovered metal part of the electrode within

the uterus (see Fig. 35) must be made of platinum. In other cases the internal electrode is made negative. It is not necessary to use a current that will cause pain. With nervous patients 40 or 50 milliampères will be sufficient. The current should always be increased slowly, 20 to 30, to 50 milliampères, by adding one or two cells of the battery at a time to the circuit. At the first application the current strength should not exceed 100 milliampères, but on subsequent occasions, if the patient bear the treatment well, 200 milliampères or even stronger currents may be used. Each application should occupy five to ten minutes, and the current strength reduced as gradually as it was increased. The electrode when passed into the uterus should be made as much as possible to touch every point of the mucous membrane by occasionally moving it from place to place. The application can be made once every week or twice as often if necessity requires. The number of applications necessary depends upon the case.

Dr. Bartholow says: "A large and accumulating experience in this country and abroad, has conclusively demonstrated the power of galvanism to bring about the absorption of inflammatory exudations in the pelvic cavity. By Remak this function of galvanism was entitled catalytic, but at present the term most in vogue is cataphoric. As has been shown, galvanism lessens or removes congestion by an influence exerted on the vessels. When the faradic current can be directly applied to the affected part, as is feasible in uterine affections, strong contractions of the arterioles can be caused by it. Whether the result, in pelvic diseases, is effected by this action on the vessels, or by catalytic or cataphoric influence, may be a merely technical question; but the important practical fact is established that by galvanic, faradic, and galvano-faradic applications very serious maladies are cured more speedily, safely, and easily, than by the best directed use of medicines."

Cancer.

The subject of the treatment of cancer by electrolysis has been again brought prominently before the profession by recent papers in one of the medical journals, and has excited some attention. The treatment of cancer by this agent has from time to time been revived and extensively employed to

again fall into disuse. The chief reasons for the fluctuation in the prevalence of the use of electricity for this as for many other affections, is that immediately it is brought into prominence it is frequently employed in unsuitable cases, and therefore gives unsatisfatory results; or, the expense and trouble with the batteries, and the time necessarily involved in carrying out the details of the treatment are so great that medical men very soon are induced to abandon it for more expeditious and less troublesome methods. The latter in most cases of cancer are no doubt the more suitable. For instance, for the removal of a cancer of the breast, the knife is in most cases much to be preferred to the use of electricity as far as our present knowledge of its application enables us to employ it. But there may be cases that are beyond the hope of successful excision or in which the patient will not submit to a cutting operation, and then the treatment by electrolysis may be the most suitable of all other alternatives.

A tumor of the breast can be destroyed and allowed to slough out with as good a prospect of its not returning as when removed by the knife; but the process by electrolysis takes a much longer time, is much more troublesome, and in the long run involves more pain.

The use of electricity for the destruction of small scirrhus masses is described by Dr. Golding Bird in his lectures on "Electricity and Galvanism," published in 1849 (p. 137); again by Dr. Althaus in 1867 in his paper "On the Electrolytic Treatment of Tumors and other Surgical Diseases." The plan advocated by Dr. Althaus was extensively tried by the late Mr. Callender in 1873 and 1874 in the wards of St. Bartholomew's, and also at the same date by the author when House Surgeon for Mr. Luther Holden, but the results were not satisfactory. Large sloughing sores were left in the patient's breasts, with only a partial destruction of the cancer. The only benefit was some reduction of pain. Dr. Althaus says, "It is a curious fact that the peculiar lancinating pains of cancer generally seem to disappear, or at least to diminish considerably, soon after the commencement of the electrolytic treatment, and long before the whole tumor has disappeared."

In nearly all the cases operated on at the date to which reference has been made, very strong currents were used. In those days there were no galvanometers graduated into mil-

liampères, but from later experience it would seem that the current strengths employed were quite as high as those recently employed for uterine fibroids.

Drs. Beard and Rockwell have described a system[32] of treating scirrhus tumors which they call "electrolysis of the base." It consists of passing needles connected with both poles of a galvanic battery into the normal tissue beneath the tumor, when that is possible, and thoroughly destroying the connection between the tumor and the subjacent structures. When the tumor is a large one they consider it best to remove it by the knife and then to thoroughly destroy the surface from which the tumor has been removed, by electrolysis, so as to destroy any cancer cells that may be in the parts subjacent to the tumor. The advantages they claim for this method are that there is less liability to recurrence, less hemorrhage, less liability to shock, more satisfactory healing, and less likelihood of septicæmia and pyæmia.

Fig. 37.

There are other cases, such as cancer of the uterus, in which the disease has spread to the vaginal walls, or where the cauliflower excrescences are so extensive and diffused that there is no prospect of removing them with the knife, the écraseur, or the galvano-cautery, in which electrolysis is of the greatest service in removing portions of the growth, arresting its increase, relieving pain, decreasing hemorrhage, and prolonging life. For this purpose an electrode composed of a broad piece of zinc is connected with the positive pole of a constant-current battery, the negative pole being placed on some indifferent part of the body. When the circuit is closed decomposition of the diseased tissue takes place with a formation of chloride of zinc around the positive electrode. This has an advantage over destruction by ordinary chloride of zinc, or the actual cautery, inasmuch as the action can be localized and arrested at pleasure, and the destruction of the tissues can be regulated to the exact amount that is desired. Needles are also used for destroying portions of the growth, and may be attached to the negative pole, or to both poles.

In the latter case the destruction takes place much more rapidly. The positive pole is used only when the additional action of the metal, of which the electrode is composed, is required.

A recent writer on the treatment of cancer by electricity has advocated the employment of shocks, given by the interruption and reversal of very strong galvanic currents. He advocates this mode of treatment on the theory that the low vitality of the cancer cells is extinguished by the sudden reversion of the current, and likens it to the process by which human life is destroyed by electricity. He denies the occurrence of any electrolytic action between the two poles in the ordinary method of treating fibroid tumors, and brings forward experiments to substantiate his contention. The author has before argued [33] that electricity probably does not kill by shocks when administered to lowly organized living material, but by the effects of shock upon highly organized living beings, acting through the inhibitory nerves, and interfering with the rhythm of the heart. It is probable that it is more difficult to extinguish lowly organized vitality by shocks than highly specialized vitality, presided over by an elaborate and finely differentiated nervous system. The experiments also brought forward to prove that no change takes place between the poles when a current of electricity is applied to animal tissue, have been proved to be fallacious. The experiments referred to are as follows: [34] "Three glasses were connected together with a stout lamp-wick. Into each of these a standard solution of iodide of potash was placed. A current was then passed through the solution, with an intensity of 200 milliampères, until the whole of the iodide of potash in the two outer glasses was decomposed. On testing the centre glass, it was found to contain as much iodide of potash as before, although in the two outer glasses it was all decomposed." "A second experiment was then tried with water in the outer glasses, and a solution of iodide of potash in the centre. The current was passed as before, but no decomposition took place in the centre glass. A third experiment was tried on the web of the frog's foot, and watched through the microscope. At the positive pole a coagulation took place, with exudation of blood from the capillaries. At the negative pole a collection of hydrogen bubbles could be seen. Between the poles no change

could be observed, and the circulation went on as before. Finally, a strong current was passed through a fibroid tumor, soon after removal from the body, for a period of several hours. On making sections of the portion between the poles, no change could be detected. All these experiments demonstrated in a conclusive way that the passage of a constant current does not cause any decomposition between the poles, although there must of necessity be an exchange of atoms between the molecules."

In referring to these experiments before the Obstetrical Society of London, on June 21st, 1888, the author stated that he had repeated the experiments,[35] "substituting pieces of copper wire for the lamp-wick, as his connecting media. The copper wire, while being a very good conductor, was also decomposed by electricity. In this case the iodide of potassium was split up in each jar, free iodine appearing at the positive ends of the pieces of connecting wire, and a large quantity of gas (hydrogen) being liberated at the negative ends."

Some interesting experiments are also detailed in Golding Bird's "Natural Philosophy," to show that changes do take place at other points than the electrodes when a current of electricity is passed through several electrolytes.[36] "The statement that, in cases of electro-chemical decomposition, the changes which take place in the electrolyte are continuous through a line of molecules, and not limited to those in contact with the electrodes, meets with an interesting illustration in the well-known experiment in which an alkali appears to traverse an acid without combining with it; and which has been erroneously regarded as a case of suspension of the laws of chemical affinity.

"Let three cups, A, S, B, be placed side by side and connected by means of pieces of lamp-cotton, moistened with a solution of sulphate of soda. Let A and B be filled with a solution of this salt, and the central cup, S, with dilute sulphuric acid. Let a platinum electrode, C, connected with the last copper cell of the battery, dip into A, and another, Z, from the last zinc plate, dip into B. The positive current will now enter the fluid in A, and escape from B through Z, traversing S in its course. Electrolysis of the sulphate of soda will take place, its acid with oxygen being set free in A, and the sodium will pass through the sulphuric acid in S and reach B, so that

a quantity of free soda will soon be found in B; the sodium being oxidized at the expense of the water. It is evident that this alkaline body must have traversed the acid in S, with which, indeed, it for an instant combined, and the resulting sulphate of soda being decomposed by the current, the soda ultimately appears in B.

"That in experiments of this kind the base really combines with the acid it is made to traverse is proved by using a salt with the base of which the acid forms an insoluble combination. Under these circumstances it is removed from the influence of the current and does not reach the third cup. Place in A, B solutions of chloride of barium, and in S dilute sulphuric acid; on the current passing, the contents of A are decomposed, chlorine is evolved and barium set free; this is conveyed in the manner before described to the middle cup and here it is

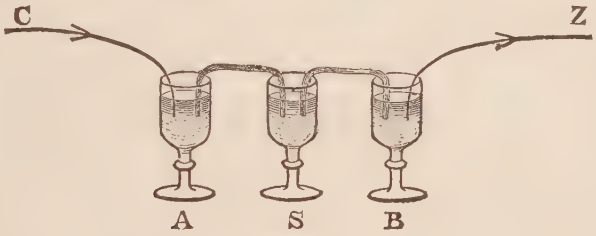


FIG. 38.

arrested in its course by the acid, which, in combining with it, forms an absolutely insoluble salt, the sulphate of barytes, which falls to the bottom of the vessel, and then neither barium nor its oxide reaches the cup B. Hence the salt chosen for experiment must be one whose base forms a soluble combination with the acid in the middle cup." (The author has repeated the above experiment, but has not been able to obtain the white precipitate mentioned by Dr. Golding Bird; possibly from insufficient battery power.)

An experiment by Professor Daniell will show that changes take place at other points than at the surfaces of the electrodes in connection with the battery when the current has to traverse two or more electrolytes, and these changes are apparent where the electrolytes are in apposition:

"A small bell-glass, with an aperture at the top, had its mouth closed by tying a piece of membrane over it. It was

half filled with a dilute solution of caustic potash, and suspended in a glass vessel containing a strong solution of neutral sulphate of copper, below the surface of which it just dipped. A platinum electrode connected with the last zinc rod of a large constant battery of twenty cells was placed in the solution of potash, and another connected with the copper of the first cell was placed in the sulphate of copper immediately under the diaphragm which separated the two solutions. The circuit conducted very readily, and the action was very energetic. Hydrogen was given off at the cathode, and oxygen at the anode in the sulphate of copper; a small quantity of gas was also seen to rise from the surface of the diaphragm. In about ten minutes the lower surface of the membrane was found beautifully coated with metallic copper, interspersed with black oxide of copper and light blue hydrated oxide.

"The explanation of these phenomena is this:

"In the experimental cell we have two electrolytes, separated by a membrane, through both of which the current must pass to complete the circuit. The sulphate of copper ($CuSO_4$) is resolved into its compound anion (SO_4) and its cathion (Cu); the latter in its passage to the cathode is stopped at the surface of the second electrolyte, which may be regarded as water improved in conducting power by potash. The metal here finds nothing by combining with which it can complete its course, but, being forced to stop, yields up its charge to the hydrogen of the second electrolyte, which passes on to the cathode and is evolved. The corresponding oxygen stops also at the diaphragm, giving up its charge to the anion of the sulphate of copper. The copper and oxygen, thus meeting at the intermediate point, partly enter into combination, and form the black oxide, but from the rapidity of the action there is not time for the whole to combine, and a portion of the copper remains in the metallic state, and a portion of the gaseous oxygen escapes. The precipitation of blue hydrated oxide doubtless arose from mixing of a small portion of the two solutions."[37]

Other reasons for supposing that some change takes place in animal tissue when traversed by an electric current are given on pp. 805, 806.

Dr. Neftel, who is a great authority on the treatment of cancer by electricity,[38] is inclined to believe that electrolysis

produces remote constitutional effects, by altering the condition of the protoplasm of the cells in which the poison of the cancer is contained, and by the propagation of which the disease becomes constitutional. As soon as the protoplasm has, by the electrolytic process, lost its specific contagious qualities, the cancer is prevented from reproducing itself, and gradually disappears through the process of absorption.

The preceding lengthened reference to the treatment of cancer by the interruption and reversion of strong voltaic currents has been made because, in the author's opinion, the method is founded on unproven and probably fallacious theories, and is not free from danger to the patients. Electrolysis is no doubt sometimes very useful in cancer, not only to destroy portions of the growth and thus check the advance of the disease, but noticeably to diminish the pain so frequently attending it.

Small growths on the vulva, vascular pimples, warts, moles, or any slight permanent thickening of the epidermis, ought to be destroyed; and it can be best accomplished, as on the face, by electrolysis. There is no part in women so obnoxious to epithelioma as the vulva.

The inflamed and sensitive condition of the orifices of the vestibular glands and the surrounding parts which so frequently cause dyspareunia can often be successfully treated by electrolysis. but cocaine is seldom sufficient to render treatment tolerable. If an anæsthetic is indispensable, it would generally be better to destroy this unhealthy congested condition of the mucous membrane more thoroughly by galvano-cautery. But with galvano-cautery the presence of a second person is necessary, and one who understands well how to manage a galvano-cautery battery. The presence of an assistant is not necessary for treatment by electrolysis.

Extra-uterine Fœtation.

The Americans claim most satisfactory results from the treatment of extra-uterine pregnancy by electricity.[39] The chief forms employed are the faradic current and the continuous current, of some considerable strength, rapidly made and broken, and attempts have also been made to kill the fœtus by galvano-puncture. In my hands the treatment of extra-

uterine fœtation by electrolysis has not seen successful. I know of only one reported successful case, that of Dr. Petch, of York.[40] In the case reported by Dr. Duncan,[41] he says: "The appearance of the fœtus post-mortem shows that the electrolysis had tremendous effect upon it, and its survival is most marvellous. The post-mortem was little more than two days after its death. The destruction of its tissues did not at all resemble that seen in the ordinary course of putrefaction —the bones extensively laid bare, the skin and great part of the tissues having been dissolved and having disappeared; and showing that this was due to the electrolysis; there were parts uninjured, the skin entire as in a fœtus only dead two or three days. Yet during most of this destruction the fœtal heart continued to beat, apparently undisturbed, except as to frequency of pulsation.

In the case reported by Dr. Percy Boulton[42] it was not possible either to verify the diagnosis or see what effect the electrolysis had produced.

Dr. Strahan, in his Jenks Prize Essay on "The Diagnosis and Treatment of Extra-Uterine Pregnancy" (1889), denounces all forms of electrical treatment, especially that by electrolysis.

CHAPTER VII.

MISCELLANEOUS.

Fistulæ.

Sinuous passages, such as those leading to the site of former abscesses in the breast and elsewhere, can be made to heal from the bottom by inserting a probe insulated to within a short distance of its end and connecting it with the negative pole of a battery. When the probe is in position at the bottom of the sinus the circuit is closed and electrolytic action takes place around its unprotected end. The current should be allowed to flow for about two minutes and the applications repeated about once every week until the sinus is completely obliterated. This is an improvement upon the old method of coating a probe with nitrate of silver and then thrusting it to the bottom of the sinus. In the latter case some of the nitrate of silver gets rubbed off, in its passage, by the walls of the sinus.

Fistula in ano can also be treated by electrolysis. A wire is inserted along the entire track of the fistula and connected with the positive pole of an ordinary galvanic battery, the negative pole being placed on some indifferent part of the body. The circuit is then closed, and the lining membrane of the fistula destroyed by chemical decomposition. If electricity is employed for this affection the galvano-cautery wire is perhaps to be preferred. It is passed through the fistula into the rectum and then brought down by the finger and the two ends are attached to handles connected with either pole of a galvano-cautery battery and the wire allowed to burn its way through the intervening tissue, so that the fistula is opened into the bowel, as when the operation is performed by the knife.

Perineal, vesico-vaginal, and *salivary fistulæ* are probably better treated by galvano-cautery than by electrolysis. A double platinum wire at a red heat is thrust into them and

the edges of the fistula cauterized, but they can also be treated by electrolysis in the same way as anal fistulæ when no galvano-cautery battery is at hand.

Piles.

Electrolysis has been used for the destruction of piles. When the piles have been brought well into view, needles attached to the negative pole have been thrust into them and the positive pole has been placed on some indifferent part of the body. A weak current of ten to fifteen milliampères is allowed to flow for ten or fifteen minutes. But if electricity is used for piles the loop of wire with the galvano-cautery battery is preferable, and is perhaps the most efficient and satisfactory way of removing piles that we possess.

Wounds and Ulcers.

For bed-sores a silver plate connected with the negative pole of a battery is placed over the sore; the positive electrode is placed on some indifferent part of the body. A current of from two to four milliampères is allowed to pass for five or ten minutes every day. Unhealthy granulating wounds may be treated in the same manner. Electricity is very effectual in stimulating granulations. The applications should not be painful.

A very interesting account of the treatment of ulcers by the electrolytic property of the galvanic current is given in a letter from Mr. T. Spencer Wells, R.N. (now Sir Spencer) to Dr. Golding Bird, to be found as an appendix to the work "Electricity and Galvanism" written by the latter in 1849.

Granular Lids.

Granular lids have been treated successfully by electrolysis. The eyelid is everted and a small electrode connected with the negative pole is moved slowly over the unhealthy mucous membrane. The positive pole is placed at the nape of the neck. The electricity should be applied for several minutes and will require to be repeated six or eight times.

Hydatids.

Galvano-puncture has been used in the treatment of hydatid cysts. Several cases have been reported by Mr. Durham and the late Dr. Hilton Fagge, of Guy's Hospital. Needles attached to the negative pole were introduced into the tumors and the positive pole applied to the surface of the body. No suppuration is reported to have taken place and the cases progressed favorably. The good effects of the electricity in these cases has been questioned, as satisfactory results have followed simple puncture and aspiration of similar tumors.

Hydrocele.

Hydrocele has been treated in the same way, but needles attached to both poles have been passed into the sac, care being taken that they should not touch one another. By this means electrolysis of the fluid contained in the sac has been set up and said to have been followed by its absorption.

Dr. Rudolfi has reported several cases in which he first aspired the hydrocele, and then passed a probe connected with the negative pole into the sac, thus setting up adhesive inflammation by the liberation of an alkaline caustic. These operations have been followed by fairly successful results.

Exophthalmic Goitre.

Goitre has been treated by electricity in various ways, but with very uncertain results. Some cases have been quickly cured, but on the other hand cases have been known suddenly to improve without any treatment whatever. Lately on the continent and also by Duncan, of Edinburgh,[43] electrolysis has been tried. Needles attached to both poles of a galvanic battery have been thrust into the enlarged thyroid, and a current of 10 to 15 milliampères allowed to flow for four or five minutes. The galvano-puncture has been repeated several times.

The pathology of exophthalmic goitre is a most interesting one, and as yet we are only on the threshold of our knowledge regarding it. That there is some connection between this diseased condition and the sympathetic nervous system

is more than probable. The recent investigations into the nature of cretinism, myxœdema, and allied conditions, and the results of thyrodectomy, will doubtless throw much light upon the nature of goitre. Its probable relationship with the nervous system would seem to point it out as an affection most favorable for treatment by electricity; and probably it will ultimately prove to be so when we have been able to decide on some uniform plan of treatment, and have some reason and explanation for the plan we may adopt. Galvano-puncture, although it has proved successful in some cases, is purely empirical; and probably not performed without risk. The unfortunate contingency of sudden death occurring to patients suffering from exophthalmic goitre must always be borne in mind. What explanation there is for such deaths occurring is at present unknown, but should one happen during an operation by galvano-puncture the treatment by electricity would receive serious discouragement.

Treatment by electrolysis has been proposed and tried for numerous other effections, but with doubtful results.

The Advantages of Electrolysis.

Electrolysis is a force that can be used with great advantage in the practice of surgery. There is no destructive agent employed by the surgeon at the same time so potent and so entirely under his control. It can in most cases be used without the loss of blood. It is a more effectual and elegant way of applying caustic than any other we possess. The pain caused by the application ceases immediately the current is broken, and when employed in painful affections, the pain by its means is often greatly relieved. It encourages absorption, and produces healthy granulations when applied to ulcerated surfaces. The chemical decomposition, short of producing a slough, is itself antiseptic. It can be applied to narrow passages of the body, and to other parts difficult of access, in a way and in cases that it would be impossible to apply any other agent. It can be most accurately localized at the part it is wished to affect. The amount used, and the extent of the tissue to be destroyed, can be regulated to a nicety; and its action can be commenced and arrested at any moment at the will of the operator.

Prof. Ogston, of Aberdeen, has written,[44] "Many common mechanical instruments for studying and treating disease, such as the measuring line, the weighing machine, the concave reflecting mirror, and the speculum, are regularly employed by the majority of practitioners; while electricity, despite its value in the diagnosis and treatment of nervous diseases, as an unsurpassed illuminating agent, as an electrolytic destroyer of diseased tissue, tumors, and nævi, as the most manageable cauterizing agent, and as supplying the electro-magnet for extracting fragments of iron from the eyeball, is superseded for these purposes by less efficient means."

For the purely surgical applications of electricity a technical knowledge of the subject and an intimate acquaintance with the manner of using the different forms of batteries is most essential. I know, from the numbers of applications I have had from members of the profession asking for details of the operation for the electrolysis of fibroids, the best batteries to use, etc. etc., that an indiscriminate application of this mode of treatment is being made by men without the slightest previous knowledge of electricity. The use of the wrong pole in electrical treatment may be followed by most disastrous results, and the chances of the wrong pole being used are very numerous, sometimes even escaping the vigilance of the most experienced. The commutator of the battery may have been arranged wrongly by the instrument maker, the galvanometer may have been inserted on the wrong side of the circuit, the current may have to be arrested during an operation and the commutator reversed by accident, or by the negligence or ignorance of an assistant. One operation may have required the use of the positive pole and the next of the negative, and the necessary alteration in the battery have escaped notice. All these accidents have occurred within the knowledge of the author. I have seen part of the mucous membrane of the urethra torn off, and followed by profuse hemorrhage, by the use of the positive pole in the treatment of stricture. I know of a case where the patient suffered from such extreme collapse after a similar operation that the surgeon for a time was fearful as to the result. The collapse was produced either from the current being used too strong, or when strong, suddenly broken. I have been upbraided by a surgeon for advocating the treatment of stricture of the urethra by electrolysis, be-

cause in two cases he had set up most intractable cystitis by not cutting off the current after his electrode had passed the stricture, but by passing it on and electrolyzing the coats of the bladder; and yet, when properly performed, I believe electrolysis to be the best mode of treating stricture. I have seen a patient disfigured for life by the too energetic use of the wrong pole in treating a hairy mole on the face. In another case an abscess or slough was produced in the upper part of the pharynx, accompanied by the most intolerable earache, by the use of the wrong pole in an attempt to electrolyze the Eustachian tube. Most profound collapse has been caused by an assistant uncoupling one of the electrodes after the electrolysis of a uterine fibroid, and before the current had been gradually reduced in strength. Very painful and disagreeable shocks have been produced when treating the same affection by the battery having been sent home by the instrument maker with the zinc of one couple of elements placed in a wrong cell, and in another instance by the battery being sent home with the fluid omitted from one cell.

A case has been published where no doubt the woman's life was in jeopardy by the slouging of a tumor produced by the use of a too strong current. I mentioned in my paper read at Dublin the difficulties, though not serious, which followed the use of the wrong pole in the treatment of lachrymal obstruction. I could greatly extend this list of the dangers and difficulties connected with electrical treatment, but I have said enough to show that it should not be undertaken without a thorough knowledge of the apparatus to be used and the mode of using it.

BIBLIOGRAPHY.

1. Electricity, by John T. Sprague: London, E. and F. N. Spon.—2. Ganot's Physics, p. 751.—3. Electricity, and its Manner of Working in the Treatment of Disease, a thesis for the M.D. degree of the University of Cambridge, by W. E. Steavenson, pp. 24, 25: J. and A. Churchill, London, 1884.—4. See Sprague on Mixed Electrolytes, op. cit., p. 374.—5. Lumleian Lectures, Brit. Med. Journ., vol. i., 1886, pp. 728, 812, 863.—6. De Watteville: Medical Electricity, p. 25.—7. Electricity and Magnetism, by Silvanus P. Thompson, p. 163.—8. De Watteville's Medical Electricity, p. 31.—9. Ibid., p. 29.—10. Poore: Electricity in Medicine and Surgery, p. 247.—11. Lancet, June 11th, 1887, p. 1,182. 12. Such a needle has been made for the author by Messrs. Coxeter and

Son, of Grafton Street, W.C.—13. Lectures on the Surgical Treatment of Aneurism, by Timothy Holmes, M.A., F.R.C.S.; Lancet, Sept. 7th, 1872, p. 326.—14. Brit. Med. Journal, Nov. 3d, 1888, p. 985.—15. This article on Nævi appeared in the Provincial Medical Journal for Dec., 1888.—16. *Vide* British Medical Journal, November 14th, 1885, p. 943.—17. The Provincial Medical Journal for July, 1888.—18. By Dr. Newman: Treatment of Strictures of the Urethra by Galvanism, Medical Record, March, 1867; Electrolysis in the Treatment of Stricture of the Urethra, Transact. Med. Soc. State of N. Y., 1874; Ten Years' Experience in the Treatment of Stricture of the Urethra by Electrolysis, Med. Record, N. Y., August 12th and 19th, 1882; Answers to Correspondents about the Electrolytic Treatment of Urethral Strictures, with a few Selected Cases, New England Med. Monthly, June, 1883; Electrolysis in Surgery, Jour. Am. Med. Ass., April 25th, 1885; Tabulated Statistics of 100 cases of Urethral Strictures treated by Electrolysis without relapse, New Eng. Med. Mon., August, 1885; Progress of Electrolysis in Surgery, Gaillard's Med. Journ., December, 1885; Causes of Failure in the Treatment of Urethral Strictures by Electrolysis, Transactions N. Y. State Med. Ass., 1885; Is Electrolysis a Failure in the Treatment of Urethral Strictures? N. Y. Med. Record, September 25th, 1886; The Armamentarium for the Treatment of Urethral Strictures by Electrolysis, New England Med. Monthly, March, 1887; A Lecture on Electrolysis, New Eng. Med. Monthly, August, 1887; Synopsis of the Second 100 cases of Urethral Stricture treated by Electrolysis, with cases, Journ. Am. Med. Ass., September 24th and October 1st, 1887. By other Operators: David Price, M.D., Jacksonville, Ill., Transactions Ill. State Medical Association, April, 1873. J. F. Frank, M.D., Multiple Strictures of the Urethra Treated by Electrolysis, Med. Record, February 2d, 1874. John Butler, M.D., New York, Am. Journ. of Electrology, October, 1879, and Three Cases of Stricture of the Urethra cured by Electrolysis after all other methods had failed, N. Y. Med. Times, November, 1882. J. H. Glass, M.D., of Utica, N. Y., Report of Nine Cases, N. Y. Med. Record, May 12th, 1883. W. H. Dickerman, M.D., of Olean, N. Y., Impermeable Strictures successfully Treated, N. Y. Med. Record, June 25th, 1883. T. Belfield, M.D., Chicago, Nine Cases of Impermeable Urethral Strictures, Journ. Am. Med. Ass., April 24th, 1886. W. E. Steavenson, M.D., and W. Bruce Clarke, F.R.C.S., London, Eng., Lond. Med. Record, May 25th, 1886. T. J. Hayes, M.D., Report to the Academy of Ireland, British Med. Journ., July 17th, 1886. Geo. C. Pitzer, M.D., of St. Louis, in his work on Electricity in Medicine and Surgery, St. Louis, 1884. C. A. Bryce, M.D., Richmond, Va., reports successful cases in Southern Clinic, July, 1886. W. E. Steavenson, M.D., London, England, The Employment of Electricity in the Treatment of Diseases of the Urinary Organs, British Med. Journal, November 27th, 1886. Robert Bartholow, M.D., Philadelphia, Penn., Lecture, December 18th, 1882. W. F. Hutchinson, M.D., of Providence, R. I., Urethral Electrolysis, New Eng. Med. Monthly, December, 1886. G. W. Overall, M.D., Memphis, Tenn., Miss. Valley Med. Monthly. W. T. Belfield, M.D.,

reports thirty-seven cases in Med. Age. L. Wolff, M.D., Philadelphia, Penn., N. Y. Medizinische Presse, 1887. W. Bruce Clarke, F.R.C.S., The Treatment of Stricture of the Urethra by Electrolysis. Edwin Morton, M.D., London, Treatment of Stricture, British Med. Journ., October 1st, 1887. And recently the paper by F. Swinford Edwards, Electrolysis in the Treatment of Resilient or Non-Dilatable Stricture of the Urethra, Med. Press and Circular, April 11th, 1888. And numerous other reported cases of successful treatment by various medical men. 19. This account of the treatment of stricture of the rectum by electrolysis appeared in Mr. Alfred Cooper's work on Diseases of the Rectum, H. K. Lewis, London, 1887.—20. The section appeared in an article by the author and Mr. W. H. Jessop on Electrolysis in the Treatment of Lachrymal Obstruction, published in the Lancet, Dec. 24th, 1887, p. 1,371.—21. Published in the Lancet, by Mr. A. E. Cumberbatch and the author, Nov. 24th, 1888, p. 1,014.—22. Provincial Medical Journal, May, 1888, p. 204.—23. Diseases of the Urinary Organs, 8th ed., p. 364.—24. Lancet, January 16th, 1888, p. 101.—25. Lancet, January 16th, 1888, p. 101. —26. A large portion of the matter contained in this section was written for a paper On the Use of Electrolysis in Gynæcological Practice, read before the Obstetrical Society of London, June 6th, 1888. *Vide* Obstetrical Society's Transactions, vol. xxx., p. 229.—27. Lancet, Sept. 18th, 1886, p. 559.—28. This section on the Treatment of Uterine Fibroids by Electrolysis was published in advance in the autumn of 1887.—29. See Electricity and its Manner of Working in the Treatment of Disease, a thesis for the degree of M.D., by W. E. Steavenson, pp. 24 and 25: J. & A. Churchill, London, 1884.—30. Dr. Apostoli's paper read at Dublin, Aug., 1887; Brit. Med. Journ., Oct. 1st, 1887.—31. The Value of Electrolysis in the Treatment of Ovarian Tumors, by Paul F. Mundé, M.D., New York; Gynæcological Trans., 1878.—32. Medical and Surgical Uses of Electricity, by Drs. Beard and Rockwell, 3d. ed., 1881, p. 668; Wm. Wood & Co., New York.—33. St. Barth. Hosp. Reports, vol. xix., p. 40. —34. *Vide* Brit. Med. Journal, April 27th, 1889, p. 937.—35. Obstetrical Transactions, vol. xxx., p. 291.—36. Pp. 292, 293.—37. Electricity, by H. M. Noad, revised by W. H. Preece, 1879, pp. 235, 236.—38. *Vide* Medical Electricity, by Dr. Althaus, p. 650.—39. *Vide* Massey on Electricity in the Diseases of Women, 1889, p. 186 et seq.; and Bartholow, Medical Electricity, Edinburgh, 1887, p. 240 et seq., and p. 266.—40. Brit. Med. Journal, Dec. 4th, 1886, p. 1,092.—41. St. Barth. Hosp. Reports, vol. xix., p. 44.—42. Brit. Med. Journal, April 30th, 1887, p. 925.—*Vide* Brit. Med. Journal, Nov. 3d, 1888, p. 986.—44. Lancet, April 30th, 1887, p. 67. The italics are by the author.

INDEX TO VOL. V.

ABDOMINAL gestation, 529
section during puerperal period, 554
section for ectopic gestation, 551
section for primary and secondary rupture, 555
Abrasion of the os uteri, treatment of, by electrolysis, 867
Acid, uric, see Uric
Acids, effects of, on uric acid in the blood, 309
Age as a factor in the prognosis of heart disease, 65
Albumin in the sputum, 129
Albuminuria, uric acid in the causation of, 357
Alkalies, effects of, on uric acid in the blood, 311
Amnesia, 188
Ampère, definition of, 808
Angina pectoris, 663
pectoris, prognosis of, 69
Antipyrin, effect of, on uric acid excretion, 325
Antiseptic value, comparative, of various chemical substances, 458
Aneurism, treatment of, by electrolysis, 823
Arcus senilis, 643
Aspergillus in the sputum, 128
Asthma crystals in the sputum, 113
senile, 663
uric acid in the causation of, 359
Atheroma, cause of, 633
Atony of the urethral sphincter, 687
Auto-suggestion, 183

BACILLI, battle with cells, 405
in milk, 375
of Koch in the sputum, 118
sustenance of, 403
Bacillus, tubercle, bearing of the discovery of, on consumption, 368
tubercle, effects of, in tuberculization, 378
Bacteriology of snow, 456
Bandages in the treatment of varicose veins, 773
Basedow's disease, prognosis of, 70
Baths in the treatment of varicose veins, 769
Batteries, medical, 811
Bennett, William H., on varicose veins of the lower extremities, 783
Bladder, discharge of suppurating sac into, 527
senile, 641, 662, 663
spasm of the, in infancy, 679
Blood-corpuscles in the sputum, 107
Bones, senile, 643
Brain, senile, 644, 655, 663
softening of the, 656

CALCULI, pulmonary, 131
Calculus, vesical, treatment of, by electrolysis, 857
Cancer, arrest of growth in, by the interrupted voltaic current, 589
electrical treatment, cases, 597
electrical treatment, modus operandi, 595
nature of, 591
treatment of, by electrolysis, 882
Cases of ectopic gestation operated upon, 512
Cell proliferation proportionate to amount of irritant, 380
Cellulitis, pelvic, treatment of, by electrolysis, 881
Cerebral congestion, senile, 663
hemorrhage, senile, 663

Cervix uteri, hypertrophy of the, treatment of, by electrolysis, 880
Childhood, urinary neuroses of, 669
Cholesterin in the sputum, 112
Classification of urinary disorders, 671
Clay electrode, 870
Coagula, fibrinous, in the sputum, 109
Colchicum, effect of, on uric acid excretion, 327
Conception, normal seat of, 464
Consciousness and suggestion, 208
Constitutional decline and the domiciliation of the bacillus, 415
Consumption, bearing of the discovery of the tubercle bacillus on, 368
the initial stages of, 367
Crystalline formations in the sputum, 111
Current, constant, effects of, 593
interrupted voltaic, arrest of the growth of cancer by, 589
Cystospasm, 679
Cysts, ovarian, treatment of, by electrolysis, 881

Decline, constitutional, and the domiciliation of the bacillus, 415
Dementia, senile, 656
Diabetes, uric acid in the causation of, 358
Diet, essentials of a normal, 451
for consumptives, 454
tables, 451
Digestive organs, senile, 639
Dobell, Horace, on the initial stages of consumption, etc., 365
Dyspepsia, relation to headache, 331
Dysmenorrhœa, treatment of, by electrolysis, 865

Ectopic gestation, cause of, 464
gestation, clinical history of, 465
pregnancy, 463
pregnancy, see also Gestation
Electricity in the treatment of varicose veins, 769
Electro-atrophy, 589
Electro-chemical series, 796
Electrolysis and galvano-cautery, difference between, 806
definition of, 796
in surgery, the uses of, 795
in tubal gestation, 522, 889
primary and secondary, 805
advantages of, 894
Electrolysis, dangers of, 895
Electro-motive force, 807
Electrode, clay, 870
Electrodes, intra-uterine, 872
Emphysema, senile, 638
Endometritis, treatment of, by electrolysis 881
Enteralgia, 54
Entozoa in the sputum, 116
Epilepsy, relation to uric-acid headache, 340
Epithelium in the sputum, 105
Eustachian tube, obstruction of the, treatment of, by electrolysis, 853
Exercise in the treatment of varicose veins, 768
Exploratory incision, principle of, 487
Extra-uterine fœtation, treatment of, by electrolysis, 522, 889

Fallacies, sources of, in experiments, 380
Fallopian tube, 465
Fats in the sputum, 130
Fatty-acid needles in the sputum, 111
Fibres, elastic, in the sputum, 108
Fibrinous coagula in the sputum, 109
Fibroids, uterine, treatment of, by electrolysis, 868
Fistulæ, treatment of, by electrolysis, 891
Fœtus, absorption of, 505
Fœtation, extra-uterine, treatment of, by electrolysis, 522, 889
Food remnants in the sputum, 117
Forel, August, on hypnotism, 159
Fungoid growths, treatment of, by electrolysis, 880

Gall-Bladder, senile, 639
Galvanometer, 816
Gangrene, senile, 655, 662
Gastralgia, 54
Gastritis, senile, 654, 661
Gastrodynia, 54
Gastro-intestinal canal, senile, 639
Genital organs, senile, 642
Gestation, abdominal, 529
abdominal, T. R. Jessop's case, 530
ectopic, illustrative cases, 487
ectopic, primary and secondary rupture, 517
ectopic, scheme of, 469
extra-uterine, treatment of, by electrolysis, 522, 889
hernial, 469

Gestation, interstitial, 466
interstitial, case of, 515
interstitial, rupture of, 467
ovarian, 469
ovarian, Dr. Walter's specimen, 473
ovarian, Granville's case, 474
ovarian, its possibility, 474
ovarian, M. Puech's case, 472
ovarian, Spiegelberg's case, 471
sous-peritoneo-pelvienne variety, 524
tubal, diagnosis of, 467
tubal, Dr. Dolan's case, 484
tubal, Mr. Hallwright's case, 483
tubal, multiple, 577
tubal, pathology of, 465
tubal, retention of fœtus, 577
tubal, rupture of, 482
tubal, suppuration in ruptured sac, 542
tubal, unruptured, symptoms of, 481
tubo-ovarian, 475
Giant cells, relation to bacilli, 376
Gleet, treatment of, by electrolysis, 856
Glycosuria, uric acid in the causation of, 358
Goitre, exophthalmic, treatment of, by electrolysis, 893
Gout, uric acid in the causation of 346
Gowers, W. R., on neuralgia; its etiology, diagnosis and treatment, 1
Guinon, Louis-J., on urinary neuroses of childhood, 667

Habitat, laws of, 399
Hæmatocele, definition of, 492
extra-peritoneal, 496
extra-peritoneal, causes of, 499
extra-peritoneal, suppuration of, 502
intra-peritoneal, 503
intra-peritoneal, causes of, 504
intra-peritoneal, exanthematic, 507
intra-peritoneal, prognosis, 508
intra-peritoneal, treatment, 508
of broad ligament, 523
pelvic, 489
Hæmoptysis, uric acid in the causation of, 359
Haig, A., on the formation and excretion of uric acid, as elucidating its action in the causation of disease, 303
Hair, superfluous, removal of, by electrolysis, 838
Hallucination, retro-active, 198
Hart and Carter, researches on ectopic pregnancy, 563–569
Headache, dyspepsia in relation to, 331
uric acid in the causation of, 329
uric-acid, relation of epilepsy to, 340
uric-acid, relation of mental depression to, 335
uric-acid, relation of pulse tension to, 332
Heart, dilatation of the, prognosis of, 71
diseases of the, prognosis of, 59
fatty, prognosis of, 76
functional diseases of the, prognosis of, 68
hypertrophy of the, prognosis of, 71
insufficiency of the, prognosis of, 74
muscle, diseases of the, prognosis of, 70
senile, 635, 653, 661
valvular lesions of the, prognosis of, 77
weak, prognosis of, 74
Hemorrhoids, treatment of, by electrolysis, 892
Hepatalgia, 55
Heredity in the etiology of nocturnal incontinence, 687
Herman's conclusions in re vaginal section, 560
Hydatids, treatment of, by electrolysis, 892
Hydrocele, treatment of, by electrolysis, 892
Hygiene of the aged, 657
Hypnosis, amnesia in, 188
and sleep, 175
degree of, 178
emotional effects of, 182
mental processes influenced by, 182
phenomena of, 180
phenomena of, motor, 180
phenomena of, sensory, 181
reflexes in, 182
simulation and dissimulation of, 201
Hypertrophy of the cervix uteri, treatment of, by electrolysis, 880
Hypnotic practice, hints for, 213
Hypnotism, 161
terminology of, 170

Hypnotism, theories and conceptions of, 162
Hypnotizability, 172
Hypnotized, resistance of the, 183
training of the, 179

ICTERUS, senile, 654, 662
Impregnation. physiology of, 465
theories of, 469
Incontinence, nocturnal, of urine, 682
Infancy, urinary physiology of, 672
Insomnia, senile, 663
Interval, eventful, between inoculation and tuberculization, 379
Invalid soup, 455
Iodide of potassium, effect of, on uric-acid excretion, 327
Iron, effect of, on uric-acid excretion, 323
Irritability, vesical, in children, 685
Irritants other than bacilli causing tuberculization, 386

JOINTS, senile, 644

KAATZER, PETER, on the sputum, 95
Keloid scars, treatment of, by electrolysis, 841
Kidneys, senile, 640
Koumiss, 456

LACHRYMAL obstruction, treatment of, by electrolysis, 849
Lead, effect of, on uric acid excretion, 323
Leprosy, causation obscure, 614
contagiousness of, 620
history of, 608
mode of dealing with, 624
spread of, 610
strongholds of, 609
the dreadful revival of, 605
transmission of, 616
Leptothrix in the sputum, 126
Leyden, E., on the prognosis of diseases of the heart, 57
Lids, granular, treatment of, by electrolysis, 892
Lithium, effect, on uric-acid excretion, 323
Lithopædion, cases of, 578–585
quiescent, 585
Liver, senile, 639
Lupoid ulceration, removal of, by electrolysis, 841
Lymphatic glands, senile, 640

MACDONALD, GREVILLE, on the forms of nasal obstruction in relation to throat and ear diseases, 237
Mackenzie, Sir Morell, on the dreadful revival of leprosy, 603
Marasmus, senile, 632, 654
Margarin crystals in the sputum, 111
Massage in the treatment of varicose veins, 768
Memory, loss of, 188
suggested deception of the, 198
Menorrhagia, treatment of, by electrolysis, 865
Mental depression, relation to uric-acid headache, 335
Metritis, treatment of, by electrolysis, 881
Micro-organisms in the sputum, 117
Micturition, frequency of, 675
quantity of urine passed during, 675
voluntary, 675
Migraine, uric acid in the causation of, 305
Milk, bacilli in, 375
Moles, treatment of, by electrolysis, 837, 841
Mucin in the sputum, 130
Mucous and salivary bodies in the sputum, 104

NÆVI, treatment of, by electrolysis, 830
Nasal duct, closure of the, treatment of, by electrolysis, 852
obstruction from mucous polypi and cysts, 271
obstruction from post-nasal growths, 283
obstruction, symptoms of, 244
obstruction, the forms of, 239
Needles for electrolysis, 832, 833
Nephralgia, 55
Nerve stretching, 49
Nerves, senile, 646
Neuralgia, 3
anæmic, 31
brachial, 22
cervico-brachial, 22
cervico-occipital, 21
definition, 3
degenerative, 33
diabetic, 31
diagnosis, 33
dorso-intercostal, 23
epileptiform, 26
etiology, 4
forms, 17

Neuralgia, gouty, 31
herpetic, 29
hysterical, 30
infra-mammary, 23
intercostal, 23
lumbo-abdominal, 24
malarial, 32
occupation, 29
ocular, 19
of the fifth nerve, 18
of the leg, 25
pathology, 11
prognosis, 39
reflex, 26
rheumatic, 31
spinal, 24
sympathetic, 26
syphilitic, 33
traumatic, 27
treatment, 40
trifacial, 18
trigeminal, 18
trunk, 23
visceral, 51
Neuroses, urinary, of childhood, 669
Neurotomy, 48
Nocturnal incontinence of urine, 682
Nutritive, special, 455

ŒSOPHAGUS, treatment of, by electrolysis, 849
Ohm, definition of, 807
Ohm's law, 808
Old age, diseases of, 631
Opium and morphia, effect of, on uric-acid excretion, 326
Ovarian cysts, treatment of, by electrolysis, 881
Ovaries, senile, 642

PANCREAS, senile, 640
Parsons, J. Inglis, on the arrest of growth in cancer by the interrupted voltaic current (electro-atrophy), 587
Peritonitis after ruptured tubal pregnancy, 524
Phosphate of sodium, effect of, on uric-acid excretion, 327
Phthisis, uric acid in the causation of, 359
Physiology, urinary, of infancy, 672
Piles, treatment of, by electrolysis, 892
Placenta, extrusion of, in ruptured tube, 476
growth after death of fœtus, 547
Placenta, secondary attachments to viscera, 477
treatment of, after operation, 574
Pleurodynia, 23
Pneumonia, senile, 650, 660
Pneumonococci in the sputum, 122
Polarization of batteries, 809
Pollakiuria, 680
Polyuria, nervous, of childhood, 700, 725
simple, of childhood, 700, 727
simple, heredity in, 706, 728
simple, pathological anatomy of, 719
simple, pathological physiology of, 719
simple, prognosis of, 718
simple, symptoms of, 711
simple, treatment of, 721
symptomatic, 729
traumatic, 722
Portals by which bacilli may enter the organism, 383
Port-wine marks, treatment of, by electrolysis, 837
Post-hypnotic phenomena, 185
suggestion, condition of the mind during, 192
Pregnancy, bifid uterus, 535
ectopic, 463
ectopic, see also Gestation
extra-uterine, treatment of, by electrolysis, 522, 889
Prosopalgia, 18
Prostate, senile, 641, 662
enlarged, treatment of, by electrolysis, 857
Pruritus, senile, 662
Pulse tension, relation to uric-acid headache, 332
Pus-corpuscles in the sputum, 104
Pyrosis senilis, 654, 661

RECTUM, discharge of suppurating sac through, 527
stricture of the, treatment of, by electrolysis, 847
Relation of uterus to gestation sac, 539
Resistance of human body, measurement of, 818
Respiratory organs, senile, 638
Rheostats, 810
Rheumatism, uric acid in the causation of, 352
Rhinitis, chronic catarrhal, 253
dry atrophic, 260
dry hyperplastic, 263
Rhinoscopy, anterior and posterior, 240

SACCHAROMYCES in the sputum, 127
Salicylates, effect of, on uric-acid excretion, 320
Salivary glands, senile, 639
Sciatica, 25
Sclerosis, 634
Sebaceous cysts, removals of, by electrolysis, 841
Seidel, A., on the diseases of old age, 629
Sex as a factor in the prognosis of heart disease, 65
Skin, senile, 642, 655
Sleep and hypnosis, 175
Snow, bacteriology of, 456
Soup, invalid, 455
Spasm of the bladder in infancy, 679
Sphincter, urethral, atony of the, 687
Spinal cord, senile, 645, 656, 663
Spirals in the sputum, 115
Spleen, senile, 640
Sputum, bloody, 134
  chemistry of the, 129
  constituents of the, 104
  diagnostic significance of the, 97
  disinfection of the, 148
  examination of the, general technique, 102
  in bacillary phthisis, 137
  in pneumonia, 140
  in pulmonary gangrene, 145
  methods of examination for tubercle bacilli, 151
  muco-purulent, 133
  mucous, 132
  purely purulent, 134
  purulent-mucoid, 133
  reagents used in the examination of the, 103
Staining methods of sputum, 118, 152
Steavenson, W. E., on the uses of electrolysis in surgery, 793
Sterility, treatment of, by electrolysis, 865
Stockings, etc., in the treatment of varicose veins, 775
Stomach, dilatation of the, senile, 654
Strapping in the treatment of varicose veins, 770
Strictures, treatment of, by electrolysis, 842
Sugar in the sputum, 129
Suggestion, 172
  and consciousness, 208
  and mental disturbances, 107
Suggestion for a definite period, 189
  illustration of cures by, 220
  judicial significance of, 223
  permanent results of, 197
  significance of, 203
Suggestive action, nature of, 205
Suppurating fœtal cyst, abdominal section, 528

TAIT, LAWSON, on ectopic pregnancy and pelvic hæmatocele, 461
Tapping, dangers of, 540
Testicle, senile, 642
Thorax, senile, 638
Tic douloureux, 18
Treatment of ectopic gestation, 543
  of ectopic gestation by electricity, 545
Trichiasis, treatment of, by electrolysis, 841
Trusses, pads, etc., in the treatment of varicose veins, 771
Tubercle bacillus, bearing of the discovery of, on consumption, 368
  bacillus, effects of, in tuberculization, 378
Tuberculization, experimental, 378
Tuberculosis and milk, 431
  bacillary, as a local and general disease, 402
  climatic treatment, 435
  dietetic treatment, 437
  exercise in the treatment of, 446
  medicinal treatment, 438
  not to be confined to the lungs, 389
  of bones and joints, 390
  of the intestines, 390
  regimen in the treatment of, 441
  treatment of the initial stage, 429
Tumors, removal of, by electrolysis, 841
Turbinated bones, hypertrophy of the, treatment of, by electrolysis, 841
Tyrosin in the sputum, 112

ULCERS, treatment of, by electrolysis, 892
Umbilicus, discharge of suppurating sac through, 527
Urea, proportion to uric acid, 318
Ureters, senile, 641

Urethra, stricture of the, treatment of, by electrolysis, 842, 864
Urethral sphincter, atony of the, 687
Uric acid in the blood, effects of acids on, 309
acid in the blood, effects of alkalies on, 311
acid in the causation of albuminuria, 357
acid in the causation of asthma, 359
acid in the causation of diabetes, 358
acid in the causation of disease, 305
acid in the causation of glycosuria, 358
acid in the causation of hæmoptysis, 359
acid in the causation of headache, 329
acid in the causation of gout, 346
acid in the causation of migraine, 305
acid in the causation of phthisis, 359
acid in the causation of rheumatism, 352
acid, proportion to urea, 318
Urinary disorders, classification of, 671
neuroses of childhood, 669
organs, senile, 640
physiology of infancy, 672
Urine, nocturnal incontinence of, 682
of childhood, properties of, 676
of childhood, sodium chloride in, 679
of childhood, specific gravity of, 676
of childhood, urea in, 677
of childhood, uric acid in, 678
quantity excreted by children, 674
quantity excreted by infants, 672
Uterine fibroids, treatment of, by electrolysis, 868
walls, abnormal thinness of, 534
Uterus, senile, 642

VAGINA, discharge of suppurating contents through, 527
Vaginal section for ectopic pregnancy, 557
Valves of veins of the lower extremities, 757
Varicocele, treatment of, by electrolysis, 829
Varicose veins, causes, probable, 741
veins, complications of, 744
veins, diagnosis of incipient, 750
veins of the lower extremities, 735
veins, relative frequency of, in the two sexes, 736
veins, time of appearance, 744
veins, treatment of, 762
veins, treatment of, by electrolysis, 829
veins, treatment of, operative, 780
Varix at the saphenous opening, 755
Vesical irritability in children, 685
calculus, treatment of, by electrolysis, 857
Virus, symmetrical and unsymmetrical effects of, 381
Volt, definition of, 807
Voltaic current, interrupted, arrest of the growth of cancer by, 589
Voltameter, 819

WARTS, treatment of, by electrolysis, 841
Water in sputum, 129
Wounds, treatment of, by electrolysis, 892

PUBLISHED MONTHLY

$10.00 A YEAR SINGLE COPIES $1.00

OOD'S MEDICAL AND SURGICAL MONOGRAPHS

NEURALGIA; ITS ETIOLOGY, DIAGNOSIS, AND TREATMENT
BY W. R. GOWERS, M.D., F.R.C.S.

THE PROGNOSIS OF DISEASES OF THE HEART
BY PROF. E. LEYDEN, BERLIN.

THE SPUTUM. A CONTRIBUTION TO CLINICAL DIAGNOSIS AND PRACTICAL EXAMINATION FOR TUBERCLE BACILLI
BY PETER KAATZER, M.D.

HYPNOTISM. ITS SIGNIFICANCE AND MANAGEMENT BRIEFLY PRESENTED
BY DR. AUGUST FOREL.

THE FORMS OF NASAL OBSTRUCTION, IN RELATION TO THROAT AND EAR DISEASES
BY GREVILLE MACDONALD, M.D.

LIAM WOOD AND COMPANY PUBLISHERS
6 & 58 LAFAYETTE PLACE NEW YORK

VOLUME 5 NUMBER 1

JANUARY 1890

No. 1

BOSTON
MEDICAL LIBRARY
ASSOCIATION,
19 BOYLSTON PLACE,

Received

By Gift of

THE LUCI-
ng.
THE AND
dical
gical
phan
Phy-
hest,

ECT M.D.
Uni-
Sur-
een's
and

Midland Hospital for Women; Honorary Consulting Surgeon to the Brooklyn Hospital for Women, to the Nottingham Samaritan Hospital for Women, to the Wolverhampton Dispensary for Women, and to the West Bromwich District Hospital, etc.; Honorary Fellow [late President] British Gynæcological Society; Honorary Fellow, American Gynæcological Society; President of Birmingham and Midland Counties Branch British Medical Association, etc., etc.

MEDICAL JOURNALS

PUBLISHED BY

WILLIAM WOOD & COMPANY

56 & 58 LAFAYETTE PLACE, NEW YORK.

THE MEDICAL RECORD,

A WEEKLY JOURNAL OF

Medicine and Surgery.

$5.00 A YEAR.

THE AMERICAN JOURNAL OF OBSTETRICS.

PUBLISHED MONTHLY.

$5.00 A YEAR.

WOOD'S MEDICAL AND SURGICAL MONOGRAPHS.

PUBLISHED MONTHLY.

$10.00 A YEAR.

AMERICAN DRUGGIST,

An Illustrated Monthly Journal of Pharmacy, Chemistry and Materia Medica.

$1.50 A YEAR.

Club Discounts for Advanced Payments.

When several of the Journals are subscribed for, to be mailed to the SAME ADDRESS, *the following "Club Rates" are offered:*

Medical Record, Am. Jour. of Obstetrics,	$10.00, Club Discount,	$1.00.
Medical Record, M. & S. Monographs,	15.00, Club Discount,	2.50.
Am. Jour. of Obstetrics, M. & S. Monographs,	15.00, Club Discount,	2.50.
Medical Record, Am. Jour. of Obstetrics, M. & S. Monographs,	20.00, Club Discount,	5.00.

The Prices, Clubbing Rates, and Terms of Subscription of the Journals are PRECISELY AS PRINTED. No VERBAL agreement whatever with a solicitor will be accepted as modifying an order.

Notice of CHANGES and Orders for DISCONTINUANCE must be addressed to William Wood & Company, and in order to discontinue a subscription all arrears must be paid and the account closed up. Parties accepting Journals from the Post Office are considered subscribers, and are expected to pay for same.

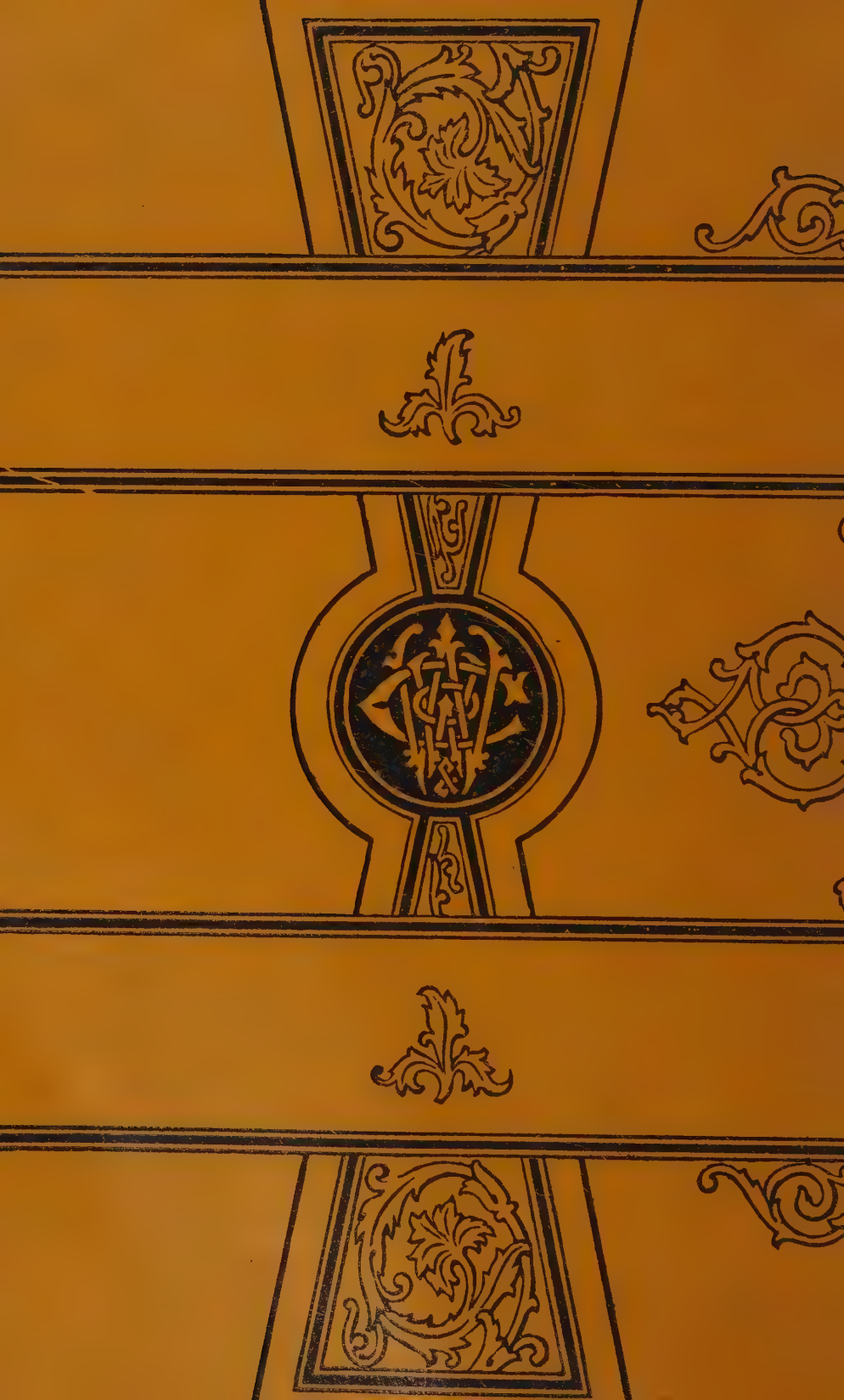
The Postal Laws especially forbid Postmasters taking orders for stoppage, or changes of address for Periodicals, at request of Subscribers.

Journal bills when sent are made at full prices. *The Club discount for the current Journal year can be deducted* from the amount of the bill if payment is made according to the terms of subscription.

There are five ways by which money can be sent at our own risk—in a Post Office Money Order, Draft, Check, or an Express Money Order, and when none of these can be procured, in a Registered Letter. All Postmasters are required to register letters whenever requested to do so.

If you send your money in any other way, you must do it at your own risk.

POSTAL NOTES are not a safe way to send money. Any one can get the money on them, and, if lost or stolen, the money cannot be recovered, as no duplicates are issued. Subscribers who send us Postal Notes do so at their own risk.



PUBLISHED MONTHLY

$10.00 A YEAR SINGLE COPIES $1.00

OOD'S MEDICAL AND SURGICAL MONOGRAPHS

CONTENTS

ACTION OF URIC ACID IN THE CAUSATION OF DISEASE
BY A. HAIG, M.D., LONDON.

INITIAL STAGES OF CONSUMPTION, THEIR NATURE AND TREATMENT
BY HORACE DOBELL, M.D., LONDON.

ECTOPIC PREGNANCY AND PELVIC HÆMATOCELE
BY LAWSON TAIT, M.D.

LLIAM WOOD AND COMPANY PUBLISHERS
6 & 58 LAFAYETTE PLACE NEW YORK

VOLUME 5 NUMBER 2

FEBRUARY 1890

CONTENTS OF THE NEXT ISSUE.

TREATMENT OF CANCER BY ELECTRICITY.
By J. INGLIS PARSONS, M.D., Assistant Physician to Chelsea Hospital for Women, London.

THE DREADFUL REVIVAL OF LEPROSY.
By SIR MORELL MACKENZIE, M.D., M.B., M.R.C.S.

DISEASES OF OLD AGE.
By Dr. A. SEIDEL, Berlin.

URINARY NEUROSES OF CHILDHOOD.
By Dr. LOUIS J. GUINOU, Paris.

VARICOSE VEINS OF THE LOWER EXTREMITIES.
By WILLIAM H. BENNETT, F.R.C.S., Surgeon to St. George's Hospital; Lecturer on Clinical Surgery and Anatomy in St. George's Hospital Medical School; Member of the Board of Examiners, Royal College of Surgeons of England. [Lithographic plate.]

THE USES OF ELECTROLYSIS IN SURGERY.
By W. E. STEVENSON, M.D. Cantab., M.R.C.P. In charge of the Electrical Department of St. Bartholomew's Hospital; Physician to the Grosvenor Hospital for Women and Children; Late House Surgeon to St. Bartholomew's Hospital, and to the Hospital for Sick Children, Great Ormond St., London. [Illustrated with numerous wood engravings.]

MEDICAL JOURNALS

PUBLISHED BY

WILLIAM WOOD & COMPANY

56 & 58 LAFAYETTE PLACE, NEW YORK.

THE MEDICAL RECORD,

A WEEKLY JOURNAL OF

Medicine and Surgery.

$5.00 A YEAR.

WOOD'S

MEDICAL AND SURGICAL MONOGRAPHS.

PUBLISHED MONTHLY.

$10.00 A YEAR.

THE AMERICAN JOURNAL OF OBSTETRICS.

PUBLISHED MONTHLY.

$5.00 A YEAR.

AMERICAN DRUGGIST,

An Illustrated Monthly Journal of Pharmacy, Chemistry and Materia Medica.

$1.50 A YEAR.

Club Discounts for Advanced Payments.

When several of the Journals are subscribed for, to be mailed to the SAME ADDRESS, *the following "Club Rates" are offered:*

Medical Record, Am. Jour. of Obstetrics,	$10.00, Club Discount,	$1.00.
Medical Record, M. & S. Monographs,	15.00, Club Discount,	2.50.
Am. Jour. of Obstetrics, M. & S. Monographs,	15.00, Club Discount,	2.50.
Medical Record, Am. Jour. of Obstetrics, M. & S. Monographs,	20.00, Club Discount,	5.00.

The Prices, Clubbing Rates, and Terms of Subscription of the Journals are PRECISELY AS PRINTED. No VERBAL agreement whatever with a solicitor will be accepted as modifying an order.

Notice of CHANGES and Orders for DISCONTINUANCE must be addressed to William Wood & Company, and in order to discontinue a subscription all arrears must be paid and the account closed up. Parties accepting Journals from the Post Office are considered subscribers, and are expected to pay for same.

The Postal Laws especially forbid Postmasters taking orders for stoppage, or changes of address for Periodicals, at request of Subscribers.

Journal bills when sent are made at full prices. *The Club discount for the current Journal year can be deducted* from the amount of the bill if payment is made according to the terms of subscription.

There are five ways by which money can be sent at our own risk—in a Post Office Money Order, Draft, Check, or an Express Money Order, and when none of these can be procured, in a Registered Letter. All Postmasters are required to register letters whenever requested to do so.

If you send your money in any other way, you must do it at your own risk.

POSTAL NOTES are not a safe way to send money. Any one can get the money on them, and, if lost or stolen, the money cannot be recovered, as no duplicates are issued. Subscribers who send us Postal Notes do so at their own risk.

PUBLISHED MONTHLY

$10.00 A YEAR SINGLE COPIES $1.00

OOD'S MEDICAL AND SURGICAL MONOGRAPHS

TREATMENT OF CANCER BY ELECTRICITY
BY DR. J. INGLIS PARSONS, LONDON

THE DREADFUL REVIVAL OF LEPROSY
BY SIR MORELL MACKENZIE, M.D.

DISEASES OF OLD AGE
BY DR. A. SEIDEL, BERLIN

URINARY NEUROSES OF CHILDHOOD
BY DR. LOUIS-J. GUINON, PARIS

VARICOSE VEINS OF THE LOWER EXTREMITIES
BY WILLIAM H. BENNETT, F.R.C.S.

USES OF ELECTROLYSIS IN SURGERY
BY W. E. STEAVENSON, M.D., LONDON

LIAM WOOD AND COMPANY PUBLISHERS
6 & 58 LAFAYETTE PLACE NEW YORK

VOLUME 5 NUMBER 3

MARCH 1890

CONTENTS OF THE NEXT ISSUE.

THE HUMAN FOOT; ITS FORM AND STRUCTURE, FUNCTIONS AND CLOTHING.

By THOMAS S. ELLIS, Consulting Surgeon to the General Infirmary at Gloucester, England. [Illustrated with many wood engravings, and eight colored lithographic plates.]

MODERN CREMATION; ITS HISTORY AND PRACTICE.

By SIR H. THOMPSON, F.R.C.S., M.B., London. President of the Cremation Society of England. [Illustrated.]

ON APHASIA; BEING A CONTRIBUTION TO THE SUBJECT OF THE DISSOLUTION OF SPEECH FROM CEREBRAL DISEASE.

By JAMES ROSS, M.D., LL.D., Aberdeen; Fellow of the Royal College of Physicians of London, and Senior Assistant Physician to the Manchester Royal Infirmary. [Illustrated with wood engravings.]

MEDICAL JOURNALS

PUBLISHED BY

WILLIAM WOOD & COMPANY

56 & 58 LAFAYETTE PLACE, NEW YORK.

THE MEDICAL RECORD,

A WEEKLY JOURNAL OF

Medicine and Surgery.

$5.00 A YEAR.

WOOD'S

MEDICAL AND SURGICAL MONOGRAPHS.

PUBLISHED MONTHLY.

$10.00 A YEAR.

THE AMERICAN JOURNAL OF OBSTETRICS.

PUBLISHED MONTHLY.

$5.00 A YEAR.

AMERICAN DRUGGIST,

An Illustrated Monthly Journal of Pharmacy, Chemistry and Materia Medica.

$1.50 A YEAR.

Club Discounts for Advanced Payments.

When several of the Journals are subscribed for, to be mailed to the SAME ADDRESS, *the following "Club Rates" are offered:*

Medical Record, Am. Jour. of Obstetrics,	$10.00, Club Discount,	$1.00.
Medical Record, M. & S. Monographs,	15.00, Club Discount,	2.50.
Am. Jour. of Obstetrics, M. & S. Monographs,	15.00, Club Discount,	2.50.
Medical Record, Am. Jour. of Obstetrics, M. & S. Monographs,	20.00, Club Discount,	5.00.

The Prices, Clubbing Rates, and Terms of Subscription of the Journals are PRECISELY AS PRINTED. No VERBAL agreement whatever with a solicitor will be accepted as modifying an order.

Notice of CHANGES and Orders for DISCONTINUANCE must be addressed to William Wood & Company, and in order to discontinue a subscription all arrears must be paid and the account closed up. Parties accepting Journals from the Post Office are considered subscribers, and are expected to pay for same.

The Postal Laws especially forbid Postmasters taking orders for stoppage, or changes of address for Periodicals, at request of Subscribers.

Journal bills when sent are made at full prices. *The Club discount for the current Journal year can be deducted* from the amount of the bill if payment is made according to the terms of subscription.

There are five ways by which money can be sent at our own risk—in a Post Office Money Order, Draft, Check, or an Express Money Order, and when none of these can be procured, in a Registered Letter. All Postmasters are required to register letters whenever requested to do so.

If you send your money in any other way, you must do it at your own risk.

POSTAL NOTES are not a safe way to send money. Any one can get the money on them, and, if lost or stolen, the money cannot be recovered, as no duplicates are issued. Subscribers who send us Postal Notes do so at their own risk.

APR 18 1903

NOV 12 1985

DATE DUE

	201-6503		Printed in USA